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1. ON the 2d September 1839, M. ARAGO communicated to the Academy of Sciences of Paris a letter by M. MELLONI, containing some very interesting experiments on the transmission of Radiant Heat. M. MELLONI finds that rock-salt (which is well known to transmit rays of heat from all sources yet tried with equal

* The substance of the present paper was communicated to the Royal Society of Edinburgh on the 16th December 1839, in the words of the memorandum which forms part of this Note. The memorandum itself was read, with some verbal explanation and citation of additional facts, on the 6th January. Every experiment to which reference is made in the present paper, was performed between the 12th November 1839 and the 4th March 1840. Since that time, I have not made a single experiment on the subject. Occupation of other kinds has prevented me from digesting, until now, the results of these experiments, and from stating the grounds of the conclusions which I formerly announced. The present paper, as it stands, having been submitted to the Council on the 15th May 1840, is printed by their authority. The following is the memorandum just referred to, reprinted from the *Proceedings of the Royal Society of Edinburgh*:—

"*On the Effect of the Mechanical Texture of Screens on the immediate Transmission of Radiant Heat. By Professor Forbes.*—On the 2d September 1839, M. ARAGO communicated to the Academy of Sciences a letter by M. MELLONI, containing some very interesting experiments on the transmission of Radiant Heat. M. MELLONI finds, that rock-salt (which is well known to transmit rays from every source with equal facility) acquires, by being *smoked*, the power of transmitting most easily heat of low temperature, or that kind of heat stopped in greatest proportion by glass, alum, and (according to M. MELLONI) every other substance. The experiments contained in the Third Series of my Researches on Heat, shew that this is equivalent to saying, that substances in general allow

facility) acquires, by being smoked, the power of transmitting most easily heat of low temperature, or that kind of heat which is stopped in greatest proportion by glass, alum, and (according to M. MELLONI) every other substance.

only the more refrangible rays to pass; and as M. MELLONI had been led by his previous experiments to the same conclusion, his statement amounts to this, that, whilst rock-salt presents the analogy of white glass, by transmitting all rays in equal proportions, every substance hitherto examined acts on the calorific rays as violet or blue glass does on light, absorbing the rays of least refrangibility, and transmitting only the others.

"M. MELLONI believes, that the first exception to this rule, or the first analogue of red glass, is rock-salt previously smoked. I desire, however, first to call attention to the fact, that, in a paper published in May 1838 (Researches on Heat, Third Series), I described a substance having similar properties, namely, mica split by heat to extreme thinness, such as I employ in polarizing heat. In the month of March 1838, I had established by reiterated experiments, that the transmission of heat through glass, far from rendering it less easily absorbed by mica in this peculiar state, had a contrary effect, and also that heat of low temperature, wholly unaccompanied by light, was transmitted almost as freely as that from a lamp previously passed through glass.

"It even appears, from experiments I have since made with the same form of mica, that some specimens transmit *scarcely half* as much luminous heat previously passed through glass, as that from a body below visible incandescence.

"Mica itself, not laminated by the action of fire, possesses, as I have shewn by contrasted tables in the paper referred to (Art. 23, 24), properties exactly the reverse; hence the effect is due to the peculiar mechanical condition of the body, and not to its elementary composition.

"It, therefore, at once occurred to me, on reading M. MELLONI'S communication, that the effect of smoking the salt might be merely owing to a mechanical change in the surface affecting the transmission.

"Roughening the surface was the most obvious experiment, and I found, as I anticipated, that heat of low temperature is very much easier transmitted by salt scratched by sand-paper in two directions at right angles, than luminous heat. Thus, a plate of salt which, when well polished, transmits 92 per cent. of heat derived from a lamp, and sifted by a glass plate, and also 92 per cent. of heat wholly unaccompanied by light, transmitted, when roughened, only 17 per cent. of the former and 45 per cent. of the latter.

"A thin plate of mica, when similarly scratched with emery-paper, so as merely to depolish it, transmitted much more nearly the same per-centage of heat from different sources than when *bright;* shewing, that the loss of polish affects the transmission of the more refrangible rays much more sensibly than that of the others.

"Yet this effect is not attributable to a variation in the ratio of the reflection of heat of different kinds at the surfaces of the plate. For, in the *first* place, I have proved, and already communicated the fact to the Royal Society (see Proceedings for April 1839), that reflection takes place at a polished surface, with almost, if not exactly, the same intensity for all kinds of heat; and, *secondly*, I have found, by direct experiment, that, at least for the higher angles of incidence, reflection is most copious from rough surfaces for heat of low temperature, or the same kind which is most freely transmitted, proving incontestably that the *stifling* action of rough surfaces is the true cause of the inequality.

"That there is a real modification of the heat in passing through a roughened surface, as well as through laminated mica and the smoky film, appears from direct experiments which I have made on the heat sifted by these different media; which, when transmitted by any one of these, is found in a fitter state to pass through each of the others; and this modification is found to be more per-

2. In the Third Series of these Reseaches, § 3, I have attempted to demonstrate, directly and numerically, that the rays of heat which have passed through alum, glass, and indeed every substance which I tried, have a *mean refrangibility* superior to that of the rays before such transmission; and as M. MELLONI had been led in a general way by his previous experiments to a similar conclusion, he inferred, and justly, that most diathermanous bodies absorb the less refrangible rays in excess, and therefore are to heat what green, blue, or violet diaphanous media are to light. Rock-salt alone (so far as we know) possesses the property of *indifferent diathermancy*, and is the single analogue of white transparent glass.

3. The generalization of this principle is a matter of much importance, and especially as it carries our knowledge a step higher in the scale of truth, by teaching us to refer to the quality of *refrangibility* certain properties of heat, which before were connected only with certain vague characters of the nature of the source whence it was derived. Amongst other things we find, what was long suspected, but what M. MELLONI first conclusively proved, that the presence or absence of light is, to a great extent, immaterial; no doubt a *concomitant*, but

ceptible as the character of the heat is more removed from that which these media transmit most readily, that is, as the temperature of the source is higher. Thus, heat derived from a lamp, has 36 per cent. transmitted by a certain smoked plate of rock-salt. But if the heat transmitted by the smoked salt has previously been sifted or analyzed by transmission through another plate of smoked salt, through laminated mica, and through roughened salt, the per-centage is raised from 36 to 44 in the two former cases, and to 40½ in the latter, proving incontestably the specific action of these transmissions in arresting the more refrangible rays.

" I next considered, that as a moderate number of scratches appeared to produce this modification, it might be practicable to obtain the effect by transmitting heat simply through fine wire-gauze. I could not obtain it finer than sixty wires to the inch, and in this case I could obtain no indications of differences in the transmitted ratios of one or other kind of heat. The proportion transmitted to the direct effect, was, in every case, almost exactly that of the area of the interstices of the gauze to its entire surface.

" When fine gratings (used for FRAUNHOFER's interference fringes) made of cotton-thread were used, even in this case no difference was perceived; here, however, the thread, having probably a certain degree of permeability, might mask the effect.

" When fine powders were strewed between salt plates, leaving minute interstices, the easier transmission of heat of low temperature was again apparent.

" Having procured delicate lines to be drawn with a diamond point on a polished salt surface, first dividing it into squares 1-100th inch in the side, then into parallel stripes 1-200th inch apart, and finally into squares of the latter dimension, in every case the effect resembled that of random scratches, and was more apparent as the surface was more furrowed.

" I have finally to observe, that the mere process of natural tarnishing by the exposure of salt to the air, produces a similar effect.

" These facts evidently point to phenomena in heat, resembling diffraction and periodic colours in light. I cannot doubt that the simple transmission through fine metallic gratings would produce effects similar to those of the striated surfaces of rock-salt.—*December* 16. 1839."

not an *indispensable* circumstance. Again, certain relations had been established at an early period in the history of the science of heat, between the *colour* of a surface and the quantity of heat which it absorbed, and this relation for any two surfaces compared (as black and white, of similar textures), was first clearly shewn by Sir JOHN LESLIE, to depend upon the luminosity of the source of heat, to which conceiving it proportional, that philosopher based upon it the principle of his Photometer.* Professor POWELL, of Oxford, conceived and executed an ingenious experiment, by which it is demonstrated that the interposition of a screen of glass, though it stops but little light, alters most materially the influence of colour on the transmitted heat, thus annihilating at once the principle of photometric measurement adopted by LESLIE, except in a very limited class of cases.† M. MELLONI has fully confirmed the experiments of Professor POWELL,‡ which therefore may be considered as establishing this conclusion, that the quality of *blackness* or *whiteness* of a surface affects its power of absorbing heat (*not* in proportion to the luminosity of that heat, as was formerly supposed, but) in proportion to its refrangibility.

4. It is both convenient and correct, therefore, to consider the refrangibility of heat as the cause of most of its distinctions of *kind* and *degree* of modification in our experiments, instead of making vague reference to the temperature of the source whence it is derived. Heat derived from the following scale of temperatures corresponds to heat of progressively elevated refrangibility; as, 1. Heat from ice has a less refrangibility than that from, 2. the hand, which again is below, 3. that from boiling-water; then comes, 4. that from a vessel of mercury under its boiling temperature, 5. a piece of smoked metal, heated by an alcohol lamp behind, but itself quite invisible in the dark, 6. incandescent platinum (a coil of wire in an alcohol flame), 7. an oil lamp (LOCATELLI'S). Such is the scale of heat which has often been referred to in M. MELLONI'S researches and my own; but though our apprehension of the temperature of the source ceases to be so clear above this limit, and the colour and brightness of the light which accompanies the heat no longer varies distinguishably, the scale may be carried upwards indefinitely by interposing screens of different materials, which either may be proved *directly* (as I have done in the Third Series of these researches) to increase the refrangibility, or we may take Professor POWELL'S, or any similar test, which our experiments lead us to conclude to be co-ordinate with the fact of refrangibility. Such a prolongation of the scale of heat-sources would be,

* Essay on Heat, 1804.

† Phil. Trans. 1825, p. 187.

‡ Ann. de Chimie, *Avril* 1834. M. MELLONI finds, for instance, that the rays from an oil-lamp falling on black and white surfaces, affects their temperature in the proportion of 1000 : 805. And the same proportion holds if they be transmitted through a plate of rock-salt; but if a plate of alum be used, though equally transparent for light with the salt, the proportion is now 1000 : 429.

8. Oil-lamp heat transmitted by Common Mica.
9. ———————————— Glass (Argand lamp).
10. ———————————— Citric Acid.
11. ———————————— Alum.
12. ———————————— Ice.

A clear appreciation of the scale of refrangibility as the important test for the qualities of heat cannot be too clearly apprehended and admitted. Heat from *any* source, if it admit of transmission at all through glass, alum, or water, will ultimately have the character of glass-heat, alum-heat, or water-heat, just as light from the sun, or from a candle, becomes red, blue, or green, by transmission through glasses of these colours.

5. Now, when M. MELLONI had shewn (and this experiment I believe was original to him), that substances which stop every ray of even intense light (as opaque glass and some kinds of dark mica), yet transmit a sensible quantity of heat, it was not unnatural to inquire whether the *invisible heat* thus obtained from a *luminous* source, might not possess the qualities of heat from a dark source, in other words, whether bodies, like black glass and mica, instead of stopping the less refrangible rays like glass, alum, &c., would not suffer these to escape, and absorb the most refrangible rays, acting upon heat as a body does upon light, which stops the yellow, blue, and violet rays, that is, as Red glass does.

6. Experiment partly fulfils this expectation, and partly not. The careful and complete series of experiments made by M. MELLONI upon the qualities of the invisible heat thus obtained,* shews, that although it resembles low-temperature-heat, in so far as it is very feebly transmitted by alum or citric acid, yet low-temperature-heat (that from boiling-water for instance), is but very faintly transmitted through the black glass or mica, which ought not to be the case if these bodies acted like a sieve, which arrested the more refrangible rays, and suffered the others to escape.

7. The direct test, however, of examining the refrangibility of the heat-rays issuing from opaque screens yet remained; and in applying this, I proved that opaque glass and mica act as clear glass and mica do in *elevating the mean refrangibility of the transmitted heat.* Hence I concluded that the effect of such media upcn heat is to absorb the rays of greatest and least refrangibility, in short, to act as homogeneous yellow glass would do upon light, the *mean* refrangibility being on the whole, however, increased by transmission. I also pointed out that heat from luminous sources is probably far more compound in its nature than dark heat; that the darkness of heat is no test of its refrangibility; and that even the most refrangible rays may contain heat separable from the light which accompanies it.†

8. In all this, then, there appears nothing exactly equivalent to the action

* Annales de Chimie, *Avril* 1834. † Researches on Heat, Third Series, art. 73, 81, &c.

of red glass upon light,—no substance which transmits most easily heat of low Refrangibility and Temperature, and which separates heat of that description from the compound emanation from luminous sources. Reasoning probably upon the conclusions just stated, M. MELLONI conceived the happy idea of combining an opaque substance, such as smoke, with a solid, which itself should effect no specific change upon the incident heat. He therefore smoked *rock-salt*, and found that it presented a complete analogy to red glass, transmitting most easily heat of low temperature and refrangibility.

9. Whilst I give full credit to M. MELLONI for the ingenuity and importance of his experiment, I must be permitted to state, that I conceive that I preceded him by eighteen months in the discovery of a substance possessing similar properties, although I very readily admit, that, having been led to that observation incidentally, I first pursued the remark into consequences which I considered important, after M. MELLONI had called particular attention to the experiment with smoked surfaces. On the 27th February, 19th and 20th March 1838 (as appears by my Journal of Experiments), I proved that Mica, split into very thin films by the action of heat, such as I employ for polarizing, possesses the property of transmitting in larger proportion several of the less refrangible kinds of heat, and in particular, that it transmits heat from a source perfectly obscure, in almost exactly the same proportion with the highly refrangible heat of a lamp transmitted through glass. I have no hesitation in saying, that no other substance known previously to M. MELLONI'S experiments with smoked salt, gave any approximation to the following results, which are taken from the Third Series of my Researches, art. 24.

TABLE of the proportion of Heat from different Sources transmitted by the Polarizing Mica Plates I and K, contrasted with the transmissions by Mica in its usual state, and with Black Glass.

Source of Heat.	Mica split by Heat. Plates I and K.	Mica .015 inch thick.	Opaque Black Glass.*
Locatelli lamp, with glass, . .	100	100	100
Locatelli, ...	116	79	70
Incandescent Platinum, ...	108	70	...
Brass at 700°, ...	96	21	...
Heat at 212°, ...	62	11	...

* A contrast experiment made at the same time, March 20. 1838.

10. This singular result of the mechanical condition of the mica did not fail to strike me greatly at the time, and was not published until after careful repetition. It afforded a triumphant reply to an objection against my experiments which I was then combating, that the quantity of heat absorbed by the polarizing

plates had modified and even inverted the results, and having satisfied myself of that, I did not pursue the matter farther. The moment, however, that I read M. MELLONI'S communication on Smoked Salt, I perceived the important light which the perfectly analogous case of the split mica might throw upon the phenomenon. It was evident that the results were similar in kind, it was probable that they might be made to approximate in degree. Instead, therefore, of interposing mica piles at the great and disadvantageous obliquities which I had employed (when I wished simply to test their action as polarizing plates), I took a split mica pile (frequently referred to in former parts of these memoirs under the designation H) and placed it *perpendicularly* to the incident rays of heat. I obtained the following results:

TRANSMISSION THROUGH SPLIT MICA H, AT A PERPENDICULAR INCIDENCE.

Source of Heat.	Per 100 of Incident Rays.	Relative Transmission.
Locatelli with glass, ...	9.2	100
Locatelli, ...	13.7	150
Dark hot brass, ...	17.3	188
Hot water, ...	16.3 *	178

* This observation having been made at a different time from the others, and probably not under exactly the same circumstances, I have stated it in the way least favourable to the views I entertain: the per-centage actually observed was 19.

11. It appears, then, very clearly, that this peculiar condition of mica induces, in opposition to the natural quality of the substance (9), the same peculiarity which a film of smoke possesses relatively to the incident heat. It is truly for heat what red glass is for light, it transmits most freely rays of lowest refrangibility.

12. Seeing clearly from the first that the change of character in mica was due to the splitting up into an almost infinite number of minute surfaces the natural laminæ of the mineral mica; and attributing the character of redness (so to speak) to the multiplied and irregular reflections and interferences which must so take place, it occurred to me as very probable, that the effect of smoke was due to the superposition of a prodigious number of minute opaque points upon a transparent surface, and *that* not so much from any physical peculiarity of its carbonaceous material, as from the mechanical distribution of opaque dust over the diaphragm of rock-salt.

13. This induced me to try the effect of *mechanical alterations of the physical surface* of the salt, expecting to find an effect analogous to that of smoking, and, guided by no other grounds of conjecture than those which I have stated, I roughened with sand-paper both sides of a polished plate of rock-salt, furrowing each surface rectangularly until it was quite dim. I then examined its trans-

missive power for heat from different sources, and was gratified to find my anticipation realized. The proportion of dark-heat transmitted, compared to that from a lamp sifted by glass, was no less than as 3 to 1.*

14. It thus appeared that there are at least three conditions under which a medium can be found capable of transmitting heat of low refrangibility, and that two of these had reference *solely* to mechanical constitution. It was natural to generalize and attempt to include the case of the film of smoke, as well as the striated and the laminated surface, under one category. I have already said that the mechanical distribution of the opaque carbonaceous particles offered a plausible analogy, which I proceeded to attempt to carry out.

15. The numbers in art. 10, may be compared with the following:

Source of Heat.	Transmission per 100 of Incident Rays, by		Relative Transmission by	
	Smoked Salt.	Rough Salt.	Smoked Salt.	Rough Salt.
Locatelli, with glass, ...	30	49	100	100
Locatelli, ...	...	62	...	126
Dark hot brass, ...	58	70	192	142
Hot water, ...	67	77	223	157

16. It occurred to me that if the action of the smoke was entirely a *superficial* one, or due to the character of a rough surface applied to the plate of rock-salt, that the effect of two such surfaces upon the transmission of heat would probably differ from that of a single film of smoke, so thick as to produce an

* I state it as a proof of the conviction which I had of the real character of split mica with respect to heat, that the reasoning stated in the text was founded upon no experiments made subsequently to those of March 1838 already quoted. The very first entry in my journal-book of last autumn contains *simultaneous* experiments, (1.) on smoked salt, to verify M. MELLONI's observations: (2.) on split mica, to extend my own of March 1838 to perpendicular incidences: (3.) on scratched surfaces, on the assumption that the two former would be realized. As M. MELLONI thinks that I had not a clear idea of the properties of split mica, which, indeed, if I understand him, he still doubts, I will quote *verbatim* the passage in my laboratory-book alluded to.—" 1839, Nov. 12. M. MELLONI having lately stated (Comptes Rendus, 2d Sept.) that smoked rock-salt is the only substance known which transmits heat of low temperature easier than luminous, this is in the first place contradicted by my experiments of 1838, Mar. 20. &c. on mica split by heat, already published,—and in the next place, I felt [feel] some doubt whether [in his experiments] it was the quality of the *material* or only the *surface* which affects the result. To try this, and to verify previous experiments, I smoked a plate of rock-salt; I *roughened* another with sand-paper, first on one, and then on both surfaces; I had also the split mica plate marked H placed *perpendicularly* to the rays of heat."

[*Here follow the experiments.*]

" It clearly appears, then, that salt simply roughened transmits most Dark Heat. I presume that the effect of smoking is only superficial, and that roughening stifles luminous heat faster than dark heat."

This is the *first* entry in my book after the publication of M. MELLONI's letter in the Comptes Rendus, and it is given *entire*.

equal absorption of heat of any particular degree of refrangibility. For this purpose I smoked three plates of polished rock-salt, so that two marked D and E absorbed *together* as much dark heat (very nearly) as the third plate A did alone.

17. I may take this opportunity of mentioning the way in which I have succeeded in smoking inflammable surfaces without burning them, or crystallized plates, like rock-salt, which crack and fly by the direct application of the flame of a candle. A coarse gas flame, surrounded by a wide metal tube 10 or 15 inches long, against the side of which the flame partly plays, affords a stream of comparatively cool smoke, which may be applied to any given surface. With these three smoked salt-plates I obtained the following results:

	Source of Heat.		
	Locatelli with Glass.	Locatelli.	Dark Heat.
	Per Cent.	Per Cent.	Per Cent.
Smoked Salt Plate A, ...	8.3	17.2	32.9
——— D, ...	26	41	58
——— E, ...	23.5	36	53.5
——— (D + E) . .	7.3	18	32.1

As most of these results are from single experiments, the first and the last line must be considered as almost identical, and certainly do not indicate any material specific difference in the absorbent qualities of one thick and two thin films of smoke, which might be expected if the action were a merely superficial one.

18. From these numbers we deduce another conclusion of some importance. Since a film of smoke transmits most easily heat of low temperature and refrangibility, we may expect that it will modify the quality of any compound beam of heat which it transmits, and that one such transmission will therefore render a second more easy. Now, we find that the plate D transmitted 26 per cent. of heat from the first of the above sources, and that of the 26 rays escaping from D, and falling upon a second smoked film E, E transmitted 7.3, or 28 per cent. of those incident upon it. But by the third line of the table E transmitted 23.5 per cent. only of the direct rays, consequently the capacity for transmission has been increased. In the same way for Locatelli heat we find the per-centage for E raised from 36 to 44 by previous transmission through D; and for dark heat from 53.5 to 56.

19. Hence a useful application of smoked surfaces to which I have sometimes had recourse. It is often important to operate with more or less refrangible rays of heat under exactly the same circumstances of parallelism or divergence, and intensity. Having adjusted an oil-lamp with a salt lens, so as to afford a compound beam stronger than required, we may, by interposing a plate of smoked salt, absorb the most refrangible rays, and suffer the others alone to pass, and by then using a glass of proper thickness, the intensity of the heat may be reduced

in the very same proportion, but the more refrangible (hottest) rays are alone retained.*

20. Now the results of (17), though not what I anticipated as most probable, do not altogether relieve us from some doubt as to the nature of the action of the film of smoke, although those experiments, as well as others which are to be detailed in this paper, incline me to M. MELLONI's opinion, that the smoke acts by its own intimate constitution, and not by its mechanical arrangement. Though I have examined smoky films with a powerful microscope, I have failed in detecting the minutely divided particles of carbonaceous matter of which it must undoubtedly consist. Still the reticulation which fine powder strewed on a surface must form, if it act by the minuteness of the spaces which are left (as in diffraction-experiments on light), must act more intensely when by superposition such reticulations become more minute and complicated. And it may little matter whether the smoky screens are distinct, and deposited on separate plates mechanically placed in succession, or whether they are accumulated by continued smoking on a single surface. I do not state this with a view to maintain my own original opinion, which I am rather disposed to abandon, and to consider a smoked surface, *diathermanous*, as well as *transparent*, in the full meaning of the words; but in extending my experiments to roughened surfaces, I was rather surprised to find that the continued action of furrowing the surface by scratching it with coarse sand-paper, not only diminished the transmission of heat, but increased the *specific* action on rays of different refrangibility, whilst one would rather have imagined that the action being here due to the destruction of polish, and therefore *superficial*, any exaggeration of the roughness would not have increased the relative *diathermancy* to rays of low refrangibility.

21. Conclusive experiments, however, mark an increased sensibility to various kinds of heat by increased roughness. Two plates of salt, marked *a* and *b*, having been scored with sand-paper in rectangular directions on both sides, were placed so as to intercept similarly a parallel beam of heat. The difference of the following numbers is due to the less degree of roughness of *a*.

	Source of Heat.		
	Locatelli with Glass.	Locatelli.	Dark Hot Brass.
	Per Cent.	Per Cent.	Per Cent.
Rough Salt Plate *a*, ...	30	48.5	59
——— *b*, ...	16.6	28.5	45
——— $(a+b)$, . . .	7.2	16	27.5
Per-centage of heat received through *a* transmitted by *b*,	24	33	46.5
Ratio of *a* to *b*, ...	100 : 55	100 : 58.5	100 : 76

* *Smoked glass* is evidently an excessively opaque compound medium, being composed of two parts which absorb opposite ends of the heat spectrum. It is curious to reflect how little the true

Here, then, we find the per-centage of transmission raised in every case by a previous transmission through a rough surface. The increased facility of transmission is greater in proportion as the incident heat was more heterogeneous; dark heat undergoes very little change. It appears also by the last line of the table, that the increased roughness of *b* compared to *a*, had *enhanced* the characteristic effect (analogous to *redness* for light).

22. I have made a great many experiments to satisfy myself that the action of all the three media already specified (14) is precisely analogous, and that they actually insulate similar rays by absorption. The following table is a specimen, shewing the increased facility with which rays of heat, from whatever source, are transmitted by smoked rock-salt after previous transmission through the same or other substances.

TABLE shewing the Per-centage of Transmission by the Smoked Rock-Salt Plate E for heat from different sources, and modified by passing through the following Media.

Source of Heat,	Heat transmitted by			
	Nothing.	Split Mica H.	Smoked Salt D.	Rough Salt *a*.
Locatelli with glass, ...	23.5	...	28	29
Locatelli, ...	36	43.5	44	40.4
Dark hot brass, ...	53.5	56	56	55

23. It is very important to consider how this action of rough surfaces may be explained, and whether we have any analogous phenomena in the case of light. Can it be owing to the circumstance that the depolished surface reflecting differently the various kinds of heat, those kinds least copiously reflected persevere, and form the majority of the transmitted rays? To this it may be replied, that the intensity of reflection at polished surfaces, is so insignificant at a perpendicular incidence for either heat or light,* that were the *whole* specularly reflected heat, transmitted in the one case, and absorbed in the other, the difference, instead of amounting to 30 per cent. or more, of the incident heat (21), could not exceed 4 per cent.

24. Arguing from the analogous case of light, I anticipated, on the contrary, that the *reflected* as well as the *transmitted* beam, would be more intense from

cause of the opacity of a film of smoke deposited upon glass was understood at the time that it was quoted as a convincing proof of the *immediate* radiation of heat through solid bodies. Far from smoke being the untransparent substance supposed (I use the word loosely in applying it to heat), it transmits a quantity of some kinds of heat really surprising, although the thickness of the smoke be considerable.

* See MELLONI, Ann. de Chimie, Dec. 1835, and my Memorandum on the Intensity of Reflected Heat and Light, Proceedings Royal Society of Edinburgh, p. 254.

such a surface, as it is well known that polish becomes more specular for rays of light consisting of longer undulations, the inequalities of the surface first becoming insignificant for red light.

25. In this I was not deceived. My purpose not being to investigate fully the subject of diffuse reflection, I confined my attention to the establishment of the general fact. Employing an apparatus which I have not yet described, but which bears a great analogy to that figured in the Society's Transactions, vol. xiv, Plate XIII., and described in art. 51 of the Third Series, I observed the intensity of reflection of heat from different sources at a *single* polished surface of flint-glass, and at a similar surface depolished with emery. I obtained at considerable incidences the following striking results as to the increased susceptibility of heat to be *regularly* reflected at a rough surface, when it is of low temperature or refrangibility.

Ratio of the Intensities of Heat reflected by a POLISHED and a ROUGH Surface of Flint-Glass.

Angle of Incidence.	Source of Heat.		
	Locatelli with Glass.	Locatelli.	Dark Hot Brass.
60°	...	100 : 34	100 : 35.4
70	100 : 26.5	100 : 38.3	100 : 43.5

So far then the character of the action of depolished surfaces is consistent. *The stifling effect* (which diminishes both the reflected and refracted ray) *of a rough or laminated surface, diminishes with the refrangibility of the incident heat.* That the same thing takes place in the Reflection of light we know ; it is probable that it does so in its transmission likewise, though this has not been so distinctly observed. Most impure substances transmit a ruddy gleam, vapour of water does so whenever it is not colourless,* and every practical optician knows, that in a great majority of media the violet end of the spectrum is first absorbed.

26. A more minute analysis of the influence of surface upon heat is what we now propose. And three questions present themselves for immediate solution, (1.) If deficiency of polish produce a variation in the proportion of not less than 3 to 1 in the quantity of transmitted radiated heat from different sources, can we employ salt plates with the ordinary degree of polish, and yet consider them as equally transparent for every kind of heat, as M. MELLONI'S discovery has hitherto entitled us to do? (2.) Is the effect of roughness common to other substances as well as rock-salt? (3.) The operation of depolishing with sand-paper is nothing more than the making of an infinite number of distinct grooves on a polished

* Edinburgh Transactions, vol. xiv. p. 371.

surface; supposing these grooves to be regularly formed, and capable of numerical estimation, will the effect continue?

27. (1.) With respect to the first of these questions, it is satisfactory to be able to answer it affirmatively in a general way. I took two salt plates, of which the surfaces had not been *regularly* polished for a long time, and which, though bright and clear, were by no means particularly even and true. Of heat from LOCATELLI's lamp previously sifted by glass, these *four surfaces* of rock-salt transmitted 72 per cent. With dark heat from smoked brass the per-centage was 73, a difference which, in this experiment, could hardly be considered as appreciable. *The transmission of these two very different kinds of heat was therefore equal.* M. MELLONI has shewn that when rock-salt is pure and perfectly polished, .92 of the incident heat is transmitted by a pair of surfaces, and therefore four surfaces should transmit $(.92)^2$ or 84.5 per cent. This estimate I have verified, and am satisfied of its accuracy. The deviation in the present case (which I think it right not to pass over) is due partly, no doubt, to the inequalities of surface but chiefly to some imperfections in the salt itself, which, as the experiment was merely a relative one, were not adverted to. In contrast with this, I used at the same time (December 11. 1839) a piece of salt, which once had been polished on both sides, but which, by being laid aside for some years, had become completely dull and grey on its surface. This specimen, then, was simply *depolished*; it contained no furrows, and had been subjected to no mechanical action whatever. Its per-centage of transmission was,

	Locatelli with Glass.	Dark hot Brass.
Tarnished salt, ...	66	77

clearly establishing the general principle.

28. (2.) With respect to the question, whether roughness of surface has a similar effect in modifying the diathermancy of other substances as well as rock-salt, we are able to give a distinctly affirmative answer. Rock-salt being, so to speak, quite indifferent to the quality and source of the incident heat, any cause of specific action becomes immediately apparent. Not so with any other substance, which, exercising already a specific action in virtue of its nature, is to have that specific action modified by a modification of surface. At least the question is, whether or not this modification will occur? An example will best illustrate how this modification may be discovered and expressed. I took a plate of mica with its natural bright surfaces, and so thin as to transmit in considerable abundance heat from different sources. The per-centages in this state were determined as follows:

	Locatelli with Glass.	Locatelli.	Dark heat.
Mica with *bright* surfaces, ...	83.5	74	37

Both sides of the mica were depolished with emery-paper, and the experiment repeated (27th November 1839),

	Locatelli with Glass.	Locatelli.	Dark Heat.
Mica with *rough* surfaces, ...	45.5	51	31.5

Denoting the original transmissions by 100, the diminished effect due to the roughness of the surface will be represented by

	54	69	85

demonstrating as clearly as possible that the stoppage is proportioned to the temperature of the source of heat; thus, whilst 46 per cent. of the first kind was arrested by the roughness of the surface, only 15 per cent. of dark heat was stopped.

29. (3.) With regard to the third question, the action of a comparatively small number of scratches on a polished surface, instead of a general diminution of its polish, I proceeded thus: I caused a series of extremely minute lines to be drawn mechanically with a diamond point, on a well polished surface of rock-salt, so as to divide it into squares having *one-hundredth* of an inch for their side. A similar plate was scored by fine lines in the same manner, parallel to one another, and *one two-hundredth* of an inch apart. A portion of this second plate was crossed rectangularly, by lines drawn at the same distance, so as to divide the surface into squares four times smaller than in the first instance. These three media gave the following results with two very different kinds of heat (December 6–11. 1839).

Source of Heat.	Scored in squares 100 lines to the inch.	Scored in lines 200 to the inch.	Scored in squares 200 lines to the inch.
Locatelli with glass, ...	76.5	61.5	45
Dark hot brass, ...	82.3	68.5	64.5 *

* The part of the second plate which was scored *across* being more free from flaws than that which was *once* scored, explains the little difference between this number and that in the preceding column.

For heat of 212° the per-centage was still higher, as will afterwards be shewn.

30. *Metallic Gratings.* If the mere defect of transparency were the cause of the peculiar action of scratched surfaces, we might expect that any opaque filaments would act in the same way. Could we dispense with the *medium* altogether, and employ a *screen*, which should have the qualities which we had artificially given to the physical surface of the medium, we should evidently have advanced a step in the interpretation of the phenomena. The action of grooved surfaces and gratings upon light suggested so forcible an analogy, that before I was able to procure the mechanically striated surfaces, described in the last article, I had employed fine metallic wire-gauze as a diffraction-screen, hoping to obtain results similar to those which I anticipated, and afterwards did obtain, by drawing fine lines upon rock-salt.

31. The fact that diffraction-phenomena in light, produced by gratings, are wholly irrespective of the nature of these gratings, as, for instance, whether they be formed of metal-wires, or mere lines drawn through a soapy film stretched on

glass, gave some countenance to this experiment. I was not unaware that diffraction spectra are produced, not by a parallel beam of light, but by a picture, formed of a distant luminous point. Still, though the ground or field illuminated by parallel rays passing through a grating must evidently have a uniform tint, it does not appear absurd to suppose that that tint may be different from white. Nor does this question appear to have occurred to mathematicians or optical writers, until the problem presented itself to me in the course of this investigation.

32. With such wire-gauze as I could easily procure, I failed in obtaining any peculiarity of action as relates to heat from different sources; and farther, the quantity of heat intercepted by the metallic grating appeared to be nearly, or exactly, proportional to the surface of the opaque portion of the screen. Thinking that perhaps finer gauze than that I used (60 wires to the inch) might produce the desired effect, I obtained, through the kind assistance of Sir John Robison and M. Leonor Fresnel, the finest manufactured in Paris, going as high as about 160 per inch. In general my first results were confirmed, viz. (1.) that the proportion of heat stopped is irrespective of the source; (2.) that it is to the incident heat as the area of the wires is to the area of the surface. It must be observed, however, that the determination of this latter proportion with extreme accuracy by an examination of the grating, is not so easy as might at first sight appear. When the *wire* is fine compared to the *interstices*, the interstices are pretty nearly rectangular and equal-sided. But this is not the case in most manufactured wire-gauze. One set of wires is nearly parallel and straight, but not so the set interlaced with the former, which do not generally make their intersections at right angles, and hence, universally, the interstices are somewhat smaller than a calculation proceeding upon the number of wires per inch, and their diameter would give. Distrusting my own observations, I put three specimens of wire-gauze into Mr John Adie's hands, requesting him to determine the mean diameters and intervals of the wires. With a very accurate micrometer he determined 14 values for each of these quantities in both directions. From these data the proportion of the Interstices to the whole Surface of each grating is easily calculated, and the results are given below for three sorts of gauze of which I had *previously* determined the permeability for heat.

Micrometric Measurement of Wire-Gauze. Unit of Measure = $\frac{1}{52400}$ inch.

Wire Gauze.	Lengthwise.		Breadthwise.		Ratio of Interstices to Surface.
	Interstice.	Wire.	Interstice.	Wire.	
No. 1. (57 per inch), . .	534	371	562	384	.3504
No. 2. (92 per inch), . .	375.6	179.4	402.6	179.6	.4680
No. 3. (129 per inch), .	200	159	284	168	.3500

33. The numbers in the last column (computed on the supposition of the interstices being geometrical rectangles) are to be compared with the following experimental determinations of the proportion of incident heat transmitted by these gratings.

Proportions per 100 of Incident Rays of Heat transmitted by Wire-Gauze.

Wire Gauze.	Locatelli with Glass.	Locatelli.	Dark hot Brass.	Hot Water.
No. 1. (57), . .	32.5	32	33.5	......
No. 2. (92), . .	46.0 *	...	44.7 †	......
No. 3. (129), . .	30.5	...	30	29.7

* Two such gratings superimposed, so that the wires formed angles of 45° with one another, gave for the per-centage of transmission 20.7. The square root of this, or effect due to each grating, is 45.5, or almost the same as the number in the text.

† Two superimposed gratings gave 21.2 per cent., or 46 for each system separately.

The differences for each grating, perhaps, do not exceed the errors of experiment. In every case these numbers are *inferior* to the geometrical interstices, but what inclines me to think that this difference is due to the irregularities of figure of the gauze (including the effect of *flattening* of the wires where they overlap, making the interstices obtuse-angled) is this: that No. 2, in which the wires were finer compared to the interstices than in others (the total interstices being one-third part larger in proportion), and the gauze evidently far more regularly formed than in the other cases, the per-centage transmitted differs very little from the geometrical gauge. I own, at the same time, that a difference of 5 per cent. in No. 3 (which is evidently not due to an error of observation), seems to me barely accounted for by this remark.

34. *Thread Gratings.* With gratings of fine cotton-threads $\frac{1}{100}$ inch apart, used for shewing FRAUNHOFER'S Spectra, I obtained a similar result. These threads were arranged parallel-wise on two frames, capable of being superimposed rectangularly. Thus, we can either employ a screen of parallel threads *one-hundredth* of an inch apart, or a screen of mathematically accurate squares, formed by superposition. It is difficult in this case, however, to obtain the diameter of the thread accurately enough to estimate the ratio of interstices.

Per-centage of Incident Heat transmitted by Cotton-Thread Gratings, $\frac{1}{100}$ inch apart.

	Locatelli with Glass.	Dark Heat.
Thread Grating, SINGLE, . . .	29.5	30.2
——— ——— DOUBLE, . . .	9.0 *	8.3 †

* Corresponding *single* action = 30.0 per cent.
† ——— ——— ——— = 28.8 ———

The difference here seems imperceptible, the differences, such as they are, being in opposite directions. The results in the last column are from single experiments (November 28. 1839).

35. *Action of Powders.* Adhering to the idea (12) that the action of a smoked surface was due to the mechanical action of a number of minute opaque points distributed over a transparent body, it occurred to me almost at the commencement of these experiments, to try the effect of powders artificially sifted on such a surface. Any ingredient, however, which could make the powders adhere to the surface, would have vitiated the experiment, by introducing its own proper diathermancy. I therefore included the powders between two polished plates of rock-salt, closed at the edges with wax. The preliminary experiment (27), to shew that the salt surfaces, in the state in which I commonly employed them, exercise no perceptible influence on the quality of the transmitted heat, was evidently a very important one for the conclusions I meant to draw. It was, as I have stated, quite satisfactory.

36. The first experiments which I made with powders (December 6. 1839), were with Chalk and Alum, finely dusted between two plates of salt. I selected the chalk on account of its absolutely uncrystalline and opaque character; and alum, because its power of stopping rays of heat of low temperature was so very great, that I judged that if the influence as a *mechanical modifier of surface* should prove predominant, and allow as much, or more, heat of *low* than of *high* temperature to pass, the *mechanical* influence of a substance in fine powder would be clearly established.

37. Now, the result at which I arrived, and which was entirely conformable to my anticipation, may serve to shew the caution requisite in drawing conclusions from limited data, however apparently conclusive. The surfaces powdered with chalk suffered *rather more* heat of low than of high temperature to pass (viz. 34.5 per cent. dark heat, and only 30.5 of heat from LOCATELLI lamp, transmitted through a thick glass-lens), whilst the salt strewed with alum appeared quite indifferent to the kind of heat incident,* (transmitting only 17 per cent. of both, thus shewing that the powder was in considerable quantity). I concluded, therefore, with apparent reason, that the chalk having no *specific* action, or being (most probably) opaque or *athermanous*, the powder of it acting mechanically, allowed low-temperature-heat to pass in excess, whilst in the case of alum, the *specific* action was entirely counteracted by the *mechanical* action of the powder. I simply stated the fact amongst others detailed in the preceding pages, in a Memorandum presented to the Royal Society of Edinburgh on the 16th December 1839,† and a few days after, in a slightly different form, communicated to M. ARAGO, and printed in the *Comptes Rendus de l'Academie des Sciences*, 6th January 1840. On the 28th December, I

* Yet an alum plate of a certain thickness transmits no less than 27 per cent. of the one kind of heat, and *no sensible portion* of the other (MELLONI).

† See note page 1 of this paper.

obtained a similar result for Charcoal powder (whose affinity with smoke suggested its use), and yet it does not appear that the general conclusion which I intended is entirely warranted.

37. It is well known that Sir ISAAC NEWTON overlooked the variable dispersive power of bodies for light, in consequence of having compared two, in which the dispersion *happened* to be proportional to the mean refraction. A similar haste to generalize would have led to error on the present occasion, had not a simultaneous investigation led me to re-consider the subject of powders. Whilst waiting for the arrival of fine wire-gauze from Paris, it occurred to me to try the effect of metals in a state of extreme division. It seemed, however, first desirable to ascertain whether the metals are as incapable of transmitting heat as is commonly supposed.

38. For this purpose, I stretched a piece of the thinnest gold-leaf across a wide diaphragm of pasteboard, and suffered an intense parallel beam of heat from LOCATELLI's lamp to fall directly upon the pile. A screen of glass was interposed, which, by experiment, was known to stop 43 per cent. of this sort of heat. The needle of the galvanometer deviated 31°.2, the glass being interposed; the equivalent direct effect would have been $31.2 \times \frac{100}{43} = 72°$. When the glass was removed, and the gold-leaf put in its place, on the brass screen being alternately introduced and removed, not the faintest motion was perceptible in the needle; had it amounted to $\frac{1}{20}$ of a degree, that is, had $\frac{1}{1400}$ of the incident heat been transmitted by the gold-leaf, I considered that the effect would have been perceptible. Yet this gold-leaf was so thin that the features of a landscape could be distinctly seen through it, of the usual bluish-green tint. No more convincing proof certainly can be desired, that conduction plays no sensible part in these experiments, since it did not sensibly act on a film of one of the best known conductors of heat, and perhaps not more than $\frac{1}{300000}$th of an inch thick. I thought it worth while to repeat the experiment with dark-heat, and with the same results. The analogy of the action of split mica on light to metallic reflection led me to suspect, that if any kind of heat were transmitted by metallic leaves, it would be that of low temperature.

39. The imperviousness to heat of gold-leaf, the thinnest continuous film of metal which we can obtain, satisfied me of the importance of obtaining the metals in a condition to verify my experiments with the powder of other substances. When the hope diminished of obtaining wire-gauze of a degree of fineness (I mean fineness in the *wire*, not closeness of texture, for that was comparatively immaterial), which might vie with the diamond scratches on the salt surface, which presented, under the microscope, an irregular furrow, probably nearly $\frac{1}{2000}$ inch in mean breadth,—I recurred to the project of using the metals in *powder*. It was evident from the experiments on depolished and scored surfaces, that the *irregularity* of these streaks had nothing whatever to do with the phenomenon of checking rays of high refrangibility and admitting others. Sand-paper scratches,

than which nothing can be more irregular, produced the effect, and that more intensely as the surface became more coarsely and closely furrowed. Nay, it occurs in natural tarnish, where there can be no linear arrangement of the points affected. It seemed to me, therefore, that a surface covered with a metallic powder, presented the *limit* of a grating where the interstices were not required to have any regular form.

40. The next difficulty was to obtain impalpable powders indubitably metallic, to which I attached very considerable importance, for it was quite conceivable that the metallic sulphurets and other substances employed for the fictitious metallic powders called gold, silver, and copper bronzes, might have specific diathermancies which might injure the experiment. I at length succeeded in obtaining silver by precipitation, and copper from DANIELL'S Battery; and with some difficulty I procured from a large manufacturer coinage silver and gold, reduced by mechanical trituration to a perfectly impalpable and beautifully metallic powder. These expensive preparations are now wholly superseded by the admirable fictitious bronzes in use in the arts. These, together with metallic copper-bronze, perfectly impalpable, furnished by the same individual, and a much coarser tin powder used by druggists, formed the material of a very careful series of experiments, which I extended over a very considerable period, and varied in a great many ways. (1840, Jan. 28, &c.)

41. The following table contains the results of my experiments on Metallic Powders, which (with the exception of tin), may be considered as perfectly impalpable, adhering to the dry finger, and *undoubtedly metallic.*

Per-centage of Heat from different Sources transmitted by Metallic Powders.

Powder.	Locatelli Lamp.			Dark hot Brass.	Hot Water.	REMARKS.
	Glass interposed.	Direct.	Smoked Salt interposed.			
GOLD, No. 1, (adhering to a single surface of salt),	58	...	50.5	...	...	
GOLD, No. 2, (between two salt plates), . . .	7.4 *	...	...	4.1 *	...	
SILVER, No. 1, (between two salt plates), . .	25.3	24.2	...	21.8	...	Mean of a considerable number of results.
SILVER, No. 2, (adhering to a single surface of salt),						The same plate was used, but differently placed in respect of the pile, so that each series stands by itself.
1st Series, ...	27.7	...	18.5	...	...	
2d Series, ...	29.5	...	22.1	...	25	
COPPER, No. 1, (between two salt plates),						
1st Series, ...	14.8	...	16.0	...	...	
2d Series, ...	17.4	...	...	18.7	17	
COPPER, No. 2, (between two salt plates), . .	5.6 *	...	...	4.05 *	...	
TIN (between two salt plates), ...	27.0	26.0	...	25.5	...	Mean of a considerable number of results.

42. These observations are confessedly very imperfect. I am persuaded, however, that the apparent anomalies are not errors of observation; other in-

* See the next Article.

stances will presently occur. With a view to determine the quality of thickly strewed surfaces yielding a very feeble per-centage of transmitted heat, it was desirable to use an intense incident beam. In order, however, to keep the comparison within the range of galvanometer degrees, whose numerical values have been tested (Second Series, arts. 7–8), the observations in the preceding table marked thus * were made in the following manner. The *direct* effect of the incident heat on the pile was never observed, but only that part of it which penetrated the wire-gauze, No. 3 of art. 33, which transmits almost exactly 30 per cent. of every kind of heat. The direct effect was estimated at $\frac{100}{30}$ of the degrees of deviation corresponding to this transmission, and then the wire-gauze being removed, and the medium to be examined substituted, the effect was compared to the computed direct effect. For example, with the copper powder, No. 2, the effect of the LOCATELLI lamp, heat transmitted through thick plate-glass, and then modified by wire-gauze, was . . . . 22°.57

Direct effect $= 22.57 \times \frac{10}{3}$ . . . . 75 .2

Wire-gauze removed, and copper substituted, ... 4 .15

Ratio to direct, 5.52 : 100

In this way per-centages may be obtained with very nice accuracy: Another experiment gave in the same case 5.60 : 100.

43. The Table in art. 41. demonstrates to my conviction (strengthened by a careful examination of the very consistent observations on which it is founded), (1.) That gold, silver, and tin powders, instead of having the property which I was disposed to assign to opaque powders generally, do really transmit more heat of high than of low temperature; that is, act like glass, alum, and other transparent media in their common state. (2.) With respect to copper, two series give one result, and a third the opposite. Yet all of these were made with great care, and contain internal evidence of their accuracy. I am confident that the differences are not due to errors of observation; and I have observed other cases, in which an increase of thickness of the obstructing medium, and an increased intensity of the incident heat, gave altered results as to permeability, a result by no means paradoxical, since intense heat may be sensibly transmitted through a nearly opaque substance, and thence acquire a new character, which a feebler beam, transmitted through a less obstructing medium, would not possess. At all events, I can offer no farther explanation at present. That copper possesses a peculiar character, distinct from the other metals which I tried, I am fully persuaded.

44. The evidence which the experiments on metallic powders gave of the inadequacy of the *mere powdery form* to produce the effect of smoke, forced me to a more critical examination of other bodies in a similar state.

45. I repeated my experiments with increased care on the powders already employed. I tried a great number of new ones, chosen amongst substances differing as widely in nature as possible. Some of these substances were repeatedly

tried in different specimens, the powder more or less thickly strewed, and at different times.

46. One circumstance in particular raised a doubt as to the result of my former conclusion, where it seemed most incontrovertible. I had argued, that if alum in powder arrested equally all kinds of heat, the *mechanical* action of the powder must have opposed and destroyed the *specific* action of the alum (36.) I was gradually, however, led to admit, that, *in the state of powder, most diathermanous bodies are almost equally opaque*, or, rather perhaps, I should say, equally indifferent to the *kind* of incident heat (*i. e.* colourless in optics).

47. So far as the eye could judge of the proportion of obstacles in surfaces strewed with different kinds of powders, there did not seem any very marked peculiarity in their transparency for heat. A surface dusted with alum or citric acid appeared to transmit nearly as much as one strewed with powdered rock-salt. Nor could this arise merely from the *minute thickness* of the substance, which is well known to produce in heat, as in light, an approximation to a Colourless character; for the proportion stopped by the powder was always a large fraction (usually from $\frac{3}{4}$ths to $\frac{9}{10}$ths) of the incident heat. The opacity, then, is the result of the innumerable reflections and interferences which *scatter* and *stifle* the transmitted heat; and this is almost equally effectually done, whatever be the nature of the substance. On reflection, therefore, this general result does not appear surprising. I will quote one experiment, in particular, in illustration of it.

48. When I was at a loss to procure fine metallic fibres, I thought of employing a diaphragm irregularly covered with fine threads of spun glass, with a view (just as in the case of the alum powder) of ascertaining how far the mechanical condition of the glass might modify its well known qualities with respect to the transmission of heat. When Locatelli lamp-heat, having been transmitted by *thick plate-glass*, fell upon the spun-glass fibres, forming an irregularly reticulated diaphragm, no more than 47.5 per cent. of the incident heat was transmitted. Now, we know perfectly from the experiments of De la Roche and Melloni, that, after passing through such a thickness of plate-glass, an additional film, the thickness of the glass fibres used, would produce *no sensible resistance* to the farther passage of the heat, excepting only its superficial reflection. The loss of 52.5 per cent. of the heat was therefore due to the *scattering* and *stifling* of heat by Reflection at the surfaces of the fibres, Refraction through their cylindric surfaces, and Interference. We cannot, therefore, be surprised, if the refracted part of the heat reaching the pile (the only portion very materially affected by the nature of the medium) should not greatly alter the quantity of different sorts of heat indicated by the galvanometer. Accordingly, we find, that heat from a dark surface of brass warmed by an alcohol lamp, had 44 per cent. transmitted under the same circumstances; and even hot water had 42 per cent. although a small thickness of glass is sensibly opaque for that kind of heat.

49. If this be the case,—if the differences be so trifling—for a reticulation of regularly-formed, transparent, and polished threads of glass, much more must it hold with impalpable crystalline or other powders, presenting (no doubt) minute surfaces at every angle, and minute fissures in every direction.

50. The following Table contains the results of a large number of experiments on powders of various kinds, many of them repeated under various circumstances. The investigation is, as in the case of the metallic powders, confessedly imperfect; but since the broad simple principle which I at first tried to establish respecting the diathermanous quality of opaque powders does not appear to hold universally, I stopped this series of experiments, which were troublesome and laborious, after establishing a few general facts, which I will presently lay down, without attempting to exhaust a subject of which, by and by, we shall no doubt know more, but which at present it would be perhaps a waste of time to pursue into its insulated details. These powders were in all cases dusted between polished salt-plates, united at the edges, and then attached to diaphragms of card, so arranged as to transmit the heat in every case through the same parts of the surface.

Per-centage of Transmission of Heat, from different sources, through Non-Metallic[1] Powders.

Powder of	Source of Heat.			
	Locatelli Lamp		Dark Hot Brass.	Hot Water.
	Through Glass.	Through Smoked Salt.		
Alum, No. 1.	17.0	...	17.1	...
——— No. 2.	15.2 *	...	13.0 *	...
Citric Acid, No. 1. . . .	29	30	33 †	31.5
——— No. 2. . . .	12.9 *	...	8.7 *	...
Rock-salt, No. 1. . . .	12.8 * 13.4	11.8	11.3	...
——— No. 2. . . .	31.5 ‡	...	29.2 ‡	...
Sulphur, ...	50.0	...	44.7	...
Red Lead, ...	30.2	...	34.0	...
Galena, ...	26.3	22.4	...	...
Charcoal A.	5 ‖	...	9 ‖	...
——— No. 1. 1st Series, §	11.4	13.9	...	...
——— ——— 2d Series, §	15.1	...	16.0	17
——— No. 2.	3.2 *	...	3.5 *	...
Chalk, No. 1.	30.5	...	34.5	...
——— No. 2.	15.5 * 15.6	18.4	17.9	...
——— No 3.	27.5	...	32.0	...
Carbonate of Magnesia, .	8.3	12.6	...	...

* The observations marked thus were made with a powerful beam of heat in the way described in Art. 42.

† Not directly comparable with the other two observations on the same line, and probably 3 or 4 per cent. too high.

‡ Extremely good observations.

‖ The intensities very feeble.

§ The circumstances in these two series varied, so as to make the one not directly comparable with the other; but each is perfectly good.

51. On the preceding table, I would observe, (1.) That the pulverized crys-

[1] By *non-metallic* is meant, not in the state of a *pure* or uncombined metal.

talline bodies, such as rock-salt, alum, citric acid, and sulphur, exhibit no decided tendency to transmit an excess of heat of low temperature, depending on their powdery form. The carefully repeated experiment with rock-salt is, on this point, very conclusive, since its indifference as a substance to the quality of the heat which it transmits would at once leave the effect, if any, due to mechanical condition, apparent. It even very evidently appears in this state to transmit *less* freely heat of low than heat of high temperature. (2.) Galena, the crystallized sulphuret of lead, in fine powder, appears to possess the qualities of gold, silver, and tin (43.) (3.) Red lead, charcoal, chalk, and magnesia, all substances in an opaque *earthy* condition, appear certainly to transmit an excess of *Dark Heat*. I think it probable that this list might be extended to most bodies having a similar mechanical constitution.

53. These distinctions, I am well aware, leave the *causes* of the difference of character of powders, and the peculiarities of tarnished surfaces, nearly in the same obscurity as before. In particular, I cannot but regard it as being singular, that a surface covered with powdered salt has no analogy, but even opposite properties, to one of the same material mechanically furrowed.* The contrariety of action of metallic powders to those of opaque earths, is as singular as it was unexpected. I have already stated, however, my doubt whether a complete investigation of the peculiarities of specific substances would, *at present*, reward the necessary labour. I have made experiments on a few fibrous substances, as paper and membrane, which I thought might very probably act as tarnished surfaces do. There is an approximation to this, as will be seen, in the common cambric or tissue-paper. In the kind of tracing-paper employed (which is made in Paris, I believe, under the name of *papier vegetal*), there is evidently some foreign matter introduced to produce the transparency, which modifies the transmission. A close reticulation of cotton fibres has already been shewn to exercise no specific action (34.). The following Table contains a few results not included in preceding ones, and illustrating in several substances the quality of *heat-colour*, which in this paper we have been considering.

Per-centage of Heat transmitted by several Bodies.

Substance.	Source of Heat.		
	Locatelli Lamp with Glass.	Dark hot Brass.	Hot Water.
Gold-beater's Skin, ...	60	28	...
Cambric or Tissue Paper, . .	8.6	10.5	...
Tracing Paper (*Papier vegetal*),	36	28	...
Fibres of Spun Glass, . . .	47.5	44	42
Smoked Salt, ...	30.2	58	67
Roughened Salt, ...	49	73	76
Polished Salt, scored into 200 × 200 squares per inch, .	49.5	73	77

* To put this in the most clear point of view, I used and compared two such plates in the same experiment.

54. The leading facts contained in this paper are these:

55. I. The peculiar (*red-like*) character of films of smoke in transmitting heat of low temperature is partaken,—

A. By simple powder of charcoal.

B. By (at least some) other dull earthy powders.

C. By surfaces simply *dull* or devoid of polish.

D. By surfaces irregularly furrowed, as with emery or sand-paper.

E. By polished surfaces, on which fine distinct lines have been drawn.

F. By the mechanical lamination of transparent mica, which, as a continuous medium, possesses opposite properties.

56. II. The following media seem *indifferent* to the kind of heat which they transmit:—

A. The thinnest gold-leaf is impervious to any.

B. Metallic gratings transmit all kinds of heat in a proportion which is probably exactly as the area of the interstices which they present.

C. Thread gratings.

D. In a state of powder, most crystalline bodies *approach* to a condition of opacity for heat.

57. III. The following bodies, in addition to those commonly known, transmit most heat of high temperature (*violet-like heat*).

A. Several pure metallic powders.

B. Rock-salt in powder; and many other powders.

C. Animal membrane.

58. IV. Heat of low temperature is most regularly reflected at imperfectly polished surfaces. It is also, we have seen, most regularly transmitted. These facts are of great importance to the Theory of Heat; and may probably suggest inquiries of no small interest with regard to light, and especially the phenomena of absorption.

59. We have already (24) noticed the analogy which the fact stated in the last article bears to the easier reflection of red than violet light from depolished surfaces, and in that fact we find a confirmation of the application of the undulatory doctrine to heat, and of the opinion that the waves producing heat, are longer in proportion as the temperature of the source is less. The phenomena of transmission are more obscure; they may be compared either to the Diffraction, or to the Absorption, of light.

60. The action of lines on polished surfaces, similar to those used in many diffraction experiments, led to the inquiry (31), whether the mean colour of light transmitted by gratings was necessarily unchanged? The question does not seem to have occurred to any one to whom I have mentioned it; and though the most likely result would seem to be, that there should be no change, the grounds of such an *à priori* opinion do not appear absolutely conclusive. Professor KELLAND, how-

ever, has, I believe, first succeeded in integrating the expression for the illumination of a screen placed behind a grating of any kind (See AIRY's Mathematical Tracts, page 328) on which a plane wave falls, and he informs me, that in every case where the breadth of the interstices is any multiple of the breadth of the wires or opaque spaces, the intensity is the same as if there were a diaphragm equal in size to the sum of the interstices of the grating.

61. This result (which seems quite sufficiently general for our purpose) is so far confirmed by the absolute *indifference* of metallic gratings to the quality of the incident heat.

62. It remains, however, to be explained how furrowed surfaces can act, except by intercepting, as an opaque network would do, a part of the heat. I cannot give an explanation which appears full and satisfactory, but the condition of mica split into thin laminæ by heat, and producing the same effect, may serve to guide us, perhaps, to something like the true cause.

63. A number of thin plates, of *exactly uniform* thickness, would transmit a certain colour, and reflect the complementary one. If there be a great preponderance of plates approximating to a certain thickness, and if the disproportion of the lengths of the incident waves be great, a large proportion will be in like manner transmitted, and the remainder stifled or reflected. If this effect is not so frequently observed in bodies mechanically separated into films as we might expect, this is owing to the small range of length of wave in the visible parts of the spectrum. A small variation in the thickness of the film transmits or annihilates by interference each colour of the spectrum in succession. If the waves of heat be much more heterogeneous (as I have already surmised) than those of light, such effects would be proportionably more sensible.

64. Possibly a grooved surface may be considered as presenting a number of polished surfaces, partially detached from the general surface, under small obliquities to the incident rays; and we may suppose that these rays, after separation by partial reflection and refraction, reunite with unequal retardations, producing *first* a destructive effect upon the shorter waves, and suffering the others to persevere. I have already adverted to the fact, that most turbid fluids transmit chiefly the longer luminous waves. I offer these, however, but as vague conjectures upon a very obscure subject. I think that experiments on the Colour of media, such as those we have employed, and especially of depolished plates, might not be without value in illustrating the phenomena of Absorption in Optics.

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65. In conclusion, it might perhaps be expected that I should take some notice of the experiments and reasonings of which M. MELLONI has addressed an account to M. ARAGO, in two letters dated the 4th and 14th of March last, and published in the Comptes Rendus for the 30th of the same month. These letters were

occasioned by the announcement of my Researches, in the same work, for the 6th January. The present paper, founded *solely* upon experiments undertaken and completed before the dispatch of the earliest of M. MELLONI'S communications, will, I think, sufficiently answer all the questions which are started in his letters to M. ARAGO, at least all those in which my experiments are concerned.

12th May 1840.

II. *Account of some Additional Experiments on Terrestrial Magnetism made in different parts of Europe in* 1837. *By* James D. Forbes, *Esq. F.R.SS.L.& E., &c. Professor of Natural Philosophy in the University of Edinburgh.*

Read 6th April 1840.

51.* In 1836, I communicated to this Society the results of an extensive series of observations on Terrestrial Magnetic Intensity, made with the Hansteen Apparatus, which is the property of the Society. Some results with a small Dipping Needle, belonging to myself, were also given, but without great confidence in their accuracy.

52. I held then, however, the opinion which I still do, and to which the remarkable geometrical researches of Professor Gauss of Göttingen on Terrestrial Magnetism have given additional weight, that the element of horizontal intensity ought to be determined, and its laws of variation, in the *first* place, ascertained, independent of any other. Even should the deduction of total intensity be the sole ultimate object, I hold that an observer with only portable, and consequently imperfect instruments at his command, would do well to separate *completely* his investigations as to horizontal intensity from those upon dip, and then, by the skilful grouping of each set, having obtained a law of variation of each element according to the co-ordinates of Latitude and Longitude, the two partial results may be combined into the general one of total intensity (or $\frac{\text{hor. intensity}}{\text{cos. dip}}$), whilst either series may be used to check future observations, or be combined with any single future series in which one of the elements should be better determined.

53. As intensity observations on the Hansteen method are generally very superior to those of dip (considering the proportion which a probable error in either would alter the value of the total intensity), it is a pity to render worthless the good part of an observation, which contains an element capable of general and independent determination, by mixing it up with the erroneous results of an inferior observation.

54. It was on this ground that, in my former paper, read in December 1836, I carefully reduced the horizontal intensity observations by themselves, and it is

* These numbers are in continuation of those in the former paper on Magnetism, Edinburgh Transactions, vol. xiv. p. 1. The last paragraph of that paper ought to be numbered 50.

to that circumstance alone that I impute the consistency of the results obtained, and what I am inclined to consider the *first determination* worthy of confidence of the Decrease of the Horizontal Intensity with height above the level of the sea. If this effect is caused or modified by a variation of the dip, *that* investigation remains open to any future observer who is prepared to undertake so very difficult an inquiry. For the present we must be content to know the fact, that the horizontal part of the intensity diminishes as we ascend.

55. These observations, of course, have reference only to the particular methods of obtaining the dip and horizontal intensity which I have exclusively employed; namely, a statical method for the dip, and a dynamical one for the intensity. Professor LLOYD's elegant statical method of determining both elements at once, must of course be judged of on its own merits, and the same remark is applicable to the excellent results which Mr Fox has obtained with his Deflector.

56. The Council of the Royal Society of Edinburgh having agreed to provide a portable Dipping Needle to accompany their HANSTEEN Apparatus, one with a circle of six inches clear diameter was constructed under my directions by Mr ROBINSON of London. That size was selected in order that its bulk might not render it useless to the mountain traveller, and because I had been led, from previous experiment, to suspect that increase of size beyond a certain moderate limit is of little or no advantage in making dip observations. Increase of weight produces increased friction both directly, and because the steel axis requires increased strength, and therefore a larger diameter; and this probably out of proportion to the increased directive power of the needle's magnetism. My instructions to Mr ROBINSON were to make the needles with the most delicate axis that he could get a chronometer-maker to execute, indicating at the same time a very obvious construction by which (as in all modern needles the agate bearings are very narrow) the general strength of the axis may be made such as to avoid any chance of flexure by the weight of the needle, or any trifling accident. The working of the instrument more than satisfied my expectations, and I am inclined to think, judging from the detailed reports of observations made with dipping needles of larger sizes by the best makers, that the Royal Society's six-inch needle (which is arranged so as to pack into a mahogany-box only $10 \times 8 \times 2\frac{1}{2}$ inches external dimensions, and weighing 9 lb.), is capable of doing very nearly, if not quite as good, work as any hitherto made of larger dimensions.

57. I state this as my present belief, but I will enable the reader to judge. At the same time I speak of the needle when in *perfect adjustment*, recently from the maker's hands, for the effects of incessant jolting in long land journeys is very marked in deteriorating its performance. The best of the two needles which Mr ROBINSON has furnished (and let it be stated, to the credit of that excellent artist, that he is the first in this country who has vied with the workmanship of GAMBEY in the construction of this most troublesome instrument), gives results which I

have generally found not to differ by much more than *one minute* from the mean, when the observations have been made in favourable circumstances, the instrument *being in perfect adjustment*.

58. The method of making the dip observations has *always* been by a complete series of Eight Observations; four with the magnetism in each direction. The reversal of the poles has never been omitted. One of the needles (marked A. 1) gave a difference of about a degree when the magnetism was changed, indicating a displacement of the centre of gravity by far two great, and consequently introducing an error depending on the intensity of magnetization. Since 1838 this error has been reduced to less than a half.

59. With the needle A. 2, on which I place most confidence, I have generally found the difference of readings after successive displacements of the needle, and allowing it to come to rest, so insignificant, that I have very generally omitted this process, unless some discrepancy has led me to suspect an error, when I have repeated it over and over until the true result was clearly apparent. I am aware that this abbreviation of a tedious process will appear to many persons exceptionable. I have made repeated experiments in both ways, and with the assistance of another observer, our readings being separately recorded, and I am persuaded that whilst the condition of the axis remains nearly perfect, this most harassing operation may be greatly abridged.

60. The Intensity Needles employed in 1837, were the same as those which I used in 1832, and the methods of reduction employed are identical with those described in my former paper, art. 11, &c.* I need not here repeat them. Finding that Cylinder No. 1. still retains very nearly its magnetic constancy, so as to render any correction for epoch almost unnecessary, I have confined my deductions to observations made with it; and since Paris did not enter into the circuit of stations in 1837, I adopted, as fundamental, the relative horizontal intensity at Edinburgh, determined in 1833 and 1835, and since fully confirmed, viz. 0.840, that at Paris being 1.000. Consequently its time of vibration at Paris is constantly reckoned $247^s.70$ as before (art. 21).†

61. The journey I performed in 1837, was not undertaken for the purpose of making magnetic observations. They are, therefore, neither numerous nor regu-

* Ed. Trans. vol. xiv. p. 5.

† By art. 18, we found for Cylinder No. 1, at Edinburgh,—

		h	Log. Time 300 Vibrations.	Log. Ratio Annual Change.
	1829, July 9.	11	2.90765	.00043
	1832, June 2.	11	2.90890	.99956
	1833, May 7.	5	2.90849	.00033
	1835, May 4.	1	2.90915	.00007
To these we may now add,	1837, Apr. 27.	1	2.90900	.00070
	1838, May 10.	1	2.90970	

larly distributed. They may, however, be considered as composing two groups, one of which includes a number of the leading towns in Germany, thus checking former and somewhat discordant observations; and the other, as extending in some measure my former investigations as to the isodynamic lines of the middle or Swiss Alps, to the eastern part of that range.

62. I commence with the Intensity Observations:

Table I.

CYLINDER, No. I.

Place.	Date.	Mean Time.	No. of Vibrations observed.	Observed Time.	Rate Chronometer.	Arc.† α.	Arc.† *m*.	Temp. Reaum.	Corrected Time 100 Vibrations.	Intensity Paris =1.000.
	1837,	h m		s	s	°	°	°	s	
Edinburgh, . . .	Apr. 27.	12 42	100	272.36	+ 4.5	10	80	11.8	270.75	.837
....	...	12 52	100	272.04	+ 4.5	10	75	12.65	270.33	.840
....	...	1 10	100	271.97	+ 4.5	10	80	12.25	270.30	.840
Greenwich, . . .	May 5.	12 11	100	257.21	— 4.0	10	80	12.75	255.61	.939
....	...	12 21	100	257.39	— 4.0	10	80	12.2	255.85	.937
Brussels, ...	May 10. (*a*)	10 38	100	253.70	— 2.9	10	90	10.45	252.36	.963
....	...	11 0	100	254.23	— 2.9	10	90	9.55	252.98	.959
....	...	11 8	100	254.06	— 2.9	10	90	9.35	252.83	.960
....	...	11 22	100	254.09	— 2.9	10	90	9.0	252.90	.959
Bonn, ...	May 20.	3 13	100	252.06	— 3.0	10	80	13.0	250.45	.978
....	...	3 26	100	251.77	— 3.0	10	80	12.8	250.19	.980
....	...	3 40	100	251.66	— 3.0	10	80	13.1	250.04	.980
Drachenfels, . . .	June 21. (*b*)	3 43	100	258.10	— 5.0	10	90	21.0	255.52	.940
....	... (*b*)	3 56	100	258.43	— 5.0	10	90	20.6	255.89	.937
....	... (*b*)	4 15	100	258.09	— 5.0	10	90	19.8	255.65	.939
Göttingen, . . .	July 1. (*c*)	4 28	100	252.40	—29.3	10	90?	15.45	250.58	.977
....	...	5 0	100	252.03	—29.3	10	90	14.25	250.34	.979
....	...	5 17	100	252.01	—29.3	10	90	14.6	250.28	.979
Berlin, ...	July 22.	3 54	100	253.74	— 7.1	10	70	21.85	251.15	.973
....	...	4 9	100	253.83	— 7.1	10	70	23.6	251.04	.974
....	...	4 33	100	253.79	— 7.1	10	70?	23.05	251.06	.973
Dresden, ...	July 31.	5 20	100	249.58	— 7?	10	100	17.5	247.47	1.002
....	...	5 31	100	249.63	— 7.0	10	90	16.6	247.63	1.001
....	...	5 41	100	249.81	— 7.0	10	100	16.15	247.85	.999
Carlsbad, ...	Aug. 8.	5 37	100	247.26	— 7.0	10	80	17.35	245.22	1.020
....	...	5 47	100	247.13	— 7.0	10	70	17.2	245.12	1.021
Linz, ...	Aug. 14. (*d*)	3 48	100	242.36	— 7.0	10	85	22.75	239.76	1.067
....	... (*d*)	3 59	100	242.47	— 7.0	10	100	22.25	239.91	1.066
Ischl, ...	Aug. 17.	4 50	100	240.69	— 7.0	10	90	19.65	238.44	1.079
....	...	5 3	100	240.73	— 7.0	10	90	19.1	238.53	1.078
Salzburg, ...	Aug. 21. (*e*)	12 44	100	241.47	— 7.0	10	90	20.7	239.09	1.073
Bad Gastein, . .	Aug. 26.	12 11	100	238.89	— 7.0	10	100	14.15	237.22	1.090
....	...	12 31	100	238.94	— 7.0	10	110	13.65	237.30	1.090

† α Indicates the initial semi-arc of vibration; *m* the number of vibrations required to reduce it to half its amount.

(*a*) These observations for Brussels, viz. .963, .959, .960, .959, give a mean of .9602
My observations in 1832, viz. .959, .960, .965, ———— .9613
Observations by M. Quetelet, viz. .958, .970, .969, 961, ———— .9645
———— Major Sabine, viz. .951, .962, .959, ———— .9573
———— M. Rudberg, viz. .971, ... ———— .971
———— Prof. Bache, viz. .970, ... ———— .970
———— M. Duperrey, viz. .963, .960, . . . ———— .9615

Mean of the whole, . . .9637

(*b*) Evidently shew the effect of the volcanic nature of the soil (trachyte).
(*c*) This observation by Professor Gauss. In all the Göttingen observations, a box chronometer belonging to Prof. Gauss was used.
(*d*) Steel watch-chain not removed. Second observation best. (*e*) Most unexceptionable.

TABLE I.—(*continued.*)

CYLINDER No. I—(*continued.*)

PLACE.	Date.	Mean Time.	No. of Vibrations observed.	Observed Time.	Rate Chronometer.	Arc. a.	Arc. m.	Temp. Reaum.	Corrected Time 100 Vibrations.	Intensity Paris = 1.000.
	1837,	h m		s	s	°	°	°	s	
Windisch Matrei, Tyrol, . . .	Aug. 30. (a)	3 45	100	239.60	—7.0	10	85	16.4	237.71	1.086
... .	... (a)	3 55	100	239.87	—7.0	10	90	15.9	238.02	1.083
Inspruck, . . .	Sept. 4.	10 51	100	240.54	—7.0	10	80	16.85	238.61	1.078
....	...	11 6	100	240.67	—7.0	10	85	17.3	238.68	1.077
Bormio, ...	Sept. 8.	5 29	100	239.30	—7.0	10	85?	14.75	237.58	1.087
....	...	5 39	100	239.10	—7.0	10	85	13.95	237.47	1.088
Trent, ...	Sept. 12.	9 41	100	237.70	—7.0	10	90	16.6	235.79	1.104
Laybach, ...	Sept. 20.	3 27	100	235.11	—7.0	10	100	16.8	233.19	1.128
....	... (b)	3 36	100	235.27	—7.0	10	100	17.0	233.34	1.127
Vienna, ...	Oct. 3. (c)	11 17	100	239.63	—6.0	10	105	10.95	238.30	1.080
Ratisbon, ...	Oct. 8.	11 12	100	244.43	—6.0	10	90	12.7	242.90	1.040
....	...	11 22	100	244.33	—6.0	10	90	12.7	242.80	1.041
	1838,									
Edinburgh, . . .	May 10.	12 45	100	272.28	...	10	90	10.45	270.83	.836
....	...	1 1	100	272.01	...	10	90	9.45	270.68	.834

(a) A large iron key in the pocket. Second obs. best. (b) Best. (c) Farther observations prevented by rain.

GENERAL NOTE.—This Table contains the whole results, none having been rejected. In a very few cases, single values of the time of 100 vibrations have been rejected when they differed much from the remaining ones in the same set, as inevitable errors of observation.

63. In the two following tables are contained the observations with the six-inch dipping needle above described. I have been careful to give the separate results with the needles magnetized in either direction, because they afford some estimate of the confidence due to the observations. Some of these varying differences do not appear to be errors of observation, but rather due to an accidental displacement of the centre of gravity, or to the inequality of magnetization with reversed poles.

TABLE II.

6-INCH DIPPING-NEEDLE, A. 1.

PLACE.	Date.	Hour.	Marked End. N. Pole.	Marked End. S. Pole.	Difference.	Mean or Dip.
	1838,		° ′	° ′	° ′	° ′
Edinburgh, Canaan Park, . . .	April 28.	3 P. M.	71 21.5	72 34.1	1 12.6	71 57.8
Greenwich Observatory, . . .	May 5.	2 P. M.	68 33.6	69 51.2	1 17.6	69 12.4
Brussels Observatory, ...	May 10.	1 P. M.	67 55.0	69 19.4	1 24.4	68 37.2
....	...	7 P. M.	67 53.1	69 18.1	1 25.0	68 35.6
Bonn Botanic Garden, ...	May 23.	......	67 3.1	68 29.6	1 26.5	67 46.3
....	... (a)	......	67 5.6	68 31.8	1 26.2	67 48.7
....	June 7.	......	67 11.1	68 27.5	1 16.4	67 49.3
....	June 16.	3 P. M.	67 11.7	68 32.5	1 20.8	67 52.1
Göttingen Observatory, . . .	July 1.	12–1 P. M.	67 15.6	68 18.4	1 2.8	67 47.0
Berlin Magnetic Observatory, .	July 12.	5 P. M.	67 23.8	68 40.0	1 16.2	68 1.9
Carlsbad, ...	Aug. 9.	12–1 P. M.	66 4.0	67 7.9	1 3.9	66 36.0
Vienna, ...	Oct. 3.	10–11 A. M.	63 59.6	65 22.9	1 23.3	64 41.2
	1839,					
Edinburgh Botanic Garden, . .	April 19.	1 P. M.	71 39.5	72 11.2	0 31.7 (b)	71 55.4

(a) Observed by Mr Batten. (b) Between the former observation and this one, the instrument had passed through the maker's hands, and the centre of gravity of the needle A, 1. had been adjusted.

Table III.

6-INCH DIPPING-NEEDLE, A. 2.

Place.	Date.	Hour.	Marked End. N. Pole.	Marked End. S. Pole.	Difference.	Mean or Dip.
	1838,		° ′	° ′	′	° ′
Edinburgh, Canaan Park, . . .	Apr. 28.	4 P. M.	72 0.8	71 49.4	—11.4	71 55.1
Greenwich Observatory, . . .	May 5.	3 P. M.	69 11.8	69 11.2	— 0.6	69 11.5
Brussels Observatory, ...	May 10.	1 P. M.	68 27.1	68 29.9	— 2.8	68 28.5
Bonn Botanic Garden, ...	May 23.	......	67 51.7	67 48.3	— 3.4	67 50.0
.. ..	...... (a)	......	67 52.3	67 50.0	— 2.3	67 51.1
....	June 7.	4 P. M.	67 55.2	67 48.4	— 6.8	67 51.8
....	June 16.	3–4 P. M.	67 53.0	67 51.4	— 1.4	67 52.2
Göttingen Observatory, . . .	July 1.	12	67 56.2	67 50.8	— 5.4	67 53.5
Berlin Magnetic Observatory, .	July 12.	3–4 P. M.	68 8.3	68 2.8	— 5.5	68 5.5
Carlsbad, ...	Aug. 9.	11–12 A. M.	66 55.5	66 41.1	—14.4	66 48.3
....	......	12	66 40.8	66 40.6	— 0.2	66 40.7
Linz, ...	Aug. 15. (b)	10 A. M.	65 12.5	65 18.3	— 5.8	65 15.4
Salzburg, ...	Aug. 21.	12	65 5.2	65 1.8	— 3.4	65 3.5
Inspruck, ...	Sept. 4. (c)	10 A. M.	64 48.0	64 49.4	+ 1.4	64 48.7
Trent, ...	Sept. 12.	9 A. M.	64 5.4	64 5.6	+ 0.2	64 5.5
Laybach, ...	Sept. 20.	2 P. M.	63 26.5	63 17.1	— 9.4	63 21.8
....	......	3 P. M.	63 28.1	63 22.7	— 5.4	63 25.4
Vienna Botanic Garden, . . .	Oct. 3. d)	10 A. M.	64 53.0	64 49.1	— 3.9	64 51.0
Ratisbon Botanic Garden, . . .	Oct. 8.	9 A. M.	66 0.8	65 38.6	—22.2(e)	65 49.7
....	......	10 A. M.	65 58.6	65 51.4	— 7.2	65 55.0
	1839,					
Edinburgh Botanic Garden, . .	Apr. 19.	1–2 P. M.	71 56.0	71 55.1	— 0.9	71 55.5

(a) Observed by Mr Batten.
(b) Hasty observation. Instrument inconveniently placed on a rock overhanging the Danube, above the town of Linz.
(c) Good observation. The principal level being broke in this and the remaining observations of the year, the instrument was levelled by the spare level laid on the agate planes, which were previously known to be in very good adjustment.
(d) Very good observation.
(e) This suspicious result is probably owing to a looseness discovered in the vertical axis, which deranged by starts the levelling of the instrument.

64. It is easy to see that of these results, those with the needle A. 2. are most worthy of confidence, and, after this clearly appeared, the observations were made with that one almost exclusively. In the reductions presently to be given, I shall in all cases adopt the results given by that needle.

65. The principal reason of the superiority of the needle A. 2. is probably the greater accuracy of adjustment of its centre of gravity. It is well known the varying force of magnetization in opposite directions *necessarily* produces an error besides the accidental ones, owing to errors likely to occur in a needle evidently less carefully adjusted than the other. The repeated observations at Bonn on different days with both needles, were made solely with a view of determining the degree of accuracy which the instrument was capable of attaining, and they must be owned to be very satisfactory. Needle A. 2. gave:

	° ′
23d May . . .	67 50.0
....	67 51.1
7th June . . .	67 51.8
16th June . . .	67 52.2
Mean,	67 51.3
Greatest deviatio n from mean,	1.3

66. Besides this there are two coincidences, with results by other observers with different instruments, proving similar accuracy, although I was in both cases unaware of the independent data, until my own observations had been made and calculated.

The dip at Brussels in the end of March 1837, was determined by M. QUETELET,	68° 28′.8
By needle A. 2. on the 10th May 1837,	68 28.5
Difference,	0.3

The dip at Berlin, 20th June 1837, was determined by M. ENCKE,	68° 4′.9
By needle A. 2. on the 12th July 1837,	68 5.5
Difference,	0.6

These were the *only* direct comparisons which I have had an opportunity of making.

67. The following table contains a notice of the particular stations at which the observations were made, their geographical positions, and the Total Intensities deduced from mean results.

TABLE IV.

PLACE.	Particular Situation.	Lat. N.	Long. from Paris.	Mean Horizontal Intensity, Paris = 1.	Mean Dip Needle A 2.	Total Intensity Equator = 1, Paris = 1.3482.
Edinburgh,	Canaan Park,	55° 57′	5° 30′ W.	.840	71° 55.1	1.4089
Greenwich,	SW. corner of drying-ground, S. of the Royal Observatory,	51 29	2 20 W.	.938	69 11.5	1.3750
Brussels,	Observatory. Garden S. of the building,	50 51	2 1 E.	.9602	68 28.5	1.3611
Bonn,	Botanic Garden at Popplesdorf, N. side,	50 44	4 45 E.	.9793	67 51.3	1.3533
Drachenfels,	In a quarry immediately below the Castle, on the top,	50 40	4 50 E.	.9387	......	......
Göttingen,	Prof. Gauss's garden, attached to the Observatory,	51 32	7 36 E.	.9777	67 53.5	1.3535
Berlin,	*Intensity*. 20 yards in front of the door of the Astronomical Observatory, *Dip*. Prof. Encke's Magnetic Observatory,	52 30	11 3 E.	.9733	68 5.5	1.3579
Dresden,	A wood-yard near the Elbe, opposite to the Japanese Palace,	51 4	11 24 E.	1.0007	......	......
Carlsbad,	*Intensity*. A wooded hill above the town. Right bank of the Tepel. On granite. *Dip*. Gartenthal W. side of Tepel, below the town. Granite,	50 13	10 34 E.	1.0205	64 40.7	1.3423
Linz,	S. bank of the Danube, a little above the town. Granite,	48 19	11 56 E.	1.066	65 15.4	1.3268
Ischl,	Church-hill W. of town. Limestone,	47 44	11 17 E.	1.0785	......	......
Salzburg,	Wooded hill N. of town,	47 48	10 42 E.	1.073	65 3.5	1.3250
Bad Gastein,	On a rising ground N. of village. Gneiss,	47 7	10 48 E.	1.090	......	......
Windisch Matrei,	4 miles W. of town. Limestone and slate,	47 1	10 13 E.	1.0845	......	......
Inspruck,	Botanic Garden. Limestone and slate,	46 16	9 4 E.	1.0775	64 48.7	1.3179
Bormio,	Baths of San Martino. A few hundred yards up the valley from the New Baths. Limestone,	46 28	7 55 E.	1.0875	......	
Trent,	In a garden just within the S. wall of the town,	46 4	8 45 E.	1.104	64 5.5	1.3149
Laybach,	Garden behind the Post-house,	46 2	12 26 E.	1.1275	63 23.6	1.3108
Vienna,	Botanic Garden. In the wood behind Baron Jacquin's house,	48 13	14 3 E.	1.080	64 51.0	1.3236
Ratisbon,	Botan. Garden, 50 yards W. gardener's house,	49 1	9 46 E.	1.0405	65 52.3	1.3250

68. Exactly as in art. 30. of my former paper, I proceeded to calculate the direction of the lines of equal horizontal intensity, and those of equal dip, in the eastern part of the Alps, taking as a basis the observations contained in the preceding tables from Linz to Vienna inclusively.

69. Assuming Bormio (the most western) as a normal station for intensity, and denoting, as in art. 28, by a and b, the co-ordinates of latitude and longitude, expressed in *minutes* of a degree for any other station compared to Bormio; denoting also by I the observed variation of intensity compared to Bormio, and by $\delta I'$ the correction applicable to the observed intensity there; the form of equation to the isodynamic (horizontal intensity) line is,

$$a_{,}x + b_{,}y + \delta I' = I,$$

x being the variation due to 1′ of latitude, N increasing; y that due to 1′ of longitude, E increasing.

70. The following equations of condition were deduced from the last table.

Equations of Condition for Lines of Equal Horizontal Intensity in the Eastern Alpine Group.

Linz,	$111x$	$+241y$	$+\delta I'$	$=-$	.0215
Ischl,	$76x$	$+202y$	$+\delta I'$	$=-$	.009
Salzburg,	$80x$	$+167y$	$+\delta I'$	$=-$	.0145
Gastein,	$39x$	$+173y$	$+\delta I'$	$=+$	.0025
W. Matrei,	$33x$	$+138y$	$+\delta I'$	$=-$	.003
Inspruck,	$48x$	$+69y$	$+\delta I'$	$=-$	.010
Bormio,	$0x$	$+0y$	$+\delta I'$	$=$	0
Trent,	$-24x$	$+50y$	$+\delta I'$	$=+$	.0165
Laybach,	$-26x$	$+331y$	$+\delta I'$	$=+$	.040
Vienna,	$105x$	$+368y$	$+\delta I'$	$=-$	.0075

71. These equations having being treated by the method of least squares, the following values of x, y, and $\delta I'$ were determined by Mr JOHN A. BROUN:

$x =$ Variation of Horizontal Intensity for 1′ of Latitude N increasing $-$ .000386
$y =$ ———————— 1′ of Longitude E increasing $+$ .0000864
$\delta I' =$ Correction applicable to Intensity at Bormio, . . $+$ .00138

In the former Western Alpine series (art. 31.), we had for the same needle No. 1,

$$x = -.000364$$
$$y = +.000055$$

a satisfactory coincidence. The length of a minute of longitude is .68 of a minute of latitude in the Eastern Alps. Hence the first of the above values of y becomes for a geographical mile of longitude + .000126, and the angle towards the

east, made by the Isodynamical Lines (of horizontal intensity) with the meridian would be,

$$\text{Arc whose tan } \frac{386}{126} = 71° \, 55'.$$

72. If we now assume the values just found for x and y, and *compute* the horizontal intensities at the preceding stations, we have, I apprehend, very satisfactory evidence of the consistency and accuracy of these observations.

Horizontal Intensity.

Place.	Observed.	Calculated.*	Difference.
Linz,	1.066	1.0669	+ .0009
Ischl,	1.0785	1.0770	— .0015
Salzburg,	1.073	1.0725	— .0005
Gastein,	1.090	1.0888	— .0012
W. Matrei,	1.0845	1.0881	+ .0036
Inspruck,	1.0775	1.0763	— .0012
Bormio,	1.0875	1.0889	+ .0014
Trent,	1.104	1.1025	— .0015
Laybach,	1.1275	1.1275	.0000
Vienna,	1.080	1.0802	+ .0002

The mean. difference *without regard to sign* is .0012; and were the observation at Windisch Matrei omitted (when a large door-key had inadvertently been left in the pocket), the error would have been very much less indeed.

73. I have proceeded in a similar way with regard to the small number of observations on dip, which may serve in a general way to indicate the direction of the Isoclinal Lines in the same region. The symbols have the same relative signification as before. Inspruck is here taken for the starting point.

Equations of Condition for Lines of Equal Dip in the Eastern Alpine Group.

Linz, ...	$63\,x + 172 + \delta\Delta' = 26'.7$
Salzburg, ...	$32\,x + 98 + \delta\Delta' = 14'.8$
Inspruck, ...	$0\,x + 0 + \delta\Delta' = 0$
Trent, ...	$-72\,x - 19 + \delta\Delta' = -43'.2$
Laybach, ...	$-74\,x + 262 + \delta\Delta' = -85'.1$
Vienna, ...	$57\,x + 299 + \delta\Delta' = 2'.3$

From these equations Mr Broun has also deduced, by the method of least squares, the following values:

x = Variation of Dip for 1′ of Latitude N increasing, $+ 0'.7114$
y = Variation of Dip for 1′ of Longitude E increasing, $- 0'.13655$
$\delta\Delta'$ = Correction applicable to Dip at Inspruck, $+ 3'.685$

The variation of y for one geographical mile is $0'.20$, and the angle towards the

* From the formula,

$$-.000386\,x + .0000864\,y + 1.0889.$$

east, of the Isoclinal lines with the meridian is,

$$\text{Arc whose tan } \frac{71}{20} = 74^\circ.3$$

Comparing the observations with the formula,

$$0'.7114\,x - 0'.13655\,y + 64^\circ\ 52'.4,$$

we have the following results:

Place.	Dip observed.	Calculated.	Difference.
	° ′	° ′	′
Linz,	65 15.4	65 13.7	— 1.7
Salzburg,	65 3.5	65 1.8	— 1.7
Inspruck,	64 48.7	64 52.4	+ 3.7
Trent,	64 5.5	64 3.8	— 1.7
Laybach,	63 23.6	63 24.0	+ 0.4
Vienna,	64 51.0	64 52.1	+ 1.1

Mean error (without regard to sign) 1′.7.

III.—*On the Plane and Angle of Polarization of Light Reflected at the surface of a Crystal.* *By* The Rev. P. KELLAND, *A.M., F.R.SS. L. & E., late Fellow of Queen's College, Cambridge; Professor of Mathematics, &c. in the University of Edinburgh.*

(Read 7th December 1840.)

THE present Memoir is, to a certain extent, a continuation of one which the author presented to the Society in December 1838, and which has since been published in the thirteenth volume of the Transactions. Other motives, however, than the desire of completing the subject, have influenced him in producing the following analysis. A very important point in the hypothetical conditions which FRESNEL assumed to hold with respect to polarized light, has, of late, been warmly combated in various quarters. FRESNEL supposed that light polarized in a given plane consists of vibrations of such a nature that the motion is perpendicular to that plane. NEUMANN and other writers contend that the very opposite is the fact. We hope to be able to offer evidence of some little weight in favour of the former view; at the same time we do not pretend to shew the actual impossibility of the truth of the latter.

Our limits will admit only of a very slight sketch of the history of the theory of Reflexion. In connection with the experimental discovery of the laws of crystalline reflexion, we have only to mention the names of BREWSTER and SEEBECK, and to refer to their papers.*

Dr T. YOUNG gave formulæ which represent the intensity of light reflected directly at a non-crystallized surface.† This demonstration was amended by POISSON.‡ Next, FRESNEL gave his attention to the problem in two memoirs which appear in the Annales de Chimie.§ His solution is based on the following hypotheses:—1. The vibrations of polarized light are transversal and perpendicular to the plane of polarization. 2. The density of the ether within a refracting medium is greater than that without. 3. The law of *vis viva* holds good. 4. The resolved parts of the motion without the crystal are the same, parallel to the common surface of separation, as those within. The first of these hypotheses is, in part, different from that of YOUNG,‖ and has been attacked by BLANCHET,

* Brewster, Ph. Tr. 1819, p. 145. Report of Brit. Ass. vol. vi., Trans. of Sect. p. 13. Seebeck, Annalen der Physik, vol. xxi. pp. 309, 289, vol. xxii. p. 126, and vol. xl. p. 462.

† Encyc. Brit. art. Chromatics.

‡ See Annales de Chimie, vol. xvii. p. 189, and 1815.

§ Vol. xvii. pp. 191, 312; also vol. xlvi. p. 225.

‖ Whewell, Hist. of the Ind. Sc. vol. ii. p. 417.

CAUCHY,* and NEUMANN,† all of whom have arrived at the opposite conclusion, that light polarized in a certain plane consists of vibrations *in* that plane. CAUCHY and NEUMANN likewise make the density invariable.‡ Mr M'CULLAGH, in some very ingenious papers on crystalline reflexion,§ has adopted the same view; and M. NEUMANN|| has proceeded to the investigation of the same subject in a most elaborate and valuable memoir. M. SEEBECK¶ has also written on the theoretical expression of the laws of crystalline reflexion. On the other hand Mr GREEN** adopted FRESNEL's views, and endeavoured to establish his equations by mechanical reasoning founded on LAGRANGE's†† equation. His great merit consists in the introduction of a function‡‡ due to the sudden transition which the vibrations experience from a motion in one direction to a motion in another, at the common surface of two media. The author of this memoir applied the equations of motion of a system of unconnected particles to the solution of the same problem.§§ In that memoir he availed himself of Mr GREEN's hypothesis, that a function is destroyed by the effect of the surface. M. CAUCHY, although he held different views in 1830, adopted nearly all of FRESNEL's hypothesis in 1836.|||| Then and subsequently he held that common light may be conceived to consist of two rays polarized in planes at right angles to each other;¶¶ and that the vibrations which constitute light polarized in a certain plane, are at right angles to that plane.*** Until very lately, he appears to have supposed that no motion is destroyed at the common surface, as well as that all the motion is of the same kind.††† His present views appear to differ considerably from those just stated. In various recent memoirs he has proceeded on a new principle.‡‡‡ In one of these,§§§ he lays down the law which regulates the changes of motion at the common surface of two media, in terms which differ little from Mr GREEN's. This law (*cette loi remarquable*)|||||| he applies to uncrystallized media,¶¶¶ and proposes to continue his investigations. He has also given a theory of metallic reflexion,****

* Mem. de l'Acad. des Sci. vol. x. p. 304.

† Annalen der Physik, vol. xxv. p. 418, &c. See also Navier, Mem. de l'Acad. 1824.

‡ Memoirs quoted, and Bulletin des Sci. Matth. Juillet, 1830. Compare Comptes Rendus, Av. 4. 1836.

§ Philos. Mag. vol. vii. p. 295; vol. viii. p. 103; vol. x. p. 43. L'Institut. vol. v. p. 223. Trans. of the Royal Irish Ac. vol. xiii. p. 11, for 1837.

|| Abhandlungen der Akad. zu Berlin, vol. xxii. p. 1, für 1835.

¶ Annalen der Physik.

** Transactions of the Cambridge Ph. Soc., vol. vii. pp. 1 and 113.

†† Mécan. Anal. Dyn. Sect., 2. Art. 5.

‡‡ Trans. Camb. Phil. Soc., vol. vii. p. 20.

§§ Trans. of the Royal Society of Edinburgh, vol. xiii. p. 393.

|||| Comptes Rendus, Avril 1836. See also Nouveaux Exercises de Mathematiques, 7e livraison.

¶¶ Ibid. vol. viii. pp. 10 and 115.

*** Ibid. p. 116.

††† Ibid. vol. ix. p. 676, Dec. 1838. See also vol. viii. pp. 40, 43.

‡‡‡ Ibid. pp. 374, 432, 459; March 1839, &c.

§§§ Ibid. vol. x. p. 273, Feb. 1840.

|||||| Cauchy, Comptes Rendus, vol. x. p. 273, for February 1840.

¶¶¶ Ib. vol. x. pp. 350, 359; 2d May 1840.

**** Ib. vol. viii. p. 553.

as Mr M'Cullagh had previously done.* We have to add that Mr M'Cullagh, whose investigations had previously rested on equations assumed from analogy, has recently taken up the mechanical solution of the problem of crystalline reflexion.†

Such is a very brief outline of the present state of theory on this branch of Physical Optics. From the extreme difficulty attendant even on the conception of *pressure* as applied to transversal vibrations, it has appeared to the author desirable to reduce all theory ultimately to the action of force. Having been once irresistibly led to the belief that the *Newtonian Law* ‡ is the true law of molecular action, he has not hesitated to adopt it in all other cases.

SECTION I.—DETERMINATION OF THE EQUATIONS OF CONDITION AT THE COMMON SURFACE OF TWO MEDIA.

To understand fully the following process, it is desirable that the reader should examine our preceding Memoir *On Fresnel's Formulæ*, in vol. xiii. We may remark, that the hypotheses are, 1. That the vibrations are transversal and perpendicular to the plane of polarization. 2. That at the surface a portion of the motion is changed in form. 3. That the vibrations are isochronous. 4. That the action of one medium on a particle at rest is the same as that of any other which may supply its place. The difference between different media is sufficiently defined by means of the different directions which a ray of light takes in passing into them.

To determine the values of the disturbances without and within the crystal. Let YOZ (see next page) be a portion of the surface of the crystal, OX the normal to the surface at the point of incidence under consideration; and let OX, OY, OZ be respectively the axes of x, y, and z: x being measured *downwards*. Conceive a sphere to be described about the point O, and let I be the point in which the incident pencil corresponding with the point O cuts its surface.

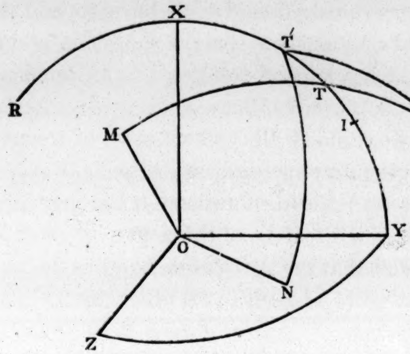
Let also T, T′ be the points in which the normals to two refracted waves passing through O and produced backwards cut the same surface; these points will lie all in the plane XOY.

The vibration of the incident pencil may be resolved into two parts, one *in* the plane XOY, and the other perpendicular to it; call these I and I′ respectively. Let A be the position of the axis as referred to the sphere: *i. e.* let OA be parallel to the direction of the axis of the crystal: then the vibration of one wave will be

* Phil. Mag., vol. x. p. 382, &c. Trans. of the Royal Irish Ac., vol. xiii. p. 71. See also Comptes Rendus, vol. viii. pp. 961, 964.

† Proceedings of the Royal Irish Ac., Dec. 9, 1839, p. 375.

‡ Trans. of the Camb. Phil. Soc., vol. vi. p. 178.



in the plane AT, and of the other perpendicular to the plane AT′. Denote the vibration in the plane AT by T, and that perpendicular to AT′ by T′. Also let R, R′ be the resolved parts of the reflected vibration, in and perpendicular to the plane XOY respectively. Denote further by $R_,$ $T_,$ the reflected and transmitted vibratory motion put in play at the surface, whose type does not contain x.

Let $XI=\phi$, $XT=\phi_,$, $XT'=\phi'$, and call α, β, γ the resolved parts of the vibrations *without* the crystal, and $\alpha_,$ $\beta_,$ $\gamma_,$ those *within*.

Conceive that at the same instant and for the same point, I tends downwards, R upwards, I′ outwards, and R′ inwards, and the following will be the resolved parts of the vibrations without the crystal.

$$\begin{aligned} \alpha &= (I-R)\sin\phi + R_, \\ \beta &= (I+R)\cos\phi \\ \gamma &= I'-R'. \end{aligned} \qquad (1.)$$

Again, if we denote ATY by θ, and AT′Y by θ', we may resolve the vibrations T and T′ parallel to the directions of x, y, and z, thus:

Draw OM perpendicular to OT in the plane AT, and ON perpendicular to OT and to the plane OT′: then turning the figure round OZ through 180° to bring it into its proper position, or (which is the same thing) changing the direction of y, we get

$$\begin{aligned} \alpha_, &= T\cos MX + T'\cos NX + T_, \\ \beta_, &= -T\cos MY - T'\cos NY \\ \gamma_, &= T\cos MZ + T'\cos NZ. \end{aligned}$$

But $\cos MX = \sin\phi_,\cos\theta$, $\cos MY = -\cos\phi_,\cos\theta$, $\cos MZ = \sin\theta$,

$\cos NX = -\sin\phi'\sin\theta'$, $\cos NY = \cos\phi'\sin\theta'$, $\cos NZ = \cos\theta'$.

Therefore, by substitution,

$$\begin{aligned} \alpha_, &= T\sin\phi_,\cos\theta - T'\sin\phi'\sin\theta' + T_, \\ \beta_, &= T\cos\phi_,\cos\theta - T'\cos\phi'\sin\theta' \\ \gamma_, &= T\sin\theta + T'\cos\theta'. \end{aligned} \qquad (2.)$$

It may be remarked that the second vibration may, in each direction, be obtained from the first by writing ϕ' for $\phi_,$ and $90+\theta'$ for θ.

We proceed next to find the variations in α, β, γ, $\alpha_,$, $\beta_,$, $\gamma_,$ due to a variation in the co-ordinates x, y, z. Let $\alpha+\delta\alpha$, &c. be the values of α and of the other quantities, when $x+\delta x$, $y+\delta y$, $z+\delta z$ are the values of the co-ordinates.

Now $$\delta\alpha=(\delta\mathrm{I}-\delta\mathrm{R})\sin\phi+\delta\mathrm{R}_,\ \&\text{c.}=\&\text{c.}$$

Therefore we must commence by finding the values of $\delta\mathrm{I}$, $\delta\mathrm{R}$, and $\delta\mathrm{R}_,$.

Now if p, p', p'', p''' be the perpendiculars from the origin (supposed above the plane of yz and of each of the waves) on the incident, reflected and two refracted waves, it is evident that

$$\begin{aligned}
p &= x\cos\phi+y\sin\phi+\text{const.}\\
p' &= x'\cos\phi-y'\sin\phi+\text{const.}\\
p'' &= x''\cos\phi_,+y''\sin\phi_,+\text{const.}\\
p''' &= x'''\cos\phi'+y'''\sin\phi'+\text{const.}
\end{aligned}$$

and therefore

$$\begin{aligned}
\mathrm{I} &= a\cos\left(\frac{2\pi}{\lambda}p+\text{ct.}\right)\\
&= a\cos\left(\frac{2\pi}{\lambda}.\overline{x\cos\phi+y\sin\phi}+\text{ct}+\text{const.}\right)\\
\mathrm{R} &= b\cos\left(-\frac{2\pi}{\lambda}p'+\text{ct}+\text{const.}\right)\\
&= b\cos\left(-\frac{2\pi}{\lambda}x'\cos\phi+\frac{2\pi}{\lambda}y'\sin\phi+\text{ct}+\text{const.}\right)\\
\mathrm{T} &= c\cos\left(\frac{2\pi}{\lambda_,}.\overline{x''\cos\phi_,+y''\sin\phi_,}+\text{ct}+\text{const.}\right)\\
\mathrm{T}' &= c\cos\left(\frac{2\pi}{\lambda'}.\overline{x'''\cos\phi'+y'''\sin\phi'}+\text{ct}+\text{const.}\right)
\end{aligned}$$

where it is to be observed that, as the reflected pencil moves in the opposite direction to the incident, the sign of p' within the circular function is negative.

We will abbreviate these equations by denoting $\frac{2\pi}{\lambda}\cos\phi$ by e, $\frac{2\pi}{\lambda}\sin\phi$ by f, &c., and suppressing the accents to the co-ordinates; thus we shall have

$$\begin{aligned}
&\mathrm{I}=a\cos(ex+fy+\text{ct}),\ \mathrm{I}'=a'\cos(ex+fy+\text{ct}),\\
&\mathrm{R}=b\cos(-ex+fy+\text{ct}+h),\ \mathrm{R}'=b'\cos(-ex+fy+\text{ct}+k), \qquad (3.)\\
&\mathrm{R}_,=\mathrm{A}e^{-mx}\cos(fy+\text{ct}+l),\ \mathrm{T}=c\cos(e_,x+fy+\text{ct}+m),\\
&\mathrm{T}'=c'\cos(e'x+fy+\text{ct}+n),\ \mathrm{T}_,=\mathrm{C}e^{-mx}\cos(fy+\text{ct}+p).
\end{aligned}$$

It must be remarked that f is the same in all the equations, since $\frac{\sin\phi}{\lambda}=\frac{\sin\phi_,}{\lambda_,}=\frac{\sin\phi'}{\lambda'}$, from the circumstance that $\frac{\lambda_,}{\lambda}=\frac{v_,}{v}=\frac{\sin\phi_,}{\sin\phi}$.

Now
$$\begin{aligned}
\delta\mathrm{I} &= a\cos(ex+e\delta x+fy+f\delta y+\text{ct})-a\cos(ex+fy+\text{ct})\\
&= -\mathrm{I}(1-\cos\overline{e\delta x+f\delta y})-a\sin(ex+fy+\text{ct})\sin(e\delta x+f\delta y)\\
&= -2\mathrm{I}\sin^2\frac{e\delta x+f\delta y}{2}+\frac{1}{e}\frac{d\mathrm{I}}{dx}\sin(e\delta x+f\delta y);
\end{aligned}$$

and in the same way the other increments are easily determined.

Let us abbreviate $e\delta x+f\delta y$ by ki, $e\delta x-f\delta y$ by kr, $e_{,}\delta x+f\delta y$ by $k_{,}e$, and $e'\delta x+f\delta y$ by $k'o$; then we have the following values of δ I, &c.

$$\delta \mathrm{I}=-2\,\mathrm{I}\sin^2\frac{ki}{2}+\frac{1}{e}\frac{d\mathrm{I}}{dx}\sin ki$$

$$\delta \mathrm{I}'=-2\,\mathrm{I}'\sin^2\frac{ki}{2}+\frac{1}{e}\frac{d\mathrm{I}'}{dx}\sin ki$$

$$\delta \mathrm{R}=-2\,\mathrm{R}\sin^2\frac{kr}{2}+\frac{1}{e}\frac{d\mathrm{R}}{dx}\sin kr$$

$$\delta \mathrm{R}'=-2\,\mathrm{R}'\sin^2\frac{kr}{2}+\frac{1}{e}\frac{d\mathrm{R}'}{dx}\sin kr$$

$$\delta \mathrm{R}_{,}=-\mathrm{R}_{,}(1-e^{-m\delta x}\cos f\delta y)+\frac{1}{f}\frac{d\mathrm{R}_{,}}{dy}e^{-m\delta x}\sin f\delta y$$

$$\delta \mathrm{T}=-2\,\mathrm{T}\sin^2\frac{k_{,}e}{2}+\frac{1}{e_{,}}\frac{d\mathrm{T}}{dx}\sin k_{,}e$$

$$\delta \mathrm{T}'=-2\,\mathrm{T}'\sin^2\frac{k'o}{2}+\frac{1}{e'}\frac{d\mathrm{T}'}{dx}\sin k'o$$

$$\delta \mathrm{T}_{,}=-\mathrm{T}_{,}(1-e^{-m_{,}\delta x}\cos f\delta y)+\frac{1}{f}\frac{d\mathrm{T}_{,}}{dy}e^{-m_{,}\delta x}\sin f\delta y$$

By substituting these results in the increments of equations (1) and (2), we shall obtain

$$\delta a=-2\,\mathrm{I}\sin\phi\sin^2\frac{ki}{2}+2\,\mathrm{R}\sin\phi\sin^2\frac{kr}{2}+\frac{1}{e}\frac{d\mathrm{I}}{dx}\sin\phi\sin ki$$
$$-\frac{1}{e}\frac{d\mathrm{R}}{dx}\sin\phi\sin kr-\mathrm{R}_{,}(1-e^{-m\delta x}\cos f\delta y)+\frac{d\mathrm{R}_{,}}{dy}\frac{1}{f}e^{-m\delta x}\sin f\delta y$$

$$\delta\beta=\left\{-2\,\mathrm{I}\sin^2\frac{ki}{2}-2\,\mathrm{R}\sin^2\frac{kr}{2}+\frac{1}{e}\frac{d\mathrm{I}}{dx}\sin ki+\frac{1}{e}\frac{d\mathrm{R}}{dx}\sin kr\right\}\cos\phi$$

$$\delta\gamma=-2\,\mathrm{I}'\sin^2\frac{ki}{2}+2\,\mathrm{R}'\sin^2\frac{kr}{2}+\frac{1}{e}\frac{d\mathrm{I}'}{dx}\sin ki-\frac{1}{e}\frac{d\mathrm{R}'}{dx}\sin kr$$

$$\delta a_{,}=-2\,\mathrm{T}_{,}\sin\phi_{,}\cos\theta\sin^2\frac{k_{,}e}{2}+2\,\mathrm{T}'\sin\phi'\sin\theta'\sin^2\frac{k'o}{2}+\frac{1}{e_{,}}\frac{d\mathrm{T}}{dx}\sin\phi_{,}\cos\theta\sin k_{,}e$$
$$-\frac{1}{e'}\frac{d\mathrm{T}'}{dx}\sin\phi'\sin\theta'\sin k'o-\mathrm{T}_{,}(1-e^{-m_{,}\delta x}\cos f\delta y)+\frac{1}{f}\frac{d\mathrm{T}_{,}}{dy}e^{-m_{,}\delta x}\sin f\delta y$$

$$\delta\beta_{,}=-2\,\mathrm{T}\cos\phi_{,}\cos\theta\sin^2\frac{k_{,}e}{2}+2\,\mathrm{T}'\cos\phi'\sin\theta'\sin^2\frac{k'o}{2}$$
$$+\frac{1}{e_{,}}\frac{d\mathrm{T}}{dx}\cos\phi_{,}\cos\theta\sin k_{,}e-\frac{1}{e'}\frac{d\mathrm{T}'}{dx}\cos\phi'\sin\theta'\sin k'o$$

$$\delta\gamma_{,}=-2\,\mathrm{T}\sin\theta\sin^2\frac{k_{,}e}{2}-2\,\mathrm{T}'\cos\theta'\sin^2\frac{k'o}{2}$$
$$+\frac{1}{e_{,}}\frac{d\mathrm{T}}{dx}\sin\theta\sin k_{,}e+\frac{1}{e'}\frac{d\mathrm{T}'}{dx}\cos\theta'\sin k'o. \qquad (4.)$$

To obtain the equations of motion of a particle in the outer medium, we adopt the follow notation:—$r\,\phi\,(r)$ represents the force which one particle exerts on another at the distance r; x, y, z are the co-ordinates of the particle under consideration; $x+\delta x$, $y+\delta y$, $z+\delta z$, those of another particle without the crystal; $x+\delta x'$, $y+\delta y'$, $z+\delta z'$, those of a particle within the crystal: r the distance between the particle under consideration and another particle in the upper medium, r' the corresponding distance for a particle in the lower medium; all taken when the system is in a state of rest: $r+\rho$ the value which r acquires at the end of the time t; $r'+\rho'$ the corresponding value of r'; $(\alpha_{,})$, $(\beta_{,})$, $(\gamma_{,})$ are the displacements of a particle within the medium at the time t. Thus, by the usual process (See Memoir on Dispersion in Trans. Camb. Philos. Soc., vol. vi. p. 158), we have, for the force on the particle resolved parallel to the axis of x,

$$\Sigma\,\phi\,(r+\rho)\,(\delta x+\delta\alpha)+\Sigma\,\phi\,(r'+\rho')\,(\delta x'+(\alpha_{,})-\alpha)$$

$$=\Sigma\left(\phi\,r+\phi'\,r\,\frac{\delta x\,\delta\alpha+\delta y\,\delta\beta+\delta z\,\delta\gamma}{r}\right)(\delta x+\delta\alpha)$$

$$+\Sigma\left\{\phi\,r'+\phi'\,r'\,\frac{\delta x'\,\overline{(\alpha_{,})-\alpha}+\delta y'\,\overline{(\beta_{,})-\beta}+\delta z'\,\overline{(\gamma_{,})-\gamma}}{r'}\right\}\delta x'+(\alpha_{,})-\alpha)$$

$$=\Sigma\left\{(\phi\,r+\frac{\phi'\,r}{r}\,\delta x^2)\,\delta\alpha+\frac{\phi'\,r}{r}\,\delta x\,\delta y\,\delta\beta+\frac{\phi'\,r}{r}\,\delta x\,\delta z\,\delta\gamma\right\}$$

$$+\Sigma\left\{(\phi\,r'+\frac{\phi'\,r'}{r'}\,\delta x'^2)\,\overline{(\alpha_{,})-\alpha}+\frac{\phi'\,r'}{r'}\,\delta x'\,\delta y'\,\overline{(\beta_{,})-\beta}+\frac{\phi'\,r'}{r'}\,\delta x'\,\delta z'\,\overline{(\gamma_{,})-\gamma}\right\}.$$

But $(\alpha_{,})=\alpha_{,}+\delta\alpha_{,}$; therefore the force parallel to the axis of x is $\dfrac{d^2\alpha}{dt^2}=$

$$\Sigma\left\{(\phi r+\frac{\phi'\,r}{r}\,\delta x^2)\,\delta\alpha+\frac{\phi'\,r}{r}\,\delta x\,\delta y\,\delta\beta+\frac{\phi'\,r}{r}\,\delta x\,\delta z\,\delta\gamma\right\}$$

$$+\Sigma\left\{(\phi\,r'+\frac{\phi'\,r'}{r'}\,\delta x'^2)\,\delta\alpha_{,}+\frac{\phi'\,r'}{r'}\,\delta x'\,\delta y'\,\delta\beta_{,}+\frac{\phi'\,r'}{r'}\,\delta x'\,\delta z'\,\delta\gamma_{,}\right\}$$

$$+\Sigma\left\{(\phi\,r'+\frac{\phi'\,r'}{r'}\,\delta x'^2)\,(\alpha_{,}-\alpha)+\frac{\phi'\,r'}{r'}\,\delta x'\,\delta y'\,(\beta_{,}-\beta)+\frac{\phi'\,r'}{r'}\,\delta x'\,\delta z'\,(\gamma_{,}-\gamma)\right\} \quad (5.)$$

It may be remarked that we have, in deducing the last equation from the preceding one, suffered ourselves to imagine that $\alpha_{,}$ has a value when the particle is without the medium. Although it can hardly be doubted that the value of $(\alpha_{,})$ so obtained is quite correct, we do not purpose to insist on it, but shall obviate the objection at once by restricting our discussion to the particles situated at the common surface of the two media. The values of Σ are the same on both sides of the surface, each extending over *half* an infinite space, the one upwards, the other downwards.

In order to find the value of the force, or of $\dfrac{d^2\alpha}{dt^2}$, all that remains to be done is to substitute in equation (5) the values of $\delta\alpha$, $\delta\beta$, &c. from equation (4).

The result is

$$\frac{d^2 a}{d t^2}=\Sigma\left(\phi r+\frac{\phi' r}{r}\,\delta x^2\right)\Big\{-2\,\mathrm{I}\sin\phi\sin^2\frac{k i}{2}+2\,\mathrm{R}\sin\phi\sin^2\frac{k r}{2}+\frac{1}{e}\,\frac{d\,\mathrm{I}}{dx}\sin\phi\sin k i$$

$$-\frac{1}{e}\,\frac{d\,\mathrm{R}}{d x}\sin\phi\sin k r-\mathrm{R}_{,}\left(1-e^{-m\delta z}\cos f\,\delta y\right)+\frac{d\,\mathrm{R}_{,}}{d y}\,\frac{1}{f}\,e^{-m\delta z}\sin f\,\delta y\Big\}+\&c. \qquad (6.)$$

This equation, although apparently long and complicated, can, by reason of its symmetry, be reduced to a very simple form.

The reduction is effected by transforming the co-ordinates of each particular part in such a way, that whilst the axis of z always remains unchanged, the other axes shall vary in such a manner that one of them shall be in the direction in which that elementary motion to which it is due is transmitted. Thus all the portions which involve $\delta\,a$, $\delta\,\beta$, $\delta\,\gamma$ will be reduced by changing the co-ordinates to others, one of which is in the direction IO of incidence, and the other perpendicular to it, in the plane of incidence. Again, all the portions which involve δ T will be reduced by changing the co-ordinates to others, one of which is in the direction TO of transmission of this vibration, and the other perpendicular to it, and so on. The effects of this transformation will be twofold; 1°, A considerable portion of the expression will vanish altogether by virtue of the symbol Σ; 2°, Those parts which remain will, by virtue of the hypothesis that all the vibrations occupy *the same time*, be reduced to known forms; or at least the major part of them.

For incident vibrations, let i and p stand for the co-ordinates reckoned *in* and *perpendicular to* the direction of incidence: then

$$\delta\,x=i\cos\phi+p\sin\phi,\quad \delta\,y=-i\sin\phi+p\cos\phi.$$

Now, the principle on which the reduction is carried on is this; after the co-ordinates have been transformed, the values of the expressions are determined by means of the law that in a complete medium extending both above and below the point under consideration, the ratio of $\frac{d^2 a}{d t^2}$ to a is $-c^2$.

But this ratio is evidently the double of the integral $-2\,\Sigma\left(\phi r+\frac{\phi' r}{r}p^2\right)\sin^2\frac{k i}{2}$ whatever direction i may indicate, or $\frac{c^2}{2}=2\,\Sigma\left(\phi r+\frac{\phi' r}{r}p^2\right)\sin^2\frac{k i}{2}$. (7.)

But further, since $r^2=p^2+z^2+i^2$, it follows that $\frac{c^2}{2}=2\,\Sigma\left(\phi r+\frac{\phi' r}{r}z^2\right)\sin^2\frac{k i}{2}$; and also, if we suppose the law of force to be that of Newton, a supposition which has been made by me, and I think established on good grounds in several preceding memoirs, we shall have the following relations:

$$\frac{c^2}{2}=\mathrm{S}\,\Sigma\left(\frac{1}{r^3}-\frac{3\,p^2}{r^5}\right)\sin^2\frac{k i}{2}$$

$$=\mathrm{S}\,\Sigma\,\frac{z^2+i^2-2\,p^2}{r'}\sin^2\frac{k i}{2}=\mathrm{S}\,\Sigma\,\frac{i^2-p^2}{r^5}\sin^2\frac{k i}{2}$$

and $$\frac{\phi' r}{r} = -\frac{3S}{r^5}. \qquad (7).$$

Let us then substitute in equation (6) these different quantities, and we shall have

$$\Sigma\left(\phi r + \frac{\phi' r}{r}\delta x^2\right) 2\sin^2\frac{ki}{2} = \Sigma\left\{\phi r + \frac{\phi' r}{r}(i^2\cos^2\phi + 2ip\sin\phi\cos\phi + p^2\sin^2\phi)\right\} 2\sin^2\frac{ki}{2}$$

$$= \Sigma\left(\phi r + \frac{\phi' r}{r}\overline{i^2\cos^2\phi + p^2\sin^2\phi}\right) 2\sin^2\frac{ki}{2},$$

because $\Sigma\frac{\phi' r}{r} ip\sin^2\frac{ki}{2} = 0$, from the circumstance that every value of i has two equal values of p with opposite signs.

Hence $$\Sigma\left(\phi r + \frac{\phi' r}{r}\delta x^2\right) 2\sin^2\frac{ki}{2} = \Sigma\left(\phi r\,\overline{\cos^2\phi + \sin^2\phi} + \frac{\phi' r}{r}\overline{i^2\cos^2\phi + p^2\sin^2\phi}\right). 2\sin^2\frac{ki}{2}$$

$$= \Sigma\left(\phi r + \frac{\phi' r}{r}p^2\right) 2\sin^2\frac{ki}{2}\sin^2\phi + \Sigma\left(\phi r + \frac{\phi' r}{r}i^2\right) 2\sin^2\frac{ki}{2}\cos^2\phi$$

$$= \frac{c^2}{2}\sin^2\phi + \Sigma\left(\phi r + \frac{\phi' r}{r}i^2\right) 2\sin^2\frac{ki}{2}\cos^2\phi.$$

But $$\Sigma\left(\phi r + \frac{\phi' r}{r}i^2\right) 2\sin^2\frac{ki}{2} = S\Sigma\left(\frac{1}{r^3} - \frac{3i^2}{r^5}\right) 2\sin^2\frac{ki}{2}$$

$$= S\Sigma\frac{2p^2 - 2i^2}{r^5} 2\sin^2\frac{ki}{2} = -2S\Sigma\frac{(i^2 - p^2)}{r^5} 2\sin^2\frac{ki}{2}$$

$$= -\frac{2c^2}{2} \text{ by (7)} = -c^2 \qquad (8).$$

Hence, finally, $$\Sigma\left(\phi r + \frac{\phi' r}{r}\delta x^2\right) 2\sin^2\frac{ki}{2} = \frac{c^2}{2}(\sin^2\phi - 2\cos^2\phi).$$

Similarly, $$\Sigma\left(\phi r + \frac{\phi' r}{r}\delta x^2\right) 2\sin^2\frac{kr}{2} = \frac{c^2}{2}(\sin^2\phi - 2\cos^2\phi)$$

Again, $$\Sigma\left(\phi r + \frac{\phi' r}{r}\delta x^2\right)\sin ki = \Sigma\left(\phi r + \frac{\phi' r}{r}p^2\right)\sin ki\sin^2\phi$$

$$+ \Sigma\left(\phi r + \frac{\phi' r}{r}i^2\right)\sin ki\cos^2\phi$$

and $$\Sigma\left(\phi r + \frac{\phi' r}{r}i^2\right)\sin ki = -2\Sigma\left(\phi r + \frac{\phi' r}{r}p^2\right)\sin ki$$

as equation (8) shews.

Denote this quantity $\Sigma\left(\phi r + \frac{\phi' r}{r}i^2\right)\sin ki$ by M;

then $$\Sigma\left(\phi r + \frac{\phi' r}{r}\delta x^2\right)\sin ki = M(\sin^2\phi - 2\cos^2\phi).$$

similarly $$\Sigma\left(\phi r + \frac{\phi' r}{r}\delta x^2\sin kr\right) = M(\sin^2\phi - 2\cos^2\phi).$$

Denote $\Sigma\,(\phi\,r+\frac{\phi'\,r}{r}\,\delta\,x^2)\,(1-e^{-m\,\delta x}\cos f\,\delta\,y)$ by D; then, by collecting all the terms, we obtain for the value of that part of $\frac{d^2\,a}{d\,t^2}$ which is *written down* in equation (6), and which is the part involving $\delta\,a$

$$\Sigma\,(\phi\,r+\frac{\phi'\,r}{r}\,\delta\,x^2)\,\delta\,a=-(\mathrm{I}-\mathrm{R})\sin\phi\frac{c^2}{2}(\sin^2\phi-2\cos^2\phi)$$

$$+\frac{1}{e}\left(\frac{d\,\mathrm{I}}{d\,x}-\frac{d\mathrm{R}}{d\,x}\right)\sin\phi\,\mathrm{M}\,(\sin^2\phi-2\cos^2\phi)-\mathrm{D}\,\mathrm{R}, \qquad (a.)$$

Having written down the full work of the reduction of this part of the expression, no difficulty will be experienced in following the rest of equation (5). It must be remarked, however, that for the reflected wave

$$\delta\,x=r\cos\phi+p\sin\phi,\quad \delta\,y=r\sin\phi-p\cos\phi\,;$$

for the wave T, within the crystal,

$$\delta\,x'=e\cos\phi+p\sin\phi,\quad \delta\,y'=-e\sin\phi+p\cos\phi\,;$$

and for the wave T′

$$\delta\,x'=o\cos\phi+p\sin\phi,\quad \delta\,y'=-o\sin\phi+p\cos\phi.$$

Hence, referring to equations (4) for the value of $\delta\,\beta$, we get

$$\Sigma\frac{\phi'r}{r}\,\delta\,x\,\delta\,y\,\delta\,\beta=-\Sigma\frac{3\mathrm{S}}{r^5}\Big\{\overline{i\cos\phi+p\sin\phi}\,(-i\sin\phi+p\cos\phi)(-2\,\mathrm{I}\sin^2\frac{k\,i}{2}+\frac{1}{e}\frac{d\,\mathrm{I}}{d\,x}\sin k\,i)$$

$$+\overline{r\cos\phi+p\sin\phi}\,(r\sin\phi-p\cos\phi)(-2\mathrm{R}\sin^2\frac{k\,r}{2}+\frac{1}{e}\frac{d\,\mathrm{R}}{d\,x}\sin k\,r\Big\}\cos\phi$$

$$=-\Sigma\frac{3\,\mathrm{S}}{r^5}\Big\{-(i^2-p^2)\sin\phi\cos\phi\,(-2\,\mathrm{I}\sin^2\frac{k\,i}{2}+\frac{1}{e}\frac{d\mathrm{I}}{d\,x}\sin k\,i)$$

$$+(r^2-p^2)\sin\phi\cos\phi\,(-2\,\mathrm{R}\sin^2\frac{k\,r}{2}+\frac{1}{e}\frac{d\mathrm{R}}{d\,x}\sin k\,r)\Big\}\cos\phi$$

$$=-3\left\{\frac{c^2}{2}(\mathrm{I}-\mathrm{R})-\frac{\mathrm{M}}{e}\left(\frac{d\mathrm{I}}{dx}-\frac{d\mathrm{R}}{d\,x}\right)\right\}\sin\phi\cos^2\phi$$

by virtue of equation (7.) . . . . (*b.*)

$$\Sigma\frac{\phi'\,r}{r}\delta\,x\,\delta\,z\,\delta\,\gamma=0.$$

$\Sigma\,(\phi\,r'+\frac{\phi'\,r'}{r'}\,\delta\,x'^2)\,\delta\,a_{,,}$ can be written down at once from equation (a), from which it differs in no respect save that $\phi_{,}$ and ϕ' occupy the place of ϕ in the incident pencil, and that the coefficients and circular functions are indicated by a different letter. An inspection of the value of $\delta\,a_{,}$ in equation (4), will therefore shew that

$$\Sigma\,(\phi\,r'+\frac{\phi'\,r'}{r'}\,\delta\,x'^2)\,\delta\,a_{,}=-\mathrm{T}\sin\phi_{,}\cos\theta\,\frac{c^2}{2}(\sin\phi_{,}-2\cos^2\phi_{,})$$

$$+\mathrm{T}'\sin\phi'\sin\theta'\,\frac{c^2}{2}(\sin^2\phi_{,}-2\cos^2\phi_{,})+\frac{1}{e_{,}}\frac{d\,\mathrm{T}}{d\,x}\sin\phi_{,}\cos\theta\,\mathrm{M}_{,}(\sin^2\phi_{,}-2\cos^2\phi_{,})$$

$$-\frac{1}{e'}\frac{d\,\mathrm{T}'}{d\,x}\sin\phi'\sin\theta'\,\mathrm{M}'(\sin^2\phi'-2\cos^2\phi')-\mathrm{D}_,\mathrm{T}_, \qquad (c.)$$

The value of $\mathrm{D}_,$ is $\Sigma\left(\phi r'+\frac{\phi' r'}{r'}\delta x'^2\right)(1-e^{-m_,\delta x'}\cos f\,\delta y')$;

and the values of $\mathrm{M}_,$ and M' respectively

$$\Sigma\left(\phi r'+\frac{\phi' r'}{r'}e^2\right)\sin k\,e, \text{ and } \Sigma\left(\phi r'+\frac{\phi' r'}{r'}o^2\right)\sin k\,o.$$

In the same way we can write down the value of $\Sigma\frac{\phi' r'}{r'}\delta x'\,\delta y'\,\delta\beta_,$ from that of $\Sigma\frac{\phi' r}{r}\delta x\,\delta y\,\delta\beta$ or (b).

Thus $\Sigma\frac{\phi' r'}{r'}\delta x'\,\delta y'\,\delta\beta_,=3\sin\phi_,\cos\phi_,\left\{-\frac{c^2}{2}\mathrm{T}\cos\phi_,\cos\theta\right.$

$$\left.+\frac{\mathrm{M}_,}{e_,}\frac{d\,\mathrm{T}}{d\,x}\cos\phi_,\cos\theta\right\}+3\sin\phi'\cos\phi'\left\{\frac{c^2}{2}\mathrm{T}'\cos\phi'\sin\theta'-\frac{\mathrm{M}'}{e'}\frac{d\,\mathrm{T}'}{d\,x}\sin\phi'\sin\theta'\right\}$$

$$=3\left\{-\sin\phi_,\cos^2\phi_,\cos\theta\left(\frac{c^2}{2}\mathrm{T}-\frac{\mathrm{M}_,}{e_,}\frac{d\,\mathrm{T}}{d\,x}\right)+\sin\phi'\cos^2\phi'\sin\theta'\left(\frac{c^2}{2}\mathrm{T}'-\frac{\mathrm{M}'}{e'}\frac{d\,\mathrm{T}'}{d\,x}\right)\right\} \qquad (d.)$$

$\Sigma\frac{\phi' r'}{r'}\delta x'\,\delta z'\,\delta\gamma_,=0.$ Denote $\Sigma\left(\phi r'+\frac{\phi' r'}{r'}\delta x'^2\right)$ by $\mathrm{Q}_{x'}$;

then $$\Sigma\left(\phi r'+\frac{\phi' r'}{r_,}\delta x'^2\right)(\alpha_,-\alpha)=\mathrm{Q}_{x'}(\alpha_,-\alpha) \quad . \quad . \quad . \qquad (e.)$$

$$\Sigma\frac{\phi' r'}{r'}\delta x'\,\delta y'\,(\beta_,-\beta)=0.$$

$$\Sigma\frac{\phi' r'}{r'}\delta x'\,\delta z'\,(\gamma_,-\gamma)=0.$$

The reduction of equation (5), or the value of $\frac{d^2\alpha}{d\,t^2}$, is consequently obtained by adding together the quantities of (a), (b), (c), (d), (e).

Now the sum of (a) and (b) is

$$-\left\{\frac{c^2}{2}\sin\phi\,(\mathrm{I}-\mathrm{R})-\frac{\mathrm{M}}{e}\left(\frac{d\,\mathrm{I}}{d\,x}-\frac{d\,\mathrm{R}}{d\,x}\right)\sin\phi\right\}(\sin^2\phi-2\cos^2\phi+3\cos^2\phi)$$

$$-\mathrm{D}\,\mathrm{R}_,=-\left\{\frac{c^2}{2}(\mathrm{I}-\mathrm{R})-\frac{\mathrm{M}}{e}\left(\frac{d\,\mathrm{I}}{d\,x}-\frac{d\,\mathrm{R}}{d\,x}\right)\right\}\sin\phi-\mathrm{D}\,\mathrm{R}_,:$$

the sum of (c) and (d) is

$$-\sin\phi_,\cos\theta\left(\frac{c^2}{2}\mathrm{T}-\frac{\mathrm{M}_,}{e_,}\frac{d\,\mathrm{T}}{d\,x}\right)(\sin^2\phi_,-2\cos^2\phi_,+3\cos^2\phi_,)$$

$$+\sin\phi'\sin\theta'\left(\frac{c^2}{2}\mathrm{T}'-\frac{\mathrm{M}'}{e'}\frac{d\,\mathrm{T}'}{d\,x}\right)(\sin^2\phi'-2\cos^2\phi'+3\cos^2\phi')-\mathrm{D}_,\mathrm{T}_,$$

$$=-\left(\frac{c^2}{2}\mathrm{T}-\frac{\mathrm{M}_,}{e_,}\frac{d\,\mathrm{T}}{d\,x}\right)\sin\phi_,\cos\theta+\left(\frac{c^2}{2}\mathrm{T}'-\frac{\mathrm{M}'}{e'}\frac{d\,\mathrm{T}'}{d\,x}\right)\sin\phi\,\sin\theta'-\mathrm{D}_,\mathrm{T}_,:$$

and (e) is $\mathrm{Q}_{x'}(\alpha_,-\alpha)$.

Hence the sum of the quantities, or the value of $\frac{d^2 a}{d t^2}$ is reduced to the following very simple expression,

$$\frac{d^2 a}{d t^2} = -\frac{c^2}{2}\left\{(\mathrm{I}-\mathrm{R})\sin\phi + \mathrm{T}\sin\phi_{,}\cos\theta - \mathrm{T}'\sin\phi'\sin\theta'\right\} + \frac{\mathrm{M}}{e}\left(\frac{d\,\mathrm{I}}{d\,x}-\frac{d\,\mathrm{R}}{d\,x}\right)\sin\phi$$

$$+\frac{\mathrm{M}_{,}}{e_{,}}\frac{d\,\mathrm{T}}{d\,x}\sin\phi_{,}\cos\theta - \frac{\mathrm{M}'}{e'}\frac{d\,\mathrm{T}'}{d\,x}\sin\phi'\sin\theta' - \mathrm{D}\,\mathrm{R}_{,} - \mathrm{D}_{,}\mathrm{T}_{,} + \mathrm{Q}_{x,}(a_{,}-a) \quad . \quad . \quad (9).$$

This value of $\frac{d^2 a}{d t^2}$ is now in its simplest form involving only the vibrations and their differential coefficients with respect to x.

To find the value of $\frac{d^2 \beta}{d t^2}$.

By interchanging y and x, y' and x', β and a, β' and a' in equation (5), we have the following expression as the first value.

$$\frac{d^2 \beta}{d t^2} = \Sigma\left\{(\phi\,r + \frac{\phi' r}{r}\delta y^2)\,\delta\beta + \frac{\phi' r}{r}\delta x\,\delta y\,\delta a + \frac{\phi' r}{r}\delta y\,\delta z\,\delta\gamma\right\}$$

$$+\Sigma\left\{(\phi\,r' + \frac{\phi' r'}{r'}\delta y'^2)\,\delta\beta_{,} + \frac{\phi' r'}{r'}\delta x'\,\delta y'\,\delta a_{,} + \frac{\phi' r'}{r'}\delta y'\,\delta z'\,\delta\gamma_{,}\right\}$$

$$+\Sigma\left\{(\phi\,r' + \frac{\phi' r'}{r'}\delta y'^2)(\beta_{,}-\beta) + \frac{\phi' r'}{r'}\delta x'\,\delta y'(a_{,}-a) + \frac{\phi' r'}{r'}\delta y'\,\delta z'(\gamma_{,}-\gamma)\right\} \quad (10).$$

Now $\Sigma(\phi\,r + \frac{\phi' r}{r}\delta y^2)\,\delta\beta = \Sigma(\phi\,r + \frac{\phi' r}{r}\overline{i^2\sin^2\phi + p^2\cos^2\phi})\,\delta\,\mathrm{I}\cos\phi$

$$+\Sigma(\phi\,r + \frac{\phi' r}{r}\overline{r^2\sin^2\phi + p^2\cos^2\phi})\,\delta\,\mathrm{R}\cos\phi$$

$$= \Sigma(\phi\,r + \frac{\phi' r}{r}i^2)\sin^2\phi\,\delta\,\mathrm{I}\cos\phi + \Sigma(\phi\,r + \frac{\phi' r}{r}p^2)\cos^2\phi\,\delta\,\mathrm{I}\cos\phi + \text{ \&c.}$$

$$= -\frac{c^2}{2}(\mathrm{I}+\mathrm{R})\cos\phi(\cos^2\phi - 2\sin^2\phi) + \frac{\mathrm{M}}{e}\left(\frac{d\,\mathrm{I}}{d\,x}+\frac{d\,\mathrm{R}}{d\,x}\right)\cos\phi\,(\cos^2\phi - 2\sin^2\phi)\ldots(a')\text{ as in }(a).$$

We have not deemed it requisite to work out this result at full; a glance will serve to shew that it is right, when we add that, by the first of equations (7) $2\,\Sigma(\phi\,r + \frac{\phi' r}{r}p^2)\sin^2\frac{k\,i}{2} = \frac{c^2}{2}$ and by the last $2\,\Sigma(\phi\,r + \frac{\phi' r}{r}i^2)\sin^2\frac{k\,i}{2} = -\frac{2\,c^2}{2}$; and like results obtain for $\Sigma(\phi\,r + \frac{\phi' r}{r}p^2)\sin k\,i$ and $\Sigma(\phi\,r + \frac{\phi' r}{r}i^2)\sin k i$.

Again, $\Sigma\frac{\phi' r}{r}\delta x\,\delta y\,\delta a$ differs from (b) in having δa in place of $\delta\beta$, or, which is the same thing, $(\mathrm{I}-\mathrm{R})\sin\phi$ in place of $(\mathrm{I}+\mathrm{R})\cos\phi$; and in having a term containing R, ∴ by (b) we obtain,

$$\Sigma\frac{\phi' r}{r}\delta x\,\delta y\,\delta a = -3\left\{\frac{c^2}{2}(\mathrm{I}+\mathrm{R}) - \frac{\mathrm{M}}{e}\left(\frac{d\,\mathrm{I}}{d\,x}+\frac{d\,\mathrm{R}}{d\,x}\right)\right\}\sin^2\phi\cos\phi + \Sigma\frac{\phi' r}{r}\delta x\,\delta y\,\delta\,\mathrm{R},$$

But $\Sigma \frac{\phi' r}{r} \delta x \delta y \delta \mathrm{R}_{,} = \Sigma \frac{\phi' r}{r} \delta x \delta y (-\mathrm{R}_{,} \overline{1 - e^{-m \delta x} \cos f \delta y} + \frac{d \mathrm{R}_{,}}{dy} \frac{1}{f} e^{-m \delta x} \sin f \delta y)$

$$= \Sigma \frac{\phi' r}{r} \delta x \delta y \, e^{-m \delta x} \sin f \delta y \, . \frac{1}{f} \frac{d \mathrm{R}_{,}}{dy}$$

since the first part vanishes.

Denote $\Sigma \frac{\phi' r}{r} \delta x \delta y \, e^{-m \delta x} \sin f \delta y$ by F;

then $\qquad \Sigma \frac{\phi' r}{r} \delta x \delta y \delta a = -3 \left(\frac{c^2}{2} (\mathrm{I} + \mathrm{R}) - \frac{\mathrm{M}}{e} (\frac{d \mathrm{I}}{dx} + \frac{d \mathrm{R}}{dx}) \right) \sin^2 \phi \cos \phi + \frac{\mathrm{F}}{f} \frac{d \mathrm{R}_{,}}{dy} \quad . . \quad (b'.)$

$$\Sigma \frac{\phi' r}{r} \delta y \delta z \delta \gamma = 0.$$

The next term $\Sigma (\phi r' + \frac{\phi' r'}{r'} \delta y'^2) \delta \beta_{,}$ is derived from (a') in precisely the same way that we obtained (c) from (a).

The result is $\Sigma (\phi r' + \frac{\phi' r'}{r'} \delta y'^2) \delta \beta_{,}$

$$= -\left(\frac{c^2}{2} \mathrm{T} - \frac{\mathrm{M}_{,}}{e_{,}} \frac{d \mathrm{T}}{dx} \right) \cos \phi_{,} \cos \theta \, (\cos^2 \phi_{,} - 2 \sin^2 \phi_{,})$$

$$+ \left(\frac{c^2}{2} \mathrm{T}' - \frac{\mathrm{M}'}{e'} \frac{d \mathrm{T}'}{dx} \right) \cos \phi' \sin \theta' \, (\cos^2 \phi' - 2 \sin^2 \phi') \quad . \; . \; . \; . \quad (c')$$

In the same manner the value of $\Sigma \frac{\phi' r'}{r'} \delta x' \delta y' \delta a_{,}$ may be derived from (b').

The result is $\Sigma \frac{\phi' r'}{r'} \delta x' \delta y' \delta a_{,} = -3 \left(\frac{c^2}{2} \mathrm{T} - \frac{\mathrm{M}_{,}}{e_{,}} \frac{d \mathrm{T}}{dx} \right) \sin^2 \phi_{,} \cos \phi_{,} \cos \theta$

$$+ 3 \left(\frac{c^2}{2} \mathrm{T}' - \frac{\mathrm{M}'}{e'} \frac{d \mathrm{T}'}{dx} \right) \sin^2 \phi' \cos \phi' \sin \theta' + \frac{\mathrm{F}_{,}}{f} \frac{d \mathrm{T}_{,}}{dy} \quad . \; . \; . \; . \; . \quad (d')$$

The value of $\mathrm{F}_{,}$ is $\Sigma \frac{\phi' r'}{r'} \delta x' \delta y' \, e^{-m_{,} \delta x'} \sin f \delta y'$.

Lastly, $\Sigma (\phi r' + \frac{\phi' r'}{r'} \delta y'^2)(\beta_{,} - \beta) = \mathrm{Q}_{\prime\prime} (\beta_{,} - \beta) \, (e')$; and the other quantities in equation (10) are zero.

The sum of (a') and $(b') = -\frac{c^2}{2} (\mathrm{I} + \mathrm{R}) \cos \phi \, (\cos^2 \phi - 2 \sin^2 \phi + 3 \sin^2 \phi)$

$$+ \frac{\mathrm{M}}{e} \left(\frac{d \mathrm{I}}{dx} + \frac{d \mathrm{R}}{dx} \right) \cos \phi \, (\cos^2 \phi - 2 \sin^2 \phi + 3 \sin^2 \phi) + \frac{\mathrm{F}}{f} \frac{d \mathrm{R}_{,}}{dy}$$

$$= -\frac{c^2}{2} (\mathrm{I} + \mathrm{R}) \cos \phi + \frac{\mathrm{M}}{e} \left(\frac{d \mathrm{I}}{dx} + \frac{d \mathrm{R}}{dx} \right) \cos \phi + \frac{\mathrm{F}}{f} \frac{d \mathrm{R}_{,}}{dy}.$$

The sum of (c') and $(d') = -\left(\frac{c^2}{2} \mathrm{T} - \frac{\mathrm{M}_{,}}{e_{,}} \frac{d \mathrm{T}}{dx} \right) \cos \phi_{,} \cos \theta \, (\cos^2 \phi_{,} - 2 \sin^2 \phi_{,} + 3 \sin^2 \phi_{,})$

$$+ \left(\frac{c^2}{2} \mathrm{T}' - \frac{\mathrm{M}'}{e'} \frac{d \mathrm{T}'}{dx} \right) \cos \phi' \sin \theta' \, (\cos^2 \phi' - 2 \sin^2 \phi' + 3 \sin^2 \phi') + \frac{\mathrm{F}_{,}}{f} \frac{d \mathrm{T}_{,}}{dy}$$

$$= -\left(\frac{c^2}{2}\mathrm{T} - \frac{\mathrm{M}_{,}}{e_{,}}\frac{d\mathrm{T}}{dx}\right)\cos\phi_{,}\cos\theta + \left(\frac{c^2}{2}\mathrm{T}' - \frac{\mathrm{M}'}{e'}\frac{d\mathrm{T}'}{dx}\right)\cos\phi'\sin\theta' + \frac{\mathrm{F}_{,}}{f}\frac{d\mathrm{T}_{,}}{dy}.$$

Therefore by addition,

$$\frac{d^2\beta}{dt^2} = -\frac{c^2}{2}(\mathrm{I}+\mathrm{R})\cos\phi + \frac{\mathrm{M}}{e}\left(\frac{d\mathrm{I}}{dx}+\frac{d\mathrm{R}}{dx}\right)\cos\phi + \frac{\mathrm{F}}{f}\frac{d\mathrm{R}_{,}}{dy}$$

$$-\left(\frac{c^2}{2}\mathrm{T} - \frac{\mathrm{M}_{,}}{e_{,}}\frac{d\mathrm{T}}{dx}\right)\cos\phi_{,}\cos\theta + \left(\frac{c^2}{2}\mathrm{T}' - \frac{\mathrm{M}'}{e'}\frac{d\mathrm{T}'}{dx}\right)\cos\phi'\sin\theta' + \frac{\mathrm{F}_{,}}{f}\frac{d\mathrm{T}_{,}}{dy} + \mathrm{Q}_{y'}(\beta_{,}-\beta) \quad . \ (11).$$

To find the value of $\frac{d^2\gamma}{dt^2}$.

By interchanging z and x, z' and x', γ and α, γ' and α' in equation (5), we obtain

$$\frac{d^2\gamma}{dt^2} = \Sigma\left\{(\phi r + \frac{\phi' r}{r}\delta z^2)\,\delta\gamma + \frac{\phi' r}{r}\delta y\,\delta z\,\delta\beta + \frac{\phi' r}{r}\delta x\,\delta z\,\delta\alpha\right\}$$

$$+\Sigma\left\{(\phi r' + \frac{\phi' r'}{r'}\delta z'^2)\,\delta\gamma_{,} + \frac{\phi' r'}{r'}\delta y'\,\delta z'\,\delta\beta_{,} + \frac{\phi' r'}{r'}\delta x'\,\delta z'\,\delta\alpha_{,}\right\}$$

$$+\Sigma\left\{(\phi r' + \frac{\phi' r'}{r'}\delta z'^2)(\gamma_{,}-\gamma) + \frac{\phi' r'}{r'}\delta y'\,\delta z'(\beta_{,}-\beta) + \frac{\phi' r'}{r'}\delta x'\,\delta z'(\alpha_{,}-\alpha)\right\} \quad . \ . \ . \ . \ (12).$$

Now $\Sigma(\phi r + \frac{\phi' r}{r}\delta z^2)\,\delta\gamma = -\frac{c^2}{2}(\mathrm{I}'-\mathrm{R}') + \frac{\mathrm{M}}{e}\left(\frac{d\mathrm{I}'}{dx} - \frac{d\mathrm{R}'}{dx}\right)$

$$\Sigma(\phi r' + \frac{\phi' r'}{r'}\delta z'^2)\,\delta\gamma_{,} = -\frac{c^2}{2}(\mathrm{T}\sin\theta + \mathrm{T}'\cos\theta') + \frac{\mathrm{M}_{,}}{e_{,}}\frac{d\mathrm{T}}{dx}\sin\theta + \frac{\mathrm{M}'}{e'}\frac{d\mathrm{T}'}{dx}\cos\theta';$$

$$\Sigma(\phi r' + \frac{\phi' r'}{r'}\delta z'^2)(\gamma_{,}-\gamma) = \mathrm{Q}_{z'}(\gamma_{,}-\gamma);$$

and all the other quantities in equation (12) are zero.

Hence $\qquad \frac{d^2\gamma}{dt^2} = -\frac{c^2}{2}(\mathrm{I}'-\mathrm{R}'+\mathrm{T}\sin\theta + \mathrm{T}'\cos\theta')$

$$+\frac{\mathrm{M}}{e}\left(\frac{d\mathrm{I}'}{dx} - \frac{d\mathrm{R}'}{dx}\right) + \frac{\mathrm{M}_{,}}{e_{,}}\frac{d\mathrm{T}}{dx}\sin\theta + \frac{\mathrm{M}'}{e'}\frac{d\mathrm{T}'}{dx}\cos\theta' + \mathrm{Q}_{z'}(\gamma_{,}-\gamma) \quad . \ . \ . \ . \ (13).$$

We proceed now to find the equations of motion of a particle situated within the crystal. Retaining the preceding notation, the resolved part of the force parallel to the axis of x is

$$\Sigma\phi(r'+\varrho')(\delta x' + \delta\alpha_{,}) + \Sigma\phi(r+\varrho)(\delta x + (\alpha) - \alpha_{,}).$$

$$\frac{d^2\alpha_{,}}{dt^2} = \Sigma\left\{\phi r' + \phi' r'\,\frac{\delta x'\,\delta\alpha_{,} + \delta y'\,\delta\beta_{,} + \delta z'\,\delta\gamma_{,}}{r'}\right\}(\delta x' + \delta\alpha_{,})$$

$$+\Sigma\left\{\phi r + \phi' r\,\frac{\delta x((\alpha)-\alpha_{,}) + \delta y((\beta)-\beta_{,}) + \delta z((\gamma)-\gamma_{,})}{r}\right\}(\delta x + (\alpha) - \alpha_{,})$$

$$= \Sigma\left\{(\phi r' + \frac{\phi' r'}{r'}\delta x'^2)\,\delta\alpha_{,} + \frac{\phi' r'}{r'}\delta x'\,\delta y'\,\delta\beta_{,} + \frac{\phi' r'}{r'}\delta x'\,\delta z'\,\delta\gamma_{,}\right\}$$

$$+\Sigma\left\{(\phi r + \frac{\phi' r}{r}\delta x^2)((\alpha)-\alpha_{,}) + \frac{\phi' r}{r}\delta x\,\delta y((\beta)-\beta_{,}) + \frac{\phi' r}{r}\delta x\,\delta z((\gamma)-\gamma_{,})\right\}$$

But $(a)=a+\delta a$, &c.; hence if we write this expression by commencing with the portions which relate to the external medium, we have

$$\frac{d^2 a_,}{dt^2}=\Sigma\left\{\left(\phi r+\frac{\phi' r}{r}\delta x^2\right)\delta a+\frac{\phi' r}{r}\delta x\,\delta y\,\delta\beta+\frac{\phi' r}{r}\delta x\,\delta z\,\delta\gamma\right\}$$

$$+\Sigma\left\{\left(\phi r'+\frac{\phi' r'}{r'}\delta x'^2\right)\delta a_,+\frac{\phi' r'}{r'}\delta x'\,\delta y'\,\delta\beta_,+\frac{\phi' r'}{r'}\delta x'\,\delta z'\,\delta\gamma_,\right\}$$

$$+\Sigma\left\{\left(\phi r+\frac{\phi' r}{r}\delta x^2\right)(a-a_,)+\frac{\phi' r}{r}\delta x\,\delta y\,(a-a_,)+\frac{\phi' r}{r}\delta x\,\delta z\,(\gamma-\gamma_,)\right\}.$$

This equation differs from equation (5) only in the last line. If, then, in conformity with our previous notation, we write Q_x for $\Sigma\left(\phi r+\frac{\phi' r}{r}\delta x^2\right)$ we can give the value of $\frac{d^2 a_,}{dt^2}$ at once.

$$\frac{d^2 a_,}{dt^2}=\frac{d^2 a}{dt^2}+Q_x(a-a_,)-Q_{x'}(a_,-a) \quad . \quad . \quad . \quad . \quad (14)$$

similarly
$$\frac{d^2 \beta_,}{dt^2}=\frac{d^2 \beta}{dt^2}+Q_y(\beta-\beta_,)-Q_{y'}(\beta_,-\beta) \quad . \quad . \quad . \quad . \quad (15)$$

$$\frac{d^2 \gamma_,}{dt^2}=\frac{d^2 \gamma}{dt^2}+Q_z(\gamma-\gamma_,)-Q_{z'}(\gamma_,-\gamma) \quad . \quad . \quad . \quad . \quad (16).$$

Thus we have obtained the six equations of motion. Let us now obtain from these equations the results to which they give rise.

In the first place $\frac{d^2 a}{dt^2}-\frac{d^2 a_,}{dt^2}=-(Q_x+Q_{x'})(a-a_,)$: but from the values of a and $a_,$, we have

$$\frac{d^2 a}{dt^2}-\frac{d^2 a_,}{dt^2}=-c^2(a-a_,): \quad . \quad . \quad . \quad . \quad . \quad (17)$$

hence either $Q_x+Q_{x'}=c^2$ or $a-a_,=0$.

Now
$$Q_x+Q_{x'}=\Sigma\left(\phi r+\frac{\phi' r}{r}\delta x^2\right)+\Sigma\left(\phi r'+\frac{\phi' r'}{r'}\delta x'^2\right)$$

and
$$c^2=2\Sigma\left(\phi r+\frac{\phi' r}{r}\delta y^2\right)\sin^2\frac{k\,\delta x}{2}+2\Sigma\left(\phi r'+\frac{\phi' r'}{r'}\delta y'^2\right)\sin^2\frac{k\,\delta x'}{2}$$

$$\therefore \quad c^2-\overline{Q_x+Q_{x'}}=2\Sigma\frac{\delta x^2-\delta z^2}{r^5}\left(1+\sin^2\frac{k\delta x}{2}\right)+2\Sigma\frac{\delta x'^2-\delta z'^2}{r^5}\left(1+\sin^2\frac{k\delta x'}{2}\right)$$

a quantity essentially different from zero.

We have therefore $a-a_,=0$; and, by (17), $\frac{d^2 a}{dt^2}-\frac{d^2 a_,}{dt^2}=0$, as it ought.

In the same way it may be shewn that

$$\beta-\beta_,=0, \text{ and } \gamma-\gamma_,=0.$$

In the second place, by doubling equation (9), we obtain $2\frac{d^2 a}{dt^2}$, or

$$\frac{d^2 a}{d t^2}+\frac{d^2 a_{\prime}}{d t^2}=-c^2\{(\mathrm{I}-\mathrm{R}) \sin \phi+\mathrm{T} \sin \phi_{\prime} \cos \theta-\mathrm{T}' \sin \phi' \sin \theta'\}+\frac{2 \mathrm{M}}{e}\left(\frac{d \mathrm{I}}{d x}-\frac{d \mathrm{R}}{d x}\right) \sin \phi$$

$$+\frac{2 \mathrm{M}_{\prime}}{e_{\prime}} \frac{d \mathrm{T}}{d x} \sin \phi_{\prime} \cos \theta-\frac{2 \mathrm{M}'}{e'} \frac{d \mathrm{T}'}{d x} \sin \phi' \sin \theta'-2 \mathrm{D} \mathrm{R}_{\prime}-2 \mathrm{D}_{\prime} \mathrm{T}_{\prime}.$$

But $\dfrac{d^2 a}{d t^2}+\dfrac{d^2 a_{\prime}}{d t^2}=-c^2(a+a_{\prime})$

$$=-c^2\{(\mathrm{I}-\mathrm{R}) \sin \phi+\mathrm{R}_{\prime}+\mathrm{T}_{\prime}+\mathrm{T} \sin \phi_{\prime} \cos \theta-\mathrm{T}' \sin \phi' \sin \theta'\}$$

by equations (1) and (2).

Hence, by subtraction, we find that

$$c^2 \mathrm{R}_{\prime}+c^2 \mathrm{T}_{\prime}+\frac{2 \mathrm{M}}{e}\left(\frac{d \mathrm{I}}{d x}-\frac{d \mathrm{R}}{d x}\right) \sin \phi+\frac{2 \mathrm{M}_{\prime}}{e_{\prime}} \frac{d \mathrm{T}}{d x} \sin \phi_{\prime} \cos \theta$$

$$-\frac{2 \mathrm{M}'}{e'} \frac{d \mathrm{T}'}{d x} \sin \phi' \sin \theta'-2 \mathrm{D} \mathrm{R}_{\prime}-2 \mathrm{D}_{\prime} \mathrm{T}_{\prime}=0 \quad . \quad . \quad . \quad . \quad (18.)$$

Again, $\dfrac{d^2 \beta}{d t^2}+\dfrac{d^2 \beta_{\prime}}{d t^2}=-c^2(\mathrm{I}+\mathrm{R}) \cos \phi+\dfrac{2 \mathrm{M}}{e}\left(\dfrac{d \mathrm{I}}{d x}+\dfrac{d \mathrm{R}}{d x}\right) \cos \phi$

$$+\frac{2 \mathrm{F}}{f} \frac{d \mathrm{R}_{\prime}}{d y}-\left(c^2 \mathrm{T}-\frac{2 \mathrm{M}_{\prime}}{e_{\prime}} \frac{d \mathrm{T}}{d x}\right) \cos \phi_{\prime} \cos \theta+\left(c^2 \mathrm{T}'-\frac{2 \mathrm{M}'}{e'} \frac{d \mathrm{T}'}{d x}\right) \cos \phi' \sin \theta'+\frac{2 \mathrm{F}_{\prime}}{f} \frac{d \mathrm{T}_{\prime}}{d y}.$$

But $\dfrac{d^2 \beta}{d t^2}+\dfrac{d^2 \beta_{\prime}}{d t^2}=-c^2(\beta+\beta_{\prime})=-c^2\{(\mathrm{I}+\mathrm{R}) \cos \phi+\mathrm{T} \cos \phi_{\prime} \cos \theta-\mathrm{T}' \cos \phi' \sin \theta'\}$

$\therefore$ by subtraction,

$$\frac{2 \mathrm{M}}{e}\left(\frac{d \mathrm{I}}{d x}+\frac{d \mathrm{R}}{d x}\right) \cos \phi+\frac{2 \mathrm{M}_{\prime}}{e_{\prime}} \frac{d \mathrm{T}}{d x} \cos \phi_{\prime} \cos \theta-\frac{2 \mathrm{M}'}{e'} \frac{d \mathrm{T}'}{d x} \cos \phi' \sin \theta'+\frac{2 \mathrm{F}}{f} \frac{d \mathrm{R}_{\prime}}{d y}+\frac{2 \mathrm{F}_{\prime}}{f} \frac{d \mathrm{T}_{\prime}}{d y}=0. \quad (19.)$$

Also $\dfrac{d^2 \gamma}{d t^2}+\dfrac{d^2 \gamma_{\prime}}{d t^2}=-c^2\{\mathrm{I}'-\mathrm{R}'+\mathrm{T} \sin \theta+\mathrm{T}' \cos \theta'\}$

$$+\frac{2 \mathrm{M}}{e}\left(\frac{d \mathrm{I}'}{d x}-\frac{d \mathrm{R}'}{d x}\right)+\frac{2 \mathrm{M}_{\prime}}{e_{\prime}} \frac{d \mathrm{T}}{d x} \sin \theta+\frac{2 \mathrm{M}'}{e'} \frac{d \mathrm{T}'}{d x} \cos \theta'.$$

But $\dfrac{d^2 \gamma}{d t^2}+\dfrac{d^2 \gamma_{\prime}}{d t^2}=-c^2(\gamma+\gamma_{\prime})=-c^2\{\mathrm{I}'-\mathrm{R}'+\mathrm{T} \sin \theta+\mathrm{T}' \cos \theta'\}$

$\therefore$ by subtraction,

$$\frac{2 \mathrm{M}}{e}\left(\frac{d \mathrm{I}'}{d x}-\frac{d \mathrm{R}'}{d x}\right)+\frac{2 \mathrm{M}_{\prime}}{e_{\prime}} \frac{d \mathrm{T}}{d x} \sin \theta+\frac{2 \mathrm{M}'}{e'} \frac{d \mathrm{T}'}{d x} \cos \theta'=0. \quad . \quad . \quad . \quad (20.)$$

The equations $a=a_{\prime}$, $\beta=\beta_{\prime}$, $\gamma=\gamma_{\prime}$, together with the equations (18), (19), and (20) are the six equations which determine the motion.

For the sake of simplicity, let us suppose the origin to be in the common surface of the two media: then x will equal 0. But we shall not omit it, as it will guide us in the differentiations: we will conceive that its place is supplied by zero. Thus equation (3) will be reduced to the following:

$$\begin{array}{ll}
\mathrm{I}=a \cos (e x+f y+c t), & \mathrm{I}'=a' \cos (e x+f y+c t), \\
\mathrm{R}=b \cos (-e x+f y+c t), & \mathrm{R}'=b' \cos (-e x+f y+c t), \qquad (3') \\
\mathrm{R}_{\prime}=\mathrm{A}\, e^{-m x} \cos (f y+c t), & \mathrm{T}=c \cos (e_{\prime} x+f y=c t), \\
\mathrm{T}'=c' \cos (e' x+f y+c t), & \mathrm{T}_{\prime}=\mathrm{C}\, e^{-m_{\prime} x} \cos (f y+c t);
\end{array}$$

which, since $z=0$, have all the same type or the same circular function.

Our equations of motion are also

$$(\mathrm{I}-\mathrm{R})\sin\phi+\mathrm{R}_{,}=\mathrm{T}\sin\phi_{,}\cos\theta-\mathrm{T}'\sin\phi'\sin\theta'+\mathrm{T}_{,} \quad \text{(I)}$$

$$(\mathrm{I}+\mathrm{R})\cos\phi=\mathrm{T}\cos\phi_{,}\cos\theta-\mathrm{T}'\cos\phi'\sin\theta' \quad \text{(II)}$$

$$\mathrm{I}'-\mathrm{R}'=\mathrm{T}\sin\theta+\mathrm{T}'\cos\theta' \quad \text{(III)}$$

$$\frac{\mathrm{M}}{e}\left(\frac{d\mathrm{I}}{dz}-\frac{d\mathrm{R}}{dz}\right)\sin\phi+\frac{\mathrm{M}_{,}}{e_{,}}\frac{d\mathrm{T}}{dz}\sin\phi_{,}\cos\theta-\frac{\mathrm{M}'}{e'}\frac{d\mathrm{T}'}{dz}\sin\phi'\sin\theta'$$
$$+\left(\frac{c^2}{2}-\mathrm{D}\right)\mathrm{R}_{,}+\left(\frac{c^2}{2}-\mathrm{D}_{,}\right)\mathrm{T}_{,}=0 \quad \text{(IV)}$$

$$\frac{\mathrm{M}}{e}\left(\frac{d\mathrm{I}}{dz}+\frac{d\mathrm{R}}{dz}\right)\cos\phi+\frac{\mathrm{M}_{,}}{e_{,}}\frac{d\mathrm{T}}{dz}\cos\phi_{,}\cos\theta-\frac{\mathrm{M}'}{e'}\frac{d\mathrm{T}'}{dz}\cos\phi'\sin\theta'+\frac{\mathrm{F}}{f}\frac{d\mathrm{R}_{,}}{dy}+\frac{\mathrm{F}_{,}}{f}\frac{d\mathrm{T}_{,}}{dy}=0 \quad \text{(V)}$$

$$\frac{\mathrm{M}}{e}\left(\frac{d\mathrm{I}'}{dz}-\frac{d\mathrm{R}'}{dz}\right)+\frac{\mathrm{M}_{,}}{e_{,}}\frac{d\mathrm{T}}{dz}\sin\theta+\frac{\mathrm{M}'}{e'}\frac{d\mathrm{T}'}{dz}\cos\theta'=0 \quad \text{(VI)}$$

Also equation (IV.) consists of two parts, one depending on cosines, and the other on sines of the same arc. These two must, therefore, separately equal 0, so that equation (IV) is divided into the two following equations:

$$\frac{\mathrm{M}}{e}\left(\frac{d\mathrm{I}}{dz}-\frac{d\mathrm{R}}{dz}\right)\sin\phi+\frac{\mathrm{M}_{,}}{e_{,}}\frac{d\mathrm{T}}{dz}\sin\phi_{,}\cos\theta-\frac{\mathrm{M}'}{e'}\frac{d\mathrm{T}'}{dz}\sin\phi'\sin\theta'=0 \quad \text{(IV}')$$

and

$$(c^2-2\mathrm{D})\mathrm{R}_{,}+(c^2-2\mathrm{D}_{,})\mathrm{T}_{,}=0 \quad \text{(VII)}$$

We can obtain one further reduction of the equations in the following manner:

Let $\qquad \dfrac{\mathrm{M}_{,}}{e_{,}}=-p\dfrac{\mathrm{M}}{e},\ \dfrac{\mathrm{M}'}{e'}=-p'\dfrac{\mathrm{M}}{e}:$

then, since $e=\dfrac{2\pi\cos\phi}{\lambda},\ e_{,}=\dfrac{2\pi\cos\phi_{,}}{\lambda_{,}}=\dfrac{2\pi\cos\phi_{,}\sin\phi}{\lambda\sin\phi_{,}},\ e'=\dfrac{2\pi\cos\phi'}{\lambda'}=\dfrac{2\pi\cos\phi'\sin\phi}{\lambda\sin\phi'}$,

if we substitute these values in (IV′) and divide by $\sin(fy+ct)$, retaining the notation of (3′), we obtain

$$(a+b)\sin\phi\cos\phi-pc\frac{\sin\phi_{,}\cos\theta\cos\phi_{,}\sin\phi}{\sin\phi_{,}}+p'c'\frac{\sin\phi'\sin\theta'\cos\phi'\sin\phi}{\sin\phi'}=0,$$

or

$$(a+b)\cos\phi-pc\cos\phi_{,}\cos\theta+p'c'\cos\phi'\sin\theta'=0.$$

Also equation (II) gives, by dividing out $\cos(fy+ct)$,

$$(a+b)\cos\phi=c\cos\phi_{,}\cos\theta-c'\cos\phi'\sin\theta'.$$

Hence, by combining these two, we have

$$(1-p)c\cos\phi_{,}\cos\theta-(1-p')c'\cos\phi'\sin\theta'=0 \quad \text{(VIII)}$$

Equation (V) is also reduced by writing $\dfrac{\mathrm{F}}{f}=-\dfrac{\mathrm{M}}{e}$ (to which it is equal), $\dfrac{\mathrm{F}_{,}}{f}=+s\dfrac{\mathrm{M}}{e}$ and $f=\dfrac{2\pi}{\lambda}\sin\phi$: it gives

$$(a-b)\cos^2\phi-pc\frac{\cos^2\phi_{,}\cos\theta\sin\phi}{\sin\phi_{,}}+p'c'\frac{\cos^2\phi'\sin\theta'\sin\phi}{\sin\phi'}-\mathrm{A}\sin\phi+s\mathrm{C}\sin\phi=0 \quad \text{(V}')$$

and equation (VI) gives

$$(a'+b')\cos\phi - pc\frac{\sin\theta\cos\phi_,\sin\phi}{\sin\phi_,} - p'c'\frac{\cos\theta'\cos\phi'\sin\phi}{\sin\phi'}=0 \quad . \quad . \quad (\text{VI}')$$

Now it will be convenient to alter the notation slightly, since the notation I for the incident vibration is more intelligible to the eye than a. Let us then denote the incident vibration by I, or, which is the same thing, multiply every part of our expressions by $\cos(fy+ct)$: then our equations are (I), (II), (III), which remain unaltered;

and $$(1-p)\,\text{T}\cos\phi_,\cos\theta-(1-p')\,\text{T}'\cos\phi'\sin\theta'=0 \quad . \quad . \quad . \quad . \quad . \quad (\text{IV}'')$$

$$(\text{I}-\text{R})\frac{\cos^2\phi}{\sin\phi}-p\,\text{T}\frac{\cos^2\phi_,}{\sin\phi_,}\cos\theta+p'\,\text{T}'\frac{\cos^2\phi'}{\sin\phi'}\sin\theta'-\text{R}_,+s\,\text{T}_,=0 \quad . \quad . \quad (\text{V}'')$$

$$(\text{I}'+\text{R}')\frac{\cos\phi}{\sin\phi}-p\,\text{T}\frac{\cos\phi_,}{\sin\phi_,}\sin\theta-p'\,\text{T}'\frac{\cos\phi'}{\sin\phi'}\cos\theta'=0 \quad . \quad . \quad . \quad . \quad . \quad (\text{VI}'')$$

together with $$(c^2-2\,\text{D})\,\text{R}_,+(c^2-2\,\text{D}_,)\,\text{T}_,=0 \quad . \quad . \quad . \quad . \quad . \quad (\text{VII})$$

or (see previous Memoir) $$\text{R}_,+\text{T}_,=0 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (\text{VII}')$$

SECTION II. APPLICATION TO ORDINARY REFRACTION.

The first application we propose to make of our formulæ is the determination of the intensity of light reflected and refracted at the surface of a non-crystallized medium. This problem differs from that which we solved in the memoir on Fresnel's formulæ, in this respect, that in that case the incident light was supposed to be light polarized in two planes at right angles to each other. We now suppose common light to be composed of light whose plane of polarization continually shifts its position.

Our equations at once answer this hypothesis by making $\phi_,=\phi'$, and $\therefore\ p=p'$.

Now equation (IV″) is, in this case,

$$(1-p)\,(\text{T}\cos\theta-\text{T}'\sin\theta')\cos\phi_,=0.$$

Either, therefore, $1-p=0$, or $\text{T}\cos\theta=\text{T}'\sin\theta'$.

If the latter be the case, equations (I), (II), and (V″), that is, all the equations depending on the plane of incidence are independent of T and T′, or of the transmitted ray. But this can never be conceived to exist, except perhaps in metals; we must, therefore, adopt the other solution $1-p=0$.

Also $s=1$; $\therefore$ our equations become

$$(\text{I}-\text{R})\sin\phi+\text{R}_,=\text{T}\sin\phi'\cos\theta-\text{T}'\sin\phi'\sin\theta'+\text{T}_, \quad . \quad . \quad (1)$$

$$(\text{I}+\text{R})\cos\phi=\text{T}\cos\phi'\cos\theta-\text{T}'\cos\phi'\sin\theta' \quad . \quad . \quad . \quad (2)$$

$$\text{I}'-\text{R}'=\text{T}\sin\theta+\text{T}'\cos\theta' \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

$$(\text{I}-\text{R})\frac{\cos^2\phi}{\sin\phi}=\text{T}\frac{\cos^2\phi'}{\sin\phi'}\cos\theta-\text{T}\frac{\cos^2\phi'}{\sin\phi'}\sin\theta'+\text{R}_,-\text{T}_, \quad . \quad . \quad (4)$$

$$(I'+R')\frac{\cos\phi}{\sin\phi}=T\frac{\cos\phi'}{\sin\phi'}\sin\theta+T'\frac{\cos\phi'}{\sin\phi'}\cos\theta' \quad . \quad . \quad . \quad . \quad (5)$$

By adding (1) and (4) we have

$$(I-R)\left(\sin\phi+\frac{\cos^2\phi}{\sin\phi}\right)=T\cos\theta\left(\sin\phi'+\frac{\cos^2\phi'}{\sin\phi'}\right)-T'\sin\theta'\left(\sin\phi'+\frac{\cos^2\phi'}{\sin\phi'}\right)$$

or
$$\frac{I-R}{\sin\phi}=\frac{T\cos\theta}{\sin\phi'}-\frac{T'\sin\theta'}{\sin\phi'}, \quad (1, 4)$$

By combining (1, 4) and (2) we get

$$2I=T\cos\theta\left(\frac{\cos\phi'}{\cos\phi}+\frac{\sin\phi}{\sin\phi'}\right)-T'\sin\phi'\left(\frac{\cos\phi'}{\cos\phi}+\frac{\sin\phi}{\sin\phi'}\right)$$

$$=(T\cos\theta-T'\sin\theta')\left(\frac{\cos\phi'}{\cos\phi}+\frac{\sin\phi}{\sin\phi'}\right) \quad . \quad . \quad . \quad . \quad (6)$$

$$2R=(T\cos\theta-T'\sin\theta')\left(\frac{\cos\phi'}{\cos\phi}-\frac{\sin\phi}{\sin\phi'}\right) \quad . \quad . \quad . \quad . \quad (7)$$

In the same way, by combining (3) and (5) we have

$$2I'=(T\sin\theta+T'\cos\theta')\left(\frac{\sin\phi\cos\phi'}{\sin\phi'\cos\phi}+1\right) \quad . \quad . \quad (8)$$

$$2R'=(T\sin\theta+T'\cos\theta')\left(\frac{\sin\phi\cos\phi'}{\sin\phi'\cos\phi}-1\right) \quad . \quad . \quad . \quad (9).$$

Now let the incident light be supposed to be polarized in a plane, making the angle ω with that of incidence, then $I=-\rho\sin\omega$, $I'=\rho\cos\omega$, $\therefore 2I'=-2I\cot\text{an}\,\omega$,

$$\therefore \quad (T\sin\theta+T'\cos\theta')\left(\frac{\sin\phi\cos\phi'}{\sin\phi'\cos\phi}+1\right)=-(T\cos\theta-T'\sin\theta')\cot\omega\left(\frac{\cos\phi'}{\cos\phi}+\frac{\sin\phi}{\sin\phi'}\right),$$

or
$$(T\sin\theta+T'\cos\theta')\frac{\sin\overline{\phi+\phi'}}{\sin\phi'\cos\phi}=-(T\cos\theta-T'\sin\theta')\cot\omega\frac{\sin\overline{\phi+\phi'}\cos\overline{\phi-\phi'}}{\cos\phi\sin\phi'},$$

or
$$T\sin\theta+T'\cos\theta'=-(T\cos\theta-T'\sin\theta')\cot\omega\cos\overline{\phi-\phi'}$$

which gives $T'(\cos\theta'-\sin\theta'\cot\omega\cos\overline{\phi-\phi'})=-T(\cos\theta\cot\omega\cos\overline{\phi-\phi'}+\sin\theta)$. (10).

Again, if the reflected light be polarized in a plane, making the angle α with that of incidence $R'=\rho\cos\alpha$, $R=\rho\sin\alpha$,

$$\therefore \quad 2R'=-2R\cot\alpha,$$

and
$$(T\cos\theta-T'\sin\theta')\left(\frac{\cos\phi'}{\cos\phi}-\frac{\sin\phi}{\sin\phi'}\right)\cot\alpha=-(T\sin\theta+T'\cos\theta')\frac{\sin\overline{\phi-\phi'}}{\sin\phi'\cos\phi}.$$

or
$$(T\cos\theta-T'\sin\theta')\cos\overline{\phi+\phi'}\cot\alpha=(T\sin\theta+T'\cos\theta'),$$

or
$$T(-\cos\theta\cos\overline{\phi+\phi'}\cot\alpha+\sin\theta)=-T'\{\sin\theta'\cos\overline{\phi+\phi'}\cot\alpha+\cos\theta'\} \quad . \quad . \quad (11).$$

Multiplying this equation by the former, we have

$$(\cos\theta' - \sin\theta' \cot\omega \cos\overline{\phi - \phi'})(\cos\theta \cos\overline{\phi + \phi'} \cot\alpha - \sin\theta) =$$

$$-(\cos\theta \cot\omega \cos\overline{\phi - \phi'} + \sin\theta)(\sin\theta' \cos\overline{\phi + \phi'} \cot\alpha + \cos\theta')$$

or

$$\cos\theta \cos\theta' \cos\overline{\phi + \phi'} \cot\alpha + \sin\theta \sin\theta' \cos\overline{\phi - \phi'} \cot\omega =$$

$$-\cos\theta \cos\theta' \cos\overline{\phi - \phi'} \cot\omega - \sin\theta \sin\theta' \cos\overline{\phi + \phi'} \cot\alpha, \quad . \quad . \quad . \quad (12)$$

or

$$(\cos\theta \cos\theta' + \sin\theta \sin\theta')(\cos\overline{\phi + \phi'} + \cos\overline{\phi - \phi'} \tan\alpha \cot\omega).$$

The first factor gives $\theta = \theta' + \frac{\pi}{2}$ which shews that the two planes of vibration coincide when ω is given. This result is interpreted by saying, that *if the incident light is polarized, the refracted light is polarized also.* The other factor will be zero when the light incident is common light, that is when ω is indeterminate; and, further, it is evident that both its terms will be separately zero or $\tan\alpha = 0$, and $\cos(\phi + \phi') = 0$. Of these the former shews *that the plane of polarization coincides with that of incidence;* and the latter, that the value of the polarizing angle is determined from the circumstance that *the angles of incidence and refraction are complementary to one another*, which is the well-known law obtained by experiment.

If we suppose, as FRESNEL does, that the transmitted light consists of vibrations polarized in two planes at right angles to each other, we have $\theta = \theta' = 0$,

$$\therefore \quad 2\,\mathrm{I} = \mathrm{T}\frac{\sin\overline{\phi + \phi'} \cos\overline{\phi - \phi'}}{\sin\phi' \cos\phi}, \quad 2\,\mathrm{R} = -\mathrm{T}\frac{\sin\overline{\phi - \phi'} \cos\overline{\phi + \phi'}}{\sin\phi' \cos\phi},$$

$$2\,\mathrm{I}' = \mathrm{T}'\frac{\sin\overline{\phi + \phi'}}{\sin\phi' \cos\phi}, \quad 2\,\mathrm{R}' = \mathrm{T}'\frac{\sin\overline{\phi - \phi'}}{\sin\phi' \cos\phi},$$

$$\therefore \quad \mathrm{R} = -\mathrm{I}\frac{\sin\overline{\phi - \phi'}}{\sin\overline{\phi + \phi'}} \cdot \frac{\cos\overline{\phi + \phi'}}{\cos\overline{\phi - \phi'}},$$

$$= -\mathrm{I}\frac{\tan\overline{\phi - \phi'}}{\tan\overline{\phi + \phi'}}, \qquad \text{(FRESNEL's result.)}$$

$$\mathrm{T} = \mathrm{I}\frac{2\sin\phi' \cos\phi}{\sin\overline{\phi + \phi'} \cos\overline{\phi - \phi'}}, \qquad \text{(Do.)}$$

$$\mathrm{R}' = \mathrm{I}'\frac{\sin\overline{\phi - \phi'}}{\sin\overline{\phi + \phi'}}, \qquad \text{(Do.)}$$

$$\mathrm{T}' = \frac{2\,\mathrm{I}' \sin\phi' \cos\phi}{\sin\overline{\phi + \phi'}}, \qquad \text{(Do.)}$$

Thus all FRESNEL's four results are contained in our equations as particular cases.

Lastly, let us suppose the incident light to be polarized; then ω is no longer indeterminate, and equation (12) gives

$$\tan a = -\tan\omega . \frac{\cos\overline{\phi+\phi'}}{\cos\overline{\phi-\phi'}}$$

which coincides with the value of the inclination of the plane of polarization to that of incidence given by FRESNEL'S Theory. (See AIRY'S Tracts, p. 361.)

SECTION III. MR M'CULLAGH'S HYPOTHESIS RELATIVE TO THE NATURE OF CRYSTALLINE REFLECTION ON COMMON LIGHT.

In this section we propose to determine the polarizing angle, and the planes of polarization, by means of the hypothesis that each ray within the crystal is produced by a portion of the incident light polarized in a certain plane. Let us take the extraordinary wave, and, in discussing it, let us suppose no other to exist. Let I, R, I′, R′ be the incident and reflected vibrations in, and perpendicular to, the plane of incidence. T the transmitted vibration in a plane which, by FRESNEL'S Theory, passes through the axis of the crystal. Let this plane make the angle θ with that of incidence. Then all our equations of motion apply, if we omit T′.

By equation (IV.) we get $p=1$.

Also, we know that $F=-M\tan\phi$.

The only quantity which we cannot determine is $F_{,}$; call it $M s \tan\phi$.

Then we have the following equations:—

$$(I-R)\sin\phi + R_{,} = T\sin\phi_{,}\cos\theta + T_{,} \quad . \quad . \quad . \quad (1)$$

$$(I+R)\cos\phi = T\cos\phi_{,}\cos\theta \quad . \quad . \quad . \quad (2)$$

$$I'-R' = T\sin\theta \quad . \quad . \quad . \quad (3)$$

$$R_{,}+T_{,}=0 \quad . \quad . \quad . \quad (4)$$

$$(I-R)\cos\phi = T\tan\phi\frac{\cos^2\phi_{,}}{\sin\phi_{,}}\cos\theta + R_{,}\tan\phi - T_{,}s\tan\phi \quad . \quad (5)$$

$$I'+R' = T\tan\phi\cot\phi_{,}\sin\theta \quad . \quad . \quad . \quad (6).$$

Adding (1) $\sin\phi$ to (5) $\cos\phi$, we get

$$I-R = T\frac{\sin\phi}{\sin\phi_{,}}\cos\theta - T_{,}(s-1)\sin\phi \quad . \quad . \quad . \quad (\tfrac{1}{5})$$

$$I+R = T\frac{\cos\phi_{,}}{\cos\phi}\cos\theta$$

$$2I' = T(\tan\phi\cot\phi_{,}+1)\sin\theta$$

$$2R' = T(\tan\phi\cot\phi_{,}-1)\sin\theta$$

$$2R = T\left(\frac{\cos\phi_{,}}{\cos\phi} - \frac{\sin\phi}{\sin\phi_{,}}\right)\cos\theta + T_{,}(s-1)\sin\phi.$$

Now let us suppose that the plane of polarization of the reflected ray is inclined by the angle a to that of incidence, we get $\tan a = -\frac{R}{R'}$ (since R was measured upwards)

$$\therefore \tan a = -\frac{\left(\frac{\cos\phi_{,}}{\cos\phi} - \frac{\sin\phi}{\sin\phi_{,}}\right)\cos\theta + \frac{T_{,}}{T}(s-1)\sin\phi}{(\tan\phi\cot\phi_{,}-1)\sin\theta}$$

$$= \cos(\phi+\phi_{,})\cot\theta - \frac{T_{,}}{T}\frac{(s-1)\sin\phi\sin\phi_{,}\cos\phi}{\sin(\phi-\phi_{,})\sin\theta}.$$

Now s varies as the *mass* of ether put in motion by the *ray* compared with the same mass without the crystal. Also, it is such as to equal 1 when the ray coincides with the wave; and we can easily find the ratio of the masses in the following manner.

The mass outside the crystal has a common base at the surface of the crystal with the mass inside. Also the slant heights corresponding with portions moved during the same time, are in proportion to the velocities v, $v_{\circ}$ of the wave without and of the ray within the crystal.

Lastly, the angles made by those slant heights with the common base are the complements of ϕ and $\phi_{\circ}$, which the wave and ray make respectively with the normal.

Hence we have the ratio of the volumes in motion within the crystal to that without $= \frac{v_{\circ}\cos\phi_{\circ}}{v\cos\phi}$.

Let ϵ be the angle which the ray makes with the wave: then (fig.) if T be the place of the wave, $T_{\circ}$ in the plane AT, will be that of the ray, and $XT_{\circ} = \phi_{\circ}$ $TT_{\circ} = \epsilon$.

But $v_{\circ} = \frac{v_{,}}{\cos\epsilon}$ where $v_{,}$ is the velocity of the *wave* within the crystal,

$\therefore$ the ratio of the volumes moved $= \frac{v_{,}\cos\phi_{\circ}}{v\cos\phi\cos\epsilon} = \frac{\sin\phi_{,}\cos\phi_{\circ}}{\sin\phi\cos\phi\cos\epsilon}$.

Now, by Spherical Trigonometry, it is evident that

$$\cos\phi_{\circ} = \cos\phi_{,}\cos\epsilon + \sin\phi_{,}\sin\epsilon\cos\theta,$$

therefore the ratio of the volume in motion due to the ray within the crystal, to that in motion due to the wave without, is

$$\frac{\sin\phi_{,}}{\sin\phi\cos\phi\cos\epsilon}(\cos\phi_{,}\cos\epsilon + \sin\phi_{,}\sin\epsilon\cos\theta).$$

Let A represent the ratio of the densities: then the ratio of the masses is

$$A\frac{\sin\phi_{,}\cos\phi_{,} + \sin^2\phi_{,}\cos\theta\tan\epsilon}{\sin\phi\cos\phi} \quad \text{(See M'CULLAGH, p. 29).}$$

But the value of s, when the ray coincides with the wave, is a multiple of

$\dfrac{\sin\phi_{,}\cos\phi_{,}}{\sin\phi\cos\phi}$, and $s-1$ is the difference between the value of this quantity for the *ray* and for the wave.

$$\therefore\quad \frac{T_{,}}{T}(s-1)=\frac{T_{,}}{T}A\frac{\sin^2\phi_{,}\cos\theta\tan\epsilon}{\sin\phi\cos\phi},$$

and $$\tan a=\cos(\phi+\phi_{,})\cot\theta-\frac{T_{,}}{T}A\frac{\sin^3\phi_{,}\cos\theta\tan\epsilon}{\sin(\phi-\phi_{,})\sin\theta}$$

If we suppose, with Mr M'CULLAGH and Mr NEUMANN, that $A=1$, our formula will coincide with that of the former, by supposing that T, the transmitted vibration, is the resolved part (in its proper direction) of the lost vibratory motion $T_{,}$. This supposition amounts to conceiving that *all* the motion communicated to the interior medium, is due to the motion *at first* given in a direction perpendicular to the surface.

The equation which we have obtained agrees well with experiment. Mr M'CULLAGH has given the application of this equation so fully in his memoir, that I do not think myself justified in discussing it in this place.

To obtain the result corresponding to the ordinary ray, we have only to write ϕ' for $\phi_{,}$, $90+\theta'$ for θ, and 1 for s, and we get

$$\tan a=-\cos(\phi+\phi')\tan\theta'.$$

This equation gives the value of the deviation or shifting of the plane of polarization. It is precisely the same as that obtained by M. NEUMANN and Mr M'CULLAGH, and agrees closely with experiment.

Equating the two values of $\tan a$, we have the following equation for determining the value of the polarizing angle:

$$\cos(\phi+\phi')\tan\theta'+\cos(\phi+\phi_{,})\cot\theta-\frac{T_{,}A}{T}\cdot\frac{\sin^3\phi_{,}\cos\theta\tan\epsilon}{\sin(\phi-\phi_{,})\sin\theta}=0.$$

SECTION IV. UNIAXAL CRYSTALS.

We propose, in this section, to determine the polarizing angle by a *general* process.

Since we have already shewn that for a non-crystallized medium $p=1$, and since the value of p for the ordinary ray is the same as for a non-crystallized medium, it follows that $p=1$, and therefore by equation (IV'), $p'=1$.

Hence our equations are

$$(I-R)\sin\phi+R_{,}=T\sin\phi_{,}\cos\theta-T'\sin\phi'\sin\theta'+T_{,}\quad\ldots\quad(1)$$

$$(I+R)\cos\phi=T\cos\phi_{,}\cos\theta-T'\cos\phi'\sin\theta'\quad\ldots\quad(2)$$

$$I'-R'=T\sin\theta+T'\cos\theta'\quad\ldots\quad(3)$$

$$(I-R)\frac{\cos^2\phi}{\sin\phi}-T\frac{\cos^2\phi_{,}}{\sin\phi_{,}}\cos\theta+T'\frac{\cos^2\phi'}{\sin\phi'}\sin\theta'-R_{,}+sT_{,}=0\quad\ldots\quad(4)$$

$$(\mathrm{I}'+\mathrm{R}')\frac{\cos\phi}{\sin\phi}=\mathrm{T}\,\frac{\cos\phi_,}{\sin\phi_,}\sin\theta+\mathrm{T}'\frac{\cos\phi'}{\sin\phi'}\cos\theta' \quad . \quad . \quad . \quad . \quad . \quad (5)$$

add together (1) and (4), and

$$\frac{\mathrm{I}-\mathrm{R}}{\sin\phi}=\frac{\mathrm{T}\cos\theta}{\sin\phi_,}-\frac{\mathrm{T}'\sin\theta'}{\sin\phi'}-(s-1)\,\mathrm{T}_, \quad . \quad . \quad . \quad . \quad (1,4)$$

By this equation and (2) we get

$$2\,\mathrm{I}=\mathrm{T}\cos\theta\left(\frac{\cos\phi_,}{\cos\phi}+\frac{\sin\phi}{\sin\phi_,}\right)-\mathrm{T}'\sin\theta'\left(\frac{\cos\phi'}{\cos\phi}+\frac{\sin\phi}{\sin\phi'}\right)-(s-1)\,\mathrm{T}_,\sin\phi \quad (6)$$

$$2\,\mathrm{R}=\mathrm{T}\cos\theta\left(\frac{\cos\phi_,}{\cos\phi}-\frac{\sin\phi}{\sin\phi_,}\right)-\mathrm{T}'\left(\frac{\cos\phi'}{\cos\phi}-\frac{\sin\phi}{\sin\phi'}\right)\sin\theta'+(s-1)\,\mathrm{T}_,\sin\phi \quad (7)$$

Put $\quad (s-1)\,\mathrm{T}_,\sin\phi=u\,\mathrm{T}\cos\theta \quad . \quad . \quad . \quad . \quad . \quad (8)$

then
$$2\,\mathrm{I}=\mathrm{T}\cos\theta\left(\frac{\sin\overline{\phi+\phi_,}\cos\overline{\phi-\phi_,}}{\sin\phi_,\cos\phi}-u\right)-\mathrm{T}'\frac{\sin\overline{\phi+\phi'}\cos\overline{\phi-\phi'}}{\sin\phi'\cos\phi}\sin\theta'$$

$$=m\,\mathrm{T}-n\,\mathrm{T}' \text{ suppose;} \quad . \quad . \quad . \quad . \quad . \quad (9)$$

$$-2\,\mathrm{R}=\mathrm{T}\cos\theta\left(\frac{\sin\overline{\phi-\phi_,}\cos\overline{\phi+\phi_,}}{\sin\phi_,\cos\phi}+u\right)-\mathrm{T}'\frac{\sin\overline{\phi-\phi'}\cos\overline{\phi+\phi'}}{\sin\phi'\cos\phi}\sin\theta'$$

$$=m'\,\mathrm{T}-n'\,\mathrm{T}' \text{ suppose.} \quad . \quad . \quad . \quad . \quad . \quad (10)$$

Let us also denote, as before, $\frac{\mathrm{R}'}{\mathrm{R}}$ by $-\cot\alpha$, $\frac{\mathrm{I}'}{\mathrm{I}}$ by $-\cot\omega$.

Then if we write $-\mathrm{R}\cot\alpha$ and $-\mathrm{I}\cot\omega$ for R′ and I′, and afterwards put for R and I their values given above, we obtain from (3),

$$-(m\,\mathrm{T}-n\,\mathrm{T}')\cot\omega-(m'\,\mathrm{T}-n'\,\mathrm{T}')\cot\alpha=2\,\mathrm{T}\sin\theta+2\,\mathrm{T}'\cos\theta'$$

and $$-(m\,\mathrm{T}-n\,\mathrm{T}')\cot\omega\,\frac{\cos\phi}{\sin\phi}+(m'\,\mathrm{T}-n'\,\mathrm{T}')\cot\alpha\,\frac{\cos\phi}{\sin\phi}=2\,\mathrm{T}\frac{\cos\phi_,}{\sin\phi_,}\sin\theta+2\,\mathrm{T}'\frac{\cos\phi'}{\sin\phi'}\cos\theta'.$$

From the first,

$$\mathrm{T}\{2\sin\theta+m\cot\omega+m'\cot\alpha\}=\mathrm{T}'\{-2\cos\theta'+n\cot\omega+n'\cot\alpha\}.$$

From the second,

$$\mathrm{T}'\{2\cos\theta'\cot\phi'-n\cot\omega\cot\phi+n'\cot\alpha\cot\phi\}=$$
$$\mathrm{T}\{-2\sin\theta\cot\phi_,-m\cot\omega\cot\phi+m'\cot\alpha\cot\phi\}.$$

By multiplying these equations together we get

$$(2\sin\theta+m\cot\omega+m'\cot\alpha)\,(2\cos\theta'\cot\phi'-n\cot\omega\cot\phi+n'\cot\alpha\cot\phi)$$
$$=(-2\cos\theta'+n\cot\omega+n'\cot\alpha)\,(-2\sin\theta\cot\phi_,-m\cot\omega\cot\phi+m'\cot\alpha\cot\phi).$$

But when the incident light is common light, this expression will give rise to two, of which one is the coefficient of $\cot\omega$, and the other that part of the expression which does not contain ω.

These are respectively,

$$m\,(2\cos\theta'\cot\phi'+n'\cot\alpha\cot\phi)-n\cot\phi\,(2\sin\theta+m'\cot\alpha)$$
$$=n\,(-2\sin\theta\cot\phi_,+m'\cot\alpha\cot\phi)-m\cot\phi\,(-2\cos\theta'+n'\cot\alpha),$$

and $$(2\sin\theta+m'\cot\alpha)\,(2\cos\theta'\cot\phi'+n'\cot\alpha\cot\phi)=$$
$$(-2\cos\theta'+n'\cot\alpha)\,(-2\sin\theta\cot\phi_,+m'\cot\alpha\cot\phi);$$

or $$2\cot a\{m n' \cot\phi - m' n \cot\phi\} = 2 n \sin\theta \cot\phi - 2 n \sin\theta \cot\phi_{,}$$
$$-2 m \cos\theta' \cot\phi' + 2 m \cos\theta' \cot\phi$$

and $$2\cot a\{m' \cos\theta' \cot\phi' + n' \sin\theta \cot\phi + m' \cos\theta' \cot\phi + n' \sin\theta \cot\phi_{,}\}$$
$$= 4\sin\theta\cos\theta' \cot\phi_{,} - 4\sin\theta\cos\theta' \cot\phi'$$

or $$\cot a \cot\phi\,(m n' - m' n) = n \sin\theta\,(\cot\phi - \cot\phi_{,}) + m\cos\theta'\,(\cot\phi - \cot\phi') \quad . \quad . \quad \text{(A)}$$

and $$\cot a\,(m'\cos\theta'\,\overline{\cot\phi + \cot\phi'} + n' \sin\theta\,.\,\overline{\cot\phi + \cot\phi_{,}}) = 2\sin\theta\cos\theta'\,(\cot\phi_{,} - \cot\phi') \quad \text{(B)}$$

Eliminating $\cot a$ between these two equations, there results

$$2\sin\theta\cos\theta'\,(\cot\phi_{,} - \cot\phi')\cot\phi\,(m n' - m' n) =$$
$$(m'\cos\theta'\,\overline{\cot\phi + \cot\phi'} + n'\sin\theta\,\overline{\cot\phi + \cot\phi_{,}}) \times$$
$$(n\sin\theta\,\overline{\cot\phi - \cot\phi_{,}} + m\cos\theta'\,\overline{\cot\phi - \cot\phi'}) \quad . \quad . \quad (11)$$

This is the equation which determines the polarizing angle.

We can reduce it to a much more simple form by expunging one of the factors: thus,

$$\cot\phi_{,} - \cot\phi' = -\frac{\sin\overline{\phi_{,} - \phi'}}{\sin\phi_{,}\sin\phi'}:$$

but $\phi_{,} - \phi'$ is a small quantity, depending on the differences of the squares of the refractive indices: call it *of the first order*.

$$m n' - m' n = \cos\theta\sin\theta'\left\{\frac{\sin\overline{\phi - \phi'}\cos\overline{\phi + \phi'}}{\sin\phi\cos\phi\sin\phi'\sin\phi_{,}}\,.\,\overline{\sin\overline{\phi + \phi_{,}}\cos\overline{\phi - \phi_{,}} - u\sin\phi_{,}\cos\phi}\right.$$
$$\left.-\frac{\sin\overline{\phi + \phi'}\cos\overline{\phi - \phi'}}{\sin\phi\cos\phi\sin\phi'\sin\phi_{,}}(\sin\overline{\phi - \phi_{,}}\cos\overline{\phi + \phi_{,}} - u\sin\phi_{,}\cos\phi)\right\}$$
$$= \frac{4\cos\theta\cos\theta'}{\sin\phi'\sin\phi_{,}}\left\{\sin\overline{\phi_{,} - \phi'}\cos\overline{\phi_{,} + \phi'} - \tfrac{1}{2}u\frac{\sin\phi_{,}\sin\phi'\cos\phi'}{\sin\phi}\right\}$$

Now u is a very small quantity: hence $(m n' - m' n)(\cot\phi_{,} - \cot\phi')$ is of the second order in small quantities, and may in an approximation be neglected.

We must therefore equate to zero the first factor of equation (11).

This gives $$\frac{m'\cos\theta'\sin\overline{\phi + \phi'}}{\sin\phi\sin\phi'} + n'\frac{\sin\theta\sin\overline{\phi + \phi_{,}}}{\sin\phi\sin\phi_{,}} = 0.$$

or $$\cos\theta\cos\theta'\sin\overline{\phi - \phi_{,}}\cos\overline{\phi + \phi_{,}}\sin\overline{\phi + \phi'} +$$
$$\sin\theta\sin\theta'\sin\overline{\phi - \phi'}\cos\overline{\phi + \phi'}\sin\overline{\phi + \phi_{,}} - u\cos\theta\cos\theta'\cos\phi\sin\phi_{,}\sin\overline{\phi + \phi'} = 0$$

$$\therefore\quad \tan\theta'\cos\overline{\phi + \phi'}\frac{\sin\overline{\phi - \phi'}}{\sin\overline{\phi + \phi'}} + \cot\theta\cos\overline{\phi + \phi_{,}}\frac{\sin\overline{\phi - \phi_{,}}}{\sin\overline{\phi + \phi_{,}}} - \frac{u\cos\theta}{\sin\overline{\phi + \phi_{,}}}\,.\,\frac{\cos\phi\sin\phi_{,}}{\sin\theta} = 0 \quad (13)$$

From the form of this equation, we are inclined to think that it is the correct solution of the problem.

We must remember that we owe the equation altogether to our neglect of quantities of the second order. We ought, therefore, at once to reject from it all such quantities as interfere with the simplicity of its form.

Now
$$\phi-\phi'=\phi-\phi_{,}+\overline{\phi_{,}-\phi'}=\phi-\phi_{,}+D \quad \text{suppose}$$
$$\phi+\phi'=\phi+\phi_{,}-\overline{\phi_{,}-\phi'}=\phi+\phi_{,}-D$$
$$\therefore \quad \sin\overline{\phi-\phi'}=\sin\overline{\phi-\phi_{,}}+\cos\overline{\phi-\phi_{,}}\,.\,D$$
$$\sin\overline{\phi+\phi'}=\sin\overline{\phi+\phi_{,}}-\cos\overline{\phi+\phi_{,}}\,D$$
$$\therefore \quad \frac{\sin\overline{\phi-\phi'}}{\sin(\phi+\phi')}=\frac{\sin\overline{\phi-\phi_{,}}}{\sin(\phi+\phi_{,})}+C\,.\,D$$

But this quantity has to be multiplied into $\cos\overline{\phi+\phi'}$, which is itself of the first order
$$\therefore \quad \cos\overline{\phi+\phi'}\,\frac{\sin\overline{\phi-\phi'}}{\sin\overline{\phi+\phi'}}=\frac{\sin\overline{\phi-\phi_{,}}}{\sin\overline{\phi+\phi_{,}}}\cos\overline{\phi+\phi'}$$
omitting quantities of the second order.

Hence, dividing by $\dfrac{\sin\overline{\phi-\phi_{,}}}{\sin\overline{\phi+\phi_{,}}}$, we get
$$\tan\theta'\cos\overline{\phi+\phi'}+\cot\theta\cos(\phi+\phi_{,})-\frac{u\cos\phi\sin\phi_{,}\cos\theta}{\sin\overline{\phi-\phi_{,}}\sin\theta}=0 \quad . \quad (14)$$
which is of the *form* given by Mr M'CULLAGH, and is precisely the same as that obtained by the process in Section III. The value of u too is the same as that given by the formula there employed, which shews that we are correct in assuming that u depends only on the difference between the ray and the wave.

If it seem difficult to leave the quantity $T_{,}$ as part of the undetermined quantity, we may easily get rid of the difficulty. In fact, our only reason for adopting this mode of proceeding was, that, since it is requisite to have some one indeterminate quantity, we may as well have that quantity a compound one as a simple one, provided it simplifies our operations. Let us therefore combine with our former equations, the equation (VII'.), $R_{,}+T_{,}=0$.

Then if we multiply equation (1) by $1+s$, and equation (4) by 2, and add them, we have
$$(I-R)\left(\overline{1+s}\sin\phi+\frac{2\cos^2\phi}{\sin\phi}\right)=\left\{(1+s)\sin\phi_{,}+\frac{2\cos^2\phi_{,}}{\sin\phi_{,}}\right\}T\cos\theta$$
$$-\left\{(1+s)\sin\phi'+\frac{2\cos^2\phi'}{\sin\phi'}\right\}T'\sin\theta',$$
or if $1+s=2+2t$ where t is very small,
$$(I-R)\left(\frac{1}{\sin\phi}+t\sin\phi\right)=T\cos\theta\left(\frac{1}{\sin\phi_{,}}+t\sin\phi_{,}\right)-T'\sin\theta'\left(\frac{1}{\sin\phi'}+t\sin\phi'\right)$$
$$I-R=T\cos\theta\,(1-t\,\overline{\sin^2\phi-\sin^2\phi_{,}})\,\frac{\sin\phi}{\sin\phi_{,}}-T'\sin\theta'\,(1-t\,\overline{\sin^2\phi-\sin^2\phi'})\,\frac{\sin\phi}{\sin\phi'}$$
$$2\,I=T\cos\theta\left(\frac{\cos\phi_{,}}{\cos\phi}+\frac{\sin\phi}{\sin\phi_{,}}-t\,\frac{\sin\phi}{\sin\phi_{,}}\,\overline{\sin^2\phi-\sin^2\phi_{,}}\right)$$
$$-T'\sin\theta'\left(\frac{\cos\phi'}{\cos\phi}+\frac{\sin\phi}{\sin\phi'}-t\,\frac{\sin\phi}{\sin\phi'}\,\overline{\sin^2\phi-\sin^2\phi'}\right)$$

$$\therefore \qquad m=\left(\frac{\sin\overline{\phi+\phi_,}\cos\overline{\phi-\phi_,}}{\cos\phi\sin\phi_,}-t\frac{\sin\phi}{\sin\phi_,}\cdot\overline{\sin^2\phi-\sin^2\phi_,}\right)\cos\theta$$

$$n=\left(\frac{\sin\overline{\phi+\phi'}\cos\overline{\phi-\phi'}}{\cos\phi\sin\phi'}-t\frac{\sin\phi}{\sin\phi'}\cdot\overline{\sin^2\phi-\sin^2\phi'}\right)\sin\theta'$$

$$m'=\left(\frac{\sin\overline{\phi-\phi_,}\cos\overline{\phi+\phi_,}}{\cos\phi\sin\phi_,}-t\frac{\sin\phi}{\sin\phi_,}\overline{\sin^2\phi-\sin^2\phi_,}\right)\cos\theta$$

$$n'=\left(\frac{\sin\overline{\phi-\phi'}\cos\overline{\phi+\phi'}}{\cos\phi\sin\phi'}-t\frac{\sin\phi}{\sin\phi'}\overline{\sin^2\phi-\sin^2\phi'}\right)\sin\theta'$$

therefore the factor of (11) gives us

$$\frac{\cos\theta\cos\theta'\sin\overline{\phi+\phi'}}{\sin\phi'}\left(\frac{\sin\overline{\phi-\phi_,}\cos\overline{\phi+\phi_,}}{\cos\phi\sin\phi_,}-t\frac{\sin\phi}{\sin\phi_,}\overline{\sin^2\phi-\sin^2\phi_,}\right)$$

$$+\frac{\sin\theta\sin\theta'\sin\overline{\phi+\phi_,}}{\sin\phi_,}\left(\frac{\sin\overline{\phi-\phi'}\cos\overline{\phi+\phi'}}{\cos\phi\sin\phi'}-t\frac{\sin\phi}{\sin\phi'}\overline{\sin^2\phi-\sin^2\phi'}\right)=0$$

or

$$\frac{\tan\theta'}{\sin\overline{\phi+\phi'}}\left(\sin\overline{\phi-\phi'}\cos\overline{\phi+\phi'}-t\sin\phi\cos\phi\,\overline{\sin^2\phi-\sin^2\phi'}\right)$$

$$+\frac{\cot\theta}{\sin\overline{\phi+\phi_,}}\left(\sin\overline{\phi-\phi_,}\cos\overline{\phi+\phi_,}-t\sin\phi\cos\phi\,\overline{\sin^2\phi-\sin^2\phi_,}\right)=0$$

The part which multiplies t is $\sin\phi\cos\phi\left(\frac{\tan\theta'(\sin^2\phi-\sin^2\phi')}{\sin(\phi+\phi')}+\frac{\cot\theta(\sin^2\phi-\sin^2\phi_,)}{\sin(\phi+\phi_,)}\right)$

$$=\frac{\sin\phi\cos\phi}{\sin\theta\cos\theta}\cdot\frac{\sin^2\phi-\sin^2\phi_,}{\sin(\phi+\phi_,)}\text{ very nearly,}$$

and

$$t=\frac{\mathrm{A}}{2}\cdot\frac{\sin^2\phi_,\cos\theta\tan\epsilon}{\sin\phi\cos\phi},$$

$\therefore$ the subtractive part of equation (13) is $\frac{\mathrm{A}}{2}\cdot\frac{(\sin^2\phi-\sin^2\phi_,)\sin^2\phi_,\tan\epsilon}{\sin(\phi+\phi_,)\sin\theta}$.

Our equation is therefore

$$\tan\theta'\frac{\sin(\phi-\phi')\cos(\phi+\phi')}{\sin(\phi+\phi'}+\cot\theta\frac{\sin\overline{\phi-\phi_,}\cos\overline{\phi+\phi_,}}{\sin(\phi+\phi_,)}-\frac{\mathrm{A}}{2}\frac{(\sin^2\phi-\sin^2\phi_,)\sin^2\phi_,\tan\epsilon}{\sin(\phi+\phi_,)\sin\theta}=0.$$

If we adopt the notation of Mr M'Cullagh, and put $\tan\epsilon=\frac{a^2-b^2}{s^2}\sin\omega\cos\omega$,

where

$$s^2=\frac{\sin^2\phi_,}{\sin^2\phi}=b^2+(a^2-b^2)\sin^2\omega$$

$$\tan\epsilon=\frac{(a^2-b^2)\sin\omega\cos\omega\sin^2\phi}{\sin^2\phi_,},$$

the above equation becomes

$$\tan\theta'\frac{\sin\overline{\phi-\phi'}\cos\overline{\phi+\phi'}}{\sin(\phi+\phi')}+\cot\theta\frac{\sin\overline{\phi-\phi_,}\cos\overline{\phi+\phi_,}}{\sin(\phi+\phi_,)}$$

$$-\frac{\mathrm{A}}{2}\cdot\frac{(a^2-b^2)(\sin^2\phi-\sin^2\phi_,)\sin^2\phi\sin\omega\cos\omega}{\sin(\varphi+\phi_,)\sin\theta}=0 \quad . \quad . \qquad (15.)$$

This coincides very nearly with M. Neumann's formula.

To find the value of the deviation, we have recourse to equation (A).

By substituting for m, m', &c. their values in the first side of this equation, and omitting quantities of the second order, this is reduced to

$$\cot \alpha \frac{2 \cos \theta \sin \theta'}{\sin \phi_, \sin \phi'} \sin (\phi_, - \phi') \cos (\phi_, + \phi_,)$$

By the same substitution, and by virtue of equation (13), the second side is reduced to

$$-\frac{2 \cos \theta \cos \theta' \sin (\phi_, - \phi') \cos (\phi_, + \phi')}{\sin \phi_, \sin \phi' \cos (\phi + \phi')}.$$

By equating these we have

$$\tan \alpha = -\cos (\phi + \phi') \tan \theta' \quad . \quad . \quad . \quad (16)$$

On this result it is unnecessary to offer any remark.

SECTION V. BIAXAL CRYSTALS.

We shall be very brief in our exposition of the method of proceeding which must be applied to Biaxal Crystals. In fact, we have little else to do than to repeat our previous formulæ, making the slight difference in them which consists in supposing a quantity s for each ray in both cases different from unity. Thus for the one ray, we shall have

$$\tan \alpha = \cos (\phi + \phi_,) \cot \theta - \frac{T_,}{T} A \frac{\sin^3 \phi_, \cos \theta \tan \epsilon}{\sin (\phi - \phi_,) \sin \theta}$$

and for the other,

$$\tan \alpha = \cos (\phi + \phi') \tan \theta' - \frac{T'_,}{T} A \frac{\sin^3 \phi' \sin \theta' \tan \epsilon'}{\sin (\phi - \phi') \cos \theta'}$$

and $\quad \therefore \quad \cos \overline{\phi + \phi_,} \cot \theta - \frac{T_,}{T} A \frac{\sin \phi_, \sin^2 \phi \cos \theta \sin (\omega - \omega_,) \sin \frac{1}{2} \psi}{\sin (\phi - \phi_,) \sin \theta} \frac{(a^2 - c^2) r^2}{2}$

$$= \cos \overline{\phi + \phi'} \tan \theta' - \frac{T'_,}{T} A \frac{\sin \phi' \sin^2 \phi \sin \theta' \sin (\omega + \omega_,) \sin \frac{1}{2} \psi}{\sin \overline{\phi - \phi'} \cos \theta'} \frac{(a^2 - c^2) r_,^2}{2}.$$

See M'CULLAGH (Note to p. 37).

It is to be noticed that ω, $\omega_,$ are the angles which the direction of transmission of one (and therefore approximately of both) of the waves makes with the optic axes, and ψ the angle between the plane passing through these directions and the optic axes. The same kind of proceeding may be applied to the other method. We do not think it necessary to work out the equations for biaxal crystals at full length. Should experiments be made on this branch of optics, requiring a reference to their results, a very little additional labour will enable us to reduce our formulæ to a shape fitted for numerical computation. At the present time, to

enter farther on this subject, would be to swell the bulk of the present memoir to little purpose.

By making the equations slightly more general, and omitting the terms which correspond to the transmitted rays, retaining only those which correspond to the *lost* vibration, denoted by T, in the memoir, we obtain the formulæ for metallic reflexion. There is no difficulty whatever in deducing from such formulæ the following results:—1. That if light be reflected at the surface of a metal, both the vibrations in the plane and perpendicular to the plane of reflexion, will suffer *retardation*. 2. That the retardation of the vibration, perpendicular to the plane of reflexion, will be independent of the angle of incidence. 3. That vibrations *in* the plane of reflexion, will suffer retardation depending on the angle of incidence. 4. That the *intensity* of the reflected vibrations perpendicular to the plane of reflexion is equal to that of the incident ones. 5. That the intensity of the vibrations *in* the plane of reflexion depends on the angle of incidence. The interpretation of these results would be, 1. That polarized incident light would suffer a change of polarization from plane to elliptical, from elliptical to more or less elliptical. 2. That the tendency of a very great number of reflexions would be to change light polarized in *any* plane to light polarized in the plane of incidence. 3. That the effect on common light would be twofold; *first*, to produce in it an excess of vibrations perpendicular to the plane of reflexion; and, *secondly*, to change the phases of the two parts in, and perpendicular to, the plane of reflexion relative to one another. The former change is analogous to that which transparent media produce on light, the latter it is difficult to interpret. All that would appear to result from it is the following:—"That if a continual succession of such retardations were to take place, the parts of the ray would be totally disjoined from each other, and the result would be a ray consisting of two perfectly polarized pencils, one in, and the other perpendicular to, the plane of reflexion, travelling together; the intensity of the former being much greater than that of the latter. I regret that my limits do not permit me to produce any equations.

IV. *On certain Physiological Inferences which may be drawn from the Study of the Nerves of the Eyeball.* *By* W. P. Alison, *M.D., Professor of the Theory of Medicine.*

(Read 7th December 1840.)

It has been justly observed that the great discovery of the appropriation of the different portions of the Nervous System to the exercise of different functions, would never have been clearly established, but for the fortunate circumstance that, in certain parts of the body, especially on the face, the nerves of sense and of voluntary motion are distinct throughout their whole course. And this consideration may instruct us that, when we have an organ supplied with a variety of nerves, known to be of perfectly different endowments, the study of the peculiarities of these nerves may give us an insight into the purpose or use of some of those pieces of structure in all parts of the Nervous System, in which we must still admit that we see much contrivance, without understanding its intention.

In the case of the Eyeball, it is generally allowed that we see, separated for us by Nature, almost every kind of nerve which the physiology of any part of the body includes; we have the nerve of the special sensation, and that of common sensation; we have the nerves which excite motion in obedience to the will, and those which excite motion over which the will has no control; we can point out the incident nerve and the efferent nerve, concerned in two distinct examples of the reflex function of the spinal cord; and we can specify the nerve by which the nutrition of the whole organ, and more than one secretion contained in it, are liable to be influenced and controlled. And when we attend to the peculiarities of these nerves, and to facts which have been observed in regard to their action, I think we have sufficient data for certain inferences applicable to other parts of the Nervous System, which have not yet been distinctly pointed out, and which are steps in the progress of that most difficult, but likewise most interesting department of Physiology, where our object is to detect the laws by which mental acts are connected with the physical changes of living beings; and where, accordingly, the intimations of our own consciousness must be admitted as part of the foundation of our inferences.

I. The first peculiarity in the nerves of the eyeball to which I wish to direct attention is this, that those supplying the muscles by which the eyeball is instinc-

tively or voluntarily moved are, if not wholly (as SCARPA and others have maintained), at least almost entirely devoid of any of those filaments which we now regard as the organs of common sensation; the straight and oblique muscles having their nerves from the 3d, 4th, and 6th, to the almost complete exclusion of the ophthalmic branch of the 5th.*

I think we cannot doubt that the reason of this peculiarity, by which the muscles of the eyeball are distinguished—perhaps from every other muscle in the body, viz. the absence of purely sensitive filaments in their composition—is that already assigned by VAN DEEN,† viz. that these muscles are intended to be regulated and guided in their contractions, not by sensations excited in their own substance, or in parts directly in contact with them, but by the sensations of the Retina; and I think farther, that to this peculiarity we are to ascribe, both the positive fact, that the movements of these muscles are naturally *consentient* in the two eyes, so as to preserve the parallelism of the optic axes; and likewise the negative fact, that we have hardly any power to insulate an act of the will on one of these muscles, so as to move the one eyeball in a different direction from the other; *i. e.* the left eye, for example, turns inwards when the right eye turns outwards, because both are habitually guided by the sensations of the retina, which are similarly affected by these movements of the two eyes; and we have little power of moving either eye independently of the other, because we have hardly any sensations, consequent on the movement of the one eye and not of the other, whereby to guide the efforts of the will for this purpose.‡ And this consideration suggests some important reflections on the *office of sensitive nerves and of sensations* in regard to all movements of voluntary muscles.

It appears to me, notwithstanding some difficulties recently raised, that the essential peculiarity of all strictly Animal motion is, that it is motion dependent

* "Certum et inconcussum ut," says SCARPA, "quinti nervorum cerebri ramum ophthalmicum, orbitam transgradientem, ne minimum quidem filamentum valde conspicuis cæteroquin nervis oculum moventibus addere." (De Gangliis, &c. Isis, 1832.)

† De Differentia et Nexu inter Nervos vitæ animalis et vitæ organicæ, p. 162.

‡ It has been stated by Sir CHARLES BELL, that he believes the 3d nerve to be sensitive as well as motor, because it has an origin from behind as well as from before the grey matter of the crus cerebri; and although the examples of the portio dura and the spinal accessory nerves (which appear to be purely motor, although originating in part from the posterior portion of the cord) render that inference doubtful, yet I am bound to admit that, according to the statement of VALENTIN,* there is experimental evidence of sensations being felt on irritation of the 3d nerve. But this author is equally confident, from experiment, that there is no sensibility in the 6th nerve;† and it should be remembered that movements are often performed by the 3d nerve,—such as rolling the eyes inwards, and raising the eyelid,—which are not prompted by the sensations of the retinæ, and for the regulation of which sensations in the moving parts themselves may therefore be required.

* De Functionibus Nervorum Cerebralium, &c. p. 16. † Ibid. p. 30.

more or less directly on Sensation; that if we are certain of any movement in an organized body being altogether independent of sensation, and affording no indication of any mental act, we should refer it to the same class as movements in vegetables; and that in designating such movement as Organic, but not Animal, we express a distinction of essential importance in physiology.

It has indeed been lately maintained by several eminent physiologists, who have studied the indications of what is now called the Reflex Function of the Spinal Cord, that many living actions, such as respiration, deglutition, coughing, sneezing, and vomiting, the evacuation of the bowels and bladder, and even the movements by which irritations of the surface are avoided or repelled,—certainly attended in the natural state by sensations, and usually thought to indicate sensation, and therefore to belong to the department of animal life,—are independent of sensation, and ought, therefore, according to the principle above stated, to be referred to that of organic life. But although it is well ascertained that movements may be excited in perfectly paralytic limbs, by irritations applied to the surface, which must be carried back to the sensitive, and cross from thence to the motor portions of the spinal cord connected with those limbs; and therefore that the whole series of nervous actions which takes place when any of these reflex or sympathetic actions are excited, may be in some degree imitated by mechanical irritation of the nervous matter, independently of sensation; yet when it is inferred from this fact that, in *the entire and healthy body*, Sensation does not intervene, as a part of the sequence of cause and effect on which such actions depend, this theory overlooks so much of what has been formerly ascertained and pointed out in regard to them, that I do not think we can expect it long to hold its ground in physiology.

The movements which are excited by irritation of the sensitive nerves, in the *undoubted* absence of sensation (which of course can only be known in the human body in the state of disease), are general and irregular, and have not the character of *selection and adaptation* to particular purposes, which is essential to the useful application of any such actions in the living body. And when it is supposed that such movements as respiration, coughing, or deglutition, are equally independent of sensation, we not only overlook this, their essential character, of selection of individual nerves and adaptation to particular ends, but disregard the following facts, long ago stated in evidence, that sensations intervene in the process by which they are excited.

1. In various cases, impressions on the sensitive nerves of *different* parts of the body excite *the same* sensation, and then *the same* reflex or sympathetic action follows,—as when intense nausea results from changes whether in the brain, fauces, stomach, bowels, liver, or kidneys, and is in each case followed by the same act of retching,—or when a full inspiration follows the dashing of cold water on the face, breast, abdomen, or extremities.

2. Conversely, in various instances, different impressions made on *the same* parts of the body, and therefore on the same sensitive nerve, exert *different* sensations, in which case they are *not* followed by the same reflex actions. Thus certain impressions on the nostrils and face, followed by the sensation of cold or of tickling, excite the act of inspiration, but other impressions on the same parts, fully as strongly felt, but exciting different sensations, as in cutting or bruising, have no such effect; and the same is remarkably observed as to different impressions on the fauces and on the stomach, some of which excite nausea and *then* retching, while many others have no such effect. These facts plainly indicate that, in the natural state, the reflex actions, characterized as above stated, follow not the impressions on particular nerves, but the excitement of particular sensations. And it is easy to shew that many phenomena seen during sleep, or in decapitated animals (when the medulla oblongata has been left in connexion with the cord), and which have been thought indications even of well regulated reflex movements, independent of sensation, may be reconciled to the same doctrine, if we remember that sensations may be quite distinct, but momentary, and so leave no trace on the recollection.

Then it is to be remembered, that several of these reflex actions are absolutely identical with those which are excited by emotions and passions, *i. e.* by changes which are peculiar to the mental part of our constitution, as in the cases of sighing, weeping, laughing, even retching and vomiting; and again, that they are observed to be remarkably obedient to well known laws of mind. Thus they are, like the strictly voluntary actions, obedient to the law of habit, which, as applied to the mental changes preceding muscular contractions, is merely the law of association of ideas; and they are so effectually controlled by the occurrence of any very engrossing mental act,—sensation, emotion, or voluntary effort,—as plainly to imply, that they are not only attended by the consciousness, but modified by the agency, of the mental part of our constitution.

I stated and illustrated these facts, chiefly by commenting on the writings of Whytt and Monro, before the offices of the brain and the cerebellum, in animal motion, had been clearly distinguished from those of the spinal cord;* and it does not appear to me that their force is in the least impaired by the facts which have been since ascertained, touching the portions of the nervous matter with which sensation, or recollection, or any other mental act, is especially connected.

The case now before us, however, is one in which we see exemplified, not merely the power of sensations, directly, or through the intervention of other mental acts resulting from them, to *excite* muscular motion, but more especially their office in *guiding and regulating* those muscular actions which are excited through the nerves. The difference between the muscles of the eyeball and other

* See Edinburgh Medico-Chirurgical Trans. vol. ii.

muscles of the body in the respect above stated, illustrates perfectly the importance of the sensitive nerves of muscles, whether these are bound up with their motor nerves, as in most parts of the body, or separated from them, as in the face; and the importance of those muscular sensations, excited by the contraction of muscles, on the efficacy of which, as a means of acquiring knowledge, the late Dr Brown dwelt with so much earnestness and ability, but perhaps with somewhat exaggerated ideas.

The office of the sensitive nerves of the voluntary muscles in general, and of the retina and the optic nerve in the eye, in regulating the animal motions, is obviously to furnish the sensations by which the mind is guided, in selecting the muscles and portions of muscles, and in determining the degree of contraction which is requisite for the attainment of any object. And of the necessity of such a regulator in the case of the eye, we have an instructive example when one eye is affected with anaurosis, the effect of which is to prevent that insensible eye from following accurately the movements of the sound eye, when turned in different directions, and thus to cause occasional and temporary distortion. In fixing on the muscles, or portions of muscles, on which it must act, when it feels certain sensations, in order to attain certain objects, the mind sometimes merely yields to that mysterious impulse, independent both of experience and of reasoning, to which we give the name of Instinct; but in the greater number of cases, in our species, it is guided by experience and education. The sensations which result from any particular muscular action are recollected; and it is the anticipation, or rather I believe we should say the *commencing recurrence*, of these sensations, which determines the repetition of the action. Thus the faculty of memory is essential to all strictly voluntary, as distinguished from instinctive, movements; and the experiments of Flourens and of Hertwig instruct us, that it is the cerebellum, not the brain proper, which furnishes the physical conditions requisite for this recollection of muscular sensations.

Although there appears at first some difficulty in understanding how sensations which are only *anticipated*, or the *beginning* of which only is felt, can guide the contractions on which their perfect recurrence is to depend, we shall have no difficulty in conceiving this, if we recollect that it must necessarily be precisely in the same manner that a musician is enabled to go over any piece of music from recollection;—the anticipated sensation is throughout that operation the guide to the motion by which its own recurrence is to be secured.

In the performance of any such complex successions of muscular movements, we must allow that it is difficult to conceive, that there is not only a continual transmission *downwards*, perhaps to different parts of the body, of certain definite nervous actions resulting from efforts of the will,—by motor nerves,—but likewise at least as many transmissions *upwards* by the sensitive filaments, of changes

produced by the movements excited,—sensations thereby felt,—and mental determinations consequent on these, by which the successive volitions are guided. But it is admitted that, in all sciences, "Reason can sometimes go farther than Imagination can venture to follow;" and in no department of science can we more reasonably expect to meet with such examples than in tracing the actions of that exquisite mechanism, by which the sensations and powers of living animals are placed in connection with the world which is given them to inhabit.

But we may go a step farther, and understand more distinctly the mode in which sensations continually regulate and guide muscular actions, if we reflect on the phenomena to which MÜLLER has very properly directed the attention of physiologists under the name of *Consentient motions*, and of which the study of the eye furnishes us with some of the most instructive examples.

I need hardly say that this term is applied in cases where different nerves, and thereby muscles, are excited to action *simultaneously*, and where it is difficult or impossible to separate the combination. Such cases occur very frequently, both as to the strictly voluntary and the sympathetic or reflex movements, but especially as to the latter; and the following are the facts most important to be observed in regard to them.

1. The strictly voluntary motions thus simultaneously performed, are chiefly where the action that is willed requires considerable exertion, and is performed with difficulty. "Thus when we wish to contract the muscles of the external ear, we induce contraction of the occipito-frontalis muscle at the same time, without wishing it. During the most violent muscular action, many muscles act by association, although their action serves no apparent purpose. Thus a man making much exertion moves the muscles of his face, as if they aided him in lifting a load," &c.

2. In regard to most of the cerebral motor nerves, and nerves moving the trunk of the body, particularly when these act in obedience to sensation or emotion, the most important fact regarding their consentient action is, that this tendency is observed especially in *the opposite nerves of the same pairs*. Thus in the latter description of movements performed by the irides of the eyes, by the muscles of the face, by the pharynx, diaphragm, intercostal muscles, abdominal, lumbar, and perineal muscles,—in the actions of winking from bright light, of deglutition, breathing, coughing, sneezing, vomiting, laughing, sighing, weeping, —straining for evacuation of any of the viscera of the abdomen or pelvis,—it is certain, and is essential to the due performance of each action, that the corresponding portions of the nerves of the same pair, on each side of the body, should be affected, and should act on the muscles, exactly alike; and this is observed, even when the sensation exciting the movement is felt only through one nerve, and on one side of the body; as in the contraction of both pupils from bright light acting on one eye, or in the simultaneous and successive contractions of all

the muscles of respiration, in consequence of a sensation excited in one of the nostrils, or one of the bronchiæ.

3. Another fact as to these consentient movements, is satisfactorily observed *only in the eye*, but is no doubt extensively applicable in many parts, viz. that the stimulus of this consentient movement of voluntary muscles passes *through the ganglia*, and thereby affects muscles of strictly involuntary motion; the iris being distinctly observed to contract whenever the eyeball is voluntarily and forcibly rolled inwards by the action of the third nerve. And MÜLLER relates experiments in his own person, distinctly shewing that this effect takes place even on the pupil of the right eye, in consequence of forcible voluntary exertions made through the third nerve of the left eye, and when the right eyeball is not moved.

I think it impossible to doubt that MÜLLER is so far right in ascribing these phenomena to what he calls "the conducting power of the cerebral substance at the origin of the nervous fibres, whereby those which are contiguous to each other are liable to be affected simultaneously, and the influence of the will (or of any mental act) is with difficulty confined or insulated on individual fibres," or something is required to insulate it; and that these observations put us in possession of an important fact regarding the influence, either of volition or of sensation, or of the changes in the nervous matter attending these mental acts, in exciting muscular action, viz. that this influence naturally extends to some distance in the larger masses of the nervous matter, and requires the action of some additional cause, to *insulate* it on individual muscles, or portions of muscles. And in so far as the motor influence dependent on sensation is concerned, this is strictly in accordance with what is observed as to the imitation of that influence, in experiments on the reflex function in paralyzed or decapitated animals.

I think MÜLLER is also certainly right in supposing that the tendency to consentient movement in the similar or corresponding portions of any pair of nerves, is the reason why the third nerve is not employed to give the movement outward to the eyeball; two other nerves (the fourth and sixth) being employed to give this movement, because it is a movement which must always be consentient with that excited in a dissimilar part, and therefore through dissimilar nerves, on the other side of the body. And although this tendency to consentient motion is much less seen in the nerves of the same pair going to the extremities, yet MÜLLER justly observes, that the extreme difficulty always felt in rotating one arm in one direction, and the other in the opposite at the same moment, must be ascribed to the violation implied in that effort, of this tendency to consentient action in the corresponding portions of the same pairs of nerves.

But I think it also certain, particularly from what we see in the eye, that this observation goes but very little way in explaining the general phenomenon of Consentience. The tendency to consentient action in the nerves of the same pair in any part of the extremities, is so slight as to shew, that the conducting

power at the origins of these nerves cannot be very strong, and, therefore, that proximity of origin can afford but a very imperfect explanation of the very strong tendency to consentience remarked in almost all the motions of the trunk of the body. Consciousness informs us that, although it is very difficult to act at the same moment on dissimilar portions of the same pair of nerves, yet there is in general no difficulty in refraining from acting at the same moment on the corresponding portions; and in no case any difficulty in acting, at the same moment, on dissimilar and distant nerves. And there are facts observed in the eye, which have quite the value of the experimentum crucis, as shewing, that the chief cause of consentience of movement in our muscular organs is very different from the connection of nerves, at their roots or in their course. These facts are, that while those corresponding portions of the 3d nerve, which elevate and depress the eyeball, *i. e.* those which go to the superior and inferior recti, always act simultaneously; those which go the rectus internus and inferior oblique do not usually act together in the two eyes. Again, the 4th and 6th nerves never act together on the two sides of the body, but each is uniformly combined in its movement with a portion of the 3d on the other side. The reason obviously is, that the *Sensations* which result from the action of the 4th and 6th nerves of the one eye, cannot be identified with those which result from the action of the nerves of the same pair in the other eye, and cannot be separated from those which result from the action of that portion of the 3d pair in the other eye. There is no other circumstance, but the identity of the resulting and guiding sensation, which can be pointed out as existing where the consentience is observed, and not existing where it is not observed.

From these facts, therefore, we learn that the main cause of Consentience of muscular movement is simply *Identity of the Guiding Sensations.* Whether it is by an original instinct, or by repeated trials and acquired experience, that the acts of volition are directed to the nerves in each eye, which so turn the eyeballs as to keep the optic axes parallel, and so produce the single sensations, is a different question; but what has been stated seems to me quite enough to shew, that it is because the single sensations result, that these nerves are consentient.

I have no doubt that this principle, deduced from the movements of the eyeball, is strictly applicable to all the cases of consentient movement excited by the nerves of the same pairs on the face, fauces, thorax, abdomen, and pelvis, in the different actions which have been already mentioned. The movements which these nerves excite, are always followed by certain sensations, generally grateful, influenced by the degree in which the actions are performed; and by these sensations, the extent to which the actions are carried, and the energy with which they are performed, are felt to be habitually regulated. These resulting and guiding sensations are felt to be affected exactly alike by the movement which is

excited on both sides of the body; and hence we instinctively carry the movement to the same extent in both.

It was a speculation of DARWIN, that the actions of inspiration and expiration are originally determined by the uneasy sensation of anxiety in the chest of the new-born child leading to irregular and convulsive movements, out of which those are quickly *selected*, which are found by rapid experience to be effectual in appeasing that uneasy feeling; and although I do not agree to this statement, as expressing the order of events at that early period of life, and can assign no cause but Instinct for the original selection of the proper nerves and muscles for this purpose, yet I believe that, at all periods of life, it is the sensation felt to result from the action of inspiration already in progress, which determines the energy with which it shall be performed, the extent to which it shall go, and even the number of muscles that shall be excited to partake in it.

And that this is the true account of the matter, we have farther and satisfactory proof in the fact, that in various cases of disease, particularly in cases of Empyema, the contractions of the muscles of inspiration on one side of the chest become ineffectual for inflating the lungs, and for appeasing the sense of anxiety in the breast; in which case their nerves are no longer excited, and those muscles cease to act; they remain flaccid, and even, according to the observation of Dr STOKES, they gradually become paralytic from inaction; a phenomenon, as I conceive, almost exactly similar to the loss of power in some of the muscles of the eyeball in cases of amaurosis affecting one eye.

II. Again, another important application of the information acquired by study of the nerves of the eyeball, is to explain the use of the Plexuses or analogous contrivances, through which all the nerves, sensitive and motor, pass both to the upper and lower extremities, very generally in the animal kingdom.

In regard to the use of this very remarkable piece of structure, found in those nerves, by which the most forcible and the most nicely regulated muscular movements are effected, there have been various opinions. Several authors, among others Sir CHARLES BELL, have supposed it to be intended to facilitate the combinations of different muscles for particular actions, proceeding on the plausible supposition that, when the will acts simultaneously on several muscles, its influence proceeds from a single point, and is diffused from thence to those different muscles.

" The principal cause of the irregularity and seeming intricacy in the distribution of nerves, is the necessity of arranging and combining a great many muscles in the different offices. Wherever we trace nerves of motion, we find that before entering the muscles they interchange branches, and form an intricate leash of nerves, or what is called a plexus. This plexus is intricate in proportion to the number of muscles to be moved, and the *variety of combinations* into

which the muscles enter; while the filaments of nerves which go to the skin regularly diverge to their destination. From the fin of a fish to the arm of a man, the plexus increases in complexity in proportion to the variety or extent of motions to be performed by the extremity. By the interchange of filaments, the combination among the muscles is formed; not only are the classes of extensors and flexors constituted in the plexus, but all the varieties of combinations are there formed, and the curious relations established which exist between opposing muscles, or rather between the contractions of one class and the relaxation of another." In short, it appears to be his idea, that a plexus is necessary to enable a single effort of the mind to throw into action a combination of muscular contractions, and a succession of efforts to excite such a succession of these combinations as exists in every complex movement.

But the case of the muscles of the eyeball seems quite sufficient to set aside this opinion. None of these nerves on the opposite sides of the body are connected by plexuses, yet no nerves can combine their actions more perfectly or more surely. There is no more perfect consentience in the living body than that between the 6th nerve of the right eye, and the inner portion of the 3d of the left, and both are often exerted in varied combinations with many other nerves and muscles; but no nerves in the body can have less connection, so far as anatomy informs us, either at their origin or in their course.

In fact, when we reflect on what passes within us when we throw into action any two muscles at the same moment, we shall see that when such a voluntary effort is made, it is just as easy for us to excite simultaneously the most widely distant or the most closely contiguous muscles; and again, when we attend to the necessary *selection* of so many different and distant muscles, in any of the requisite combinations which are apparently under the influence of Sensation, as in coughing, sneezing, vomiting, &c. we shall perceive that, in the entire state of our faculties, any intense sensation may be said to have at its command all the muscles of the body; and although, as I have stated, I believe all mental acts to be guided by sensations in the selections which they make, yet I think it quite plain that neither proximity of origin, nor connection in their course, can be assigned as the cause of any of these selections.

I believe that Dr Monro made a nearer approach to the true statement of the use of a plexus, and put it in a simpler view, when he said, that "the chief intention of Nature in this very solicitous intermixture of the nervous fibrils, is to lessen the danger by which accidents or diseases affecting the trunks of the nerves would, without these contrivances, have been attended. Thus let us suppose, that two nerves are sufficient to supply the flexors and extensors of the forearm, it is evidently better for us that the one-half of each nerve should go to the flexors, and the other half of each to the extensors, than the whole of the first nerve should have gone to the flexors, and the whole of the second to the extensors. For if by accident

or disease one of these nerves should be cut across, or lose its powers, we should, on the first supposition, preserve one-half of the powers, both of flexion and extension, which would surely be preferable to our possessing fully the power of flexion without any power of extension. And thus, in the arm, where five trunks are found, there would on this supposition, as to the use of a plexus, be only one-fifth of the power lost, of performing any motion, by division of any one of these nerves."—(Obs. p. 45.)

That this is really the effect of this arrangement in regard to the effects of injury, appears to be sufficiently established by the experiments by PANIZZA on frogs, in which animals the plexus supplying the inferior extremities is much less intricate than in the mammalia. "If," he says, "one anterior root of the three last spinal muscles be cut, the motions of the corresponding extremity are as perfect as if the motiferous nervous system of the part had not been injured. Even if two roots be divided, although for a moment the motions are not so energetic as at first, yet they are speedily renewed, and the frog springs as if it had suffered no injury. Yet by this operation, more than two-thirds of the nervous matter which presides over the motion of the extremity is destroyed; and if the third filament is divided, all motion immediately ceases in the limb." "Whence, if I am not mistaken, appears the use of the nervous plexuses, which, by the intermixture of the filaments of different roots having a common function, establish among them, as it were, such a concentrated force, that each is adequate to preserve the integrity of the function, when, by means of any harm, the continuity of the other filaments is interrupted." (Edin. Med. and Surg. Journal, No. 126, p. 89.)

I am aware of experiments by CRONENBERG and by MÜLLER, who found that by cutting one of the nerves entering the crural plexus in the frog, they could paralyze or greatly enfeeble certain movements of a limb, and leave others unimpaired; and of the elaborate investigations of MÜLLER and others in Germany, which lead to this conclusion, that every nervous fibril, whether passing through a plexus or not, remains perfectly distinct from its origin to its termination. Notwithstanding these observations, it is distinctly admitted by MÜLLER, that "plexuses convey to each muscle of a limb fibres from different parts of the brain and spinal cord."

It seems to me, however, hardly possible to suppose, that this very carefully adjusted piece of structure is intended merely as a guard against injury, and therefore is of no use in any person or animal on whom such an injury as the section of one of the nerves of an extremity has never been inflicted. But if we advert to what has been said already of the evidence that any voluntary effort, which excites a muscle to contraction, extends its influence over *a considerable portion* of the cerebro-spinal axis, and at the same time to the evidence, in the experiments above quoted, that every muscle supplied from a plexus, has part of its motor nerves, and may be excited to contraction, from each of the nerves en-

tering that plexus, we can hardly miss the conclusion, that this contrivance not merely provides against injury, but *multiplies the power* which acts on each of these muscles, and enables the mind to *vary* the degree of energy which it can expend on each, in a degree much greater than in any case where it can act on a muscle only from a single point of the spinal cord.

Then, if we remember farther, that by means of the plexus, each *sensitive* nerve which supplies any muscle of the extremities, consists of fibrils coming from different points of the cord, we can easily perceive that, by this arrangement, the sensations resulting from each portion of the muscle may be more distinct, and more easily discriminated from each other, than those which are excited by nervous fibrils bound in the same sheath throughout their course, and originating beside each other in the cord.

Thus the effect and use of a plexus will be, to make the muscular sensations more precise and distinct, and to make the power which the will can exert over the muscles greater, and capable of greater increase at pleasure, than where such arrangement does not exist; and therefore, to increase the force and precision with which the efforts of volition may be directed and insulated on the muscles which are thus supplied with nerves. And I think that any one who attends to the subject may observe that he is actually conscious of these differences, when he compares the effects of his voluntary exertions in his extremities with the motions of his head and trunk.

I think, therefore, that Sir CHARLES BELL was right in asserting that the plexus enables the acts of the will to form combinations of muscular motions for definite ends, in greater variety and with greater precision than they otherwise could: but I apprehend the reason to be, not that each combination is effected by an impulse emanating from a single point, nor that the different combinations are *formed* in the plexus, but that the plexus, rendering the muscular sensations more distinct, and the acts of the will more energetic, enables the mind to act on all the muscles thus supplied with more power and precision, and to recollect and resume the action at any subsequent time with more certainty and uniformity, and thus facilitates combinations.

III. Let us next attend to the information given by the study of the nerves of the eye, as to the influence and use of the Ganglia of the Sympathetic nerve, of which it is generally admitted that the ciliary ganglion, furnishing the ciliary nerves, and through which the iris is moved, is a specimen and representative.

On this subject there has been much discussion at different times, which may be set aside as irrelevant or hypothetical, because proceeding on the supposition, that part of the office of the sympathetic, as of other nerves, is to *give* the vital power or energy to the muscles it supplies. It has always seemed to me extremely improbable, that any one of the solid textures of the living body should

have for its office to *give to any other*, the power of taking on any vital action; and that the only doctrine on this subject which involves no hypothesis, is that of HALLER, who regarded every part of the body which is endowed with irritability, as possessing that property in itself, but subject to excitement and to control, of one kind or another, from the nervous system; and the nervous system as exercising that control chiefly, and in the natural and healthy state probably only, in so far as it is the seat and the instrument of mental acts.

This doctrine, excluding the larger masses of the nervous system from all share in bestowing the property of irritability or vital energy on muscles, has received, as it seems to me, the only confirmation of which, in the present state of our knowledge, it stood in need, from the experiments of Dr REID, which were laid before the British Association in 1834, and have since been repeated on warm-blooded as well as cold-blooded animals. These experiments prove, that after the irritability of muscles has been, as nearly as possible, extinguished by irritation, it is perfectly recovered by rest, notwithstanding that all their connections with the brain and spinal cord have been cut.

There is, however, nothing hypothetical or visionary in the assertion as to the nerves, that " Soli in corpore, Mentis sunt ministri;" and, therefore, when we observe that all the great organs of involuntary motion, and among others the iris, have nerves which have passed through ganglia, and when we remember that all those organs are beyond the power of the *will*, but are peculiarly liable to control from certain *involuntary* acts of Mind, particularly from Sensations and Emotions, our business is to inquire whether there is any thing in the structure of those parts of the nervous system which can be supposed to unfit them for the one of those offices, and fit them for the other. And if we keep steadily in mind this precise object of our inquiries, we shall find the subject less obscure and intricate than it has often been thought.

When it is stated that the nerves which pass through the Ciliary Ganglion supply the only muscle in the eyeball, the actions of which are truly involuntary,—that all the truly involuntary muscles of the body have in like manner nerves which pass through ganglia,—and, farther, that all these ganglia appear, from the most recent and careful examination, to be, like the ciliary ganglion, formed of filaments both from motor and sensitive nerves, it is impossible to doubt, that much of what can be ascertained as to the office of this ganglion in the eye, must be truly applicable to the other ganglia supplying involuntary muscles in the body.

If we were to assert, however, that all nerves which excite involuntary movements in the body, in obedience to sensation or emotion, are ganglionic nerves, or that it is through ganglia only, that these involuntary acts of mind affect the body, we shall be immediately met by various examples of sensations (or the ner-

vous actions which attend sensations) certainly exciting movements through motor nerves destitute of ganglia. Of this, the portio dura and phrenic nerve furnish sufficient examples.

But setting aside the supposition that the ganglia are necessary to enable the involuntary affections of mind to act on the muscles, let us inquire how far the opinion long ago stated by Dr JOHNSTON and others is correct,—that the ganglia intercept the influence of the Will,—prevent the voluntary acts of mind from acting on the muscles which have their nerves only through them.

A decided opinion is given against this supposition, both by MÜLLER, and by his very intelligent translator Dr BALY. The reason given by MÜLLER is this, that as we know from the experiment formerly mentioned of forcibly acting on the muscles of the eyeball, and thereby causing contraction of the iris, that *a motor* influence can traverse the ciliary ganglion, there is no reason to suppose that *a voluntary motor* influence should be arrested in it, if really brought to it. He considers it, therefore, more probable, that the fibres of the "sympathetic, at their origin in the spinal cord and brain, are not in communication with the source of the voluntary influence;" *i. e.* that they are not set on the fibres by which the will acts downwards from the source of voluntary power; to which Dr BALY adds, that to suppose the admixture of other fibres in the sympathetic to have the effect of removing the motor cerebro-spinal nerves from the action of the will, is in opposition to one of the fundamental principles in physiology, that of the course and influence of nerves in their "peripheral part," *i. e.* at a distance from the brain and spinal cord, being *insulated,*—*i. e.* admitting of no admixture or transference of power from one filament to another. These authors, therefore, regard the ciliary nerves as beyond the influence of the will, by reason of the mode of their origin, not of their passing through the ciliary ganglion.

But, on the other hand, if we attend to the experiment insisted on by MÜLLER, we shall see that its result is not correctly stated by his expression, that it shews that a *motor* influence can be transmitted through a ganglion, and therefore gives us reason to presume that an effort of volition could traverse the ganglion also, if really carried to it. When the 3d nerve transmits an effort of volition to the muscles of the eyeball, and at the same time causes contraction of the pupil, it is plain that the influence which affects the iris has originated in the "source of *voluntary* influence" in the brain,—that it is not only a motor influence, but one consequent on a voluntary effort, which has traversed the ciliary ganglion. The ganglion has not prevented the influence of volition from acting on the nerves and muscular fibres which it supplies, although the will has no power of regulating the movement of these fibres; and this being so, I do not see how it can be denied that it has modified, in one way or other, the endowments of the nerves entering it; rendering them incapable, not of transmitting the influence of the volition, but of obeying any specific efforts of the will.

In fact, if it were in consequence of their roots having no connection with the motor portion of the brain and spinal cord, that the ganglionic nerves in the eye or elsewhere are not obedient to the will, and if the nerves underwent no change of endowment in the ganglia, we do not see why the motor nerves of the involuntary muscles (*e. g.* the motor filaments of the ciliary nerves) should pass through ganglia at all; they would be fitted for their function merely by their mode of origin.

Nor does it seem to me difficult to define a little more precisely the modes in which, in this as in other instances, by the connection established in every one of the ganglia of the sympathetic between motor filaments from the anterior, and sensitive filaments from the posterior, column of the spinal cord, the involuntary muscles, although we believe them to be supplied with motor nerves through the ganglia, are withdrawn from the power of the will.

1. Even if we implicitly rely on the experiments of VALENTIN and others in Germany, tending to correct the previous statements of HALLER, BICHAT, WILSON PHILIP, MAYO, and many others, and to shew that all the involuntary muscles may, under certain circumstances, be excited by physical irritations applied to their nerves,*—yet I think it cannot be doubted (from the negative result of so many experiments made previously by so many experienced physiologists) that the power of the motor nerves to excite muscular contraction is *greatly diminished* by passing through ganglia. The contractions, so excited in involuntary muscles in these experiments, have followed irritation *above* the ganglia, or even in the central masses, much more surely than in the nerves *below* the ganglia; and their force, and the certainty with which they can be produced, are certainly much inferior to those of the contractions excited by similar means through nerves not ganglionic, *i. e.* voluntary muscles.

2. The vital agency of the *sensitive* nerves passing through the ganglia seems also to be much modified; they certainly do not shew on irritation, when in the natural state, nearly as much sensibility as other nerves; and their grand peculiarity seems to be, that although supplying the muscular fibres, they are incapable of transmitting those muscular sensations by which, in the case of the voluntary muscles, we are continually informed of the contractions we excite. Although the study of the eye teaches us that the influence of volition can traverse a ganglion, yet in no one instance in the body is this influence *felt to be exerted* on muscles placed beyond ganglia. And when we reflect on what has been said of the importance of the resulting and guiding sensations, in *insulating and directing* the efforts of the will, we shall easily perceive that the want of any such sensations in the present case, is sufficient to explain the inefficiency of voluntary efforts over those muscles. These seem to be results of the degree of intermixture

* See Valentin De Functionibus Nervorum, &c, p. 62.

of the motor and sensitive filaments (with the interposition of grey matter), which takes place in the ganglia, instead of taking place at the extremities of the nervous filaments in the muscular fibres themselves.

It is very well worthy of notice that there is one action of the eye, in which the ciliary nerves are essentially concerned, and in which there is a distinct *resulting sensation* consequent on their action, and that in that action the ciliary nerves and the iris may be said to act in obedience to the will: I mean that still mysterious effort, whereby the eye increases its own refracting power, and so enables the rays from an object brought gradually nearer it, to form a distinct image on the retina and excite a distinct sensation in the mind; which effort is uniformly coincident with a gradual contraction of the pupil. Here an effort of volition is made in the direction of the eye, and the continued gratification of the sense, resulting from that effort, in so far as it affects the refractive power, seems to act the same part there, as the gratification of the sensations in the chest, in regulating the contractions of the muscles of respiration.

However, I am aware that objections may be stated to these speculations; and probably it is wiser to rest at present on the general inference, deducible from a comparison of the ganglionic nerves of the eye and of other parts, that when the sensitive and motor filaments which connect a muscle with the spinal cord, meet in a ganglion before reaching the cord, their endowments are so far modified that the sensations thence resulting are rendered less precise; that the efforts of the will cannot be insulated on such a muscle, and, therefore, although capable of being influenced by the will, it is truly involuntary.

But it is obviously part of the design of Nature, in the construction of the ganglionic nerves, not only that they should withdraw the muscles they supply from the dominion of the will, but likewise that they should facilitate and increase upon them the power of what I have elsewhere called Sensorial Influence, *i. e.* the influence attending or resulting from Sensations and Emotions of mind, which we know to originate or to be excited exclusively in the larger masses of the nervous system, and to act with peculiar power on muscles and other organs which have their nerves through the ganglia. Here also the study of the eye gives us important information.

The ordinary action of the iris, in obedience to the stimulus of light, is certainly effected by a reflex action, in which the optic nerve, the corpora quadrigemina, and the 3d nerve are concerned, and which has been fully illustrated by the experiments of MAYO, FLOURENS, VALENTIN, and others. That the peculiar sensation of light, excited by the impression on the corpora quadrigemina, not only attends the action but regulates its degree, is at least highly probable; although it is right to admit, that the action occurs occasionally in cases of amaurosis, where the patient expresses himself as conscious of no sensation; and I do not think that there is so good evidence of the necessary interposition of mental changes in this action, performed by an involuntary muscle, as in the cases where

selected and regulated contractions of voluntary muscles are excited by the reflex function of the cord, as, *e. g.* in the contraction of the orbicularis oculi and of this muscle only, effected through the 7th nerve, on the same sensation being felt.

As the 3d nerve appears to have roots in the posterior as well as anterior portion of the crus cerebri, it is certainly quite possible that those of its filaments which enter the lenticular ganglion are set on sensitive, not on motor portions of the cerebro-spinal axis; but if so, the observations already made shew that they are capable of being excited by an influence acting downwards from the strictly motor portions.

The indirect and probably modified influence, resulting from volition, and transmitted through the ganglia to the involuntary muscles, and of which we have this unequivocal example in the eye, is in itself in all probability an important part of the design of Nature in the construction of the sympathetic nerve and its ganglia. I perfectly agree with MÜLLER, that it is in this way only, that the effect of muscular exercise on the action of the heart, and much of the beneficial strengthening effect of exercise, is to be explained; and this indirect influence of voluntary muscular exertion on the heart is obviously important, as keeping its actions in unison with any occasionally required increase of voluntary muscular exertion; and so enabling us to keep up exertions which must otherwise have failed. And a slighter degree of the same indirect influence of exercise is seen in the movements of the stomach and intestines, which become to a certain degree torpid from inactivity of the voluntary muscles. For this slighter agency of voluntary exertion on the moving organs supplied by the splanchnic nerves, there is probably provision made, in these nerves passing through a greater number of ganglia, before they reach the moving fibres, than the nerves of the heart, and therefore having the indirect influence of the voluntary efforts transmitted through them in a less degree of intensity.

But it is very important, in reference to the use of the ganglionic nerves, to observe, that the movement of the iris is capable of being affected, not only through the 3d nerve, but likewise through the 5th nerve and the sympathetic, *i. e.* by all the filaments which form part of the composition of the ciliary ganglion. I shall not enter on the observations which have been made on the differences observed in different muscles in this respect; nor on the speculations of some German physiologists as to the mode of action, particularly of the sympathetic, on the iris; but only observe that the effect chiefly observed from the section of both these nerves on the iris, is a gradual and permanent contraction of the pupil. The influence of both these nerves on the iris is therefore strictly analogous to the kind of influence observed in experiments on animals, from injury of different parts of the nervous system, or the sympathetic nerve, on other involuntary muscles, consisting, as MÜLLER states, " either in enduring contractions, or in a long-continued modification of the ordinary rhythmic action of the organ;" a

change, *e. g.* in the number and rapidity of the beats of the heart, or of the peristaltic movements of the intestines; in short, as HALLER long ago expressed it, a change of the property of irritability itself, as resident in these muscular organs.

Now, when we apply these observations generally, to the living actions of those muscles which have their nerves from the sympathetic, I think we can be at no loss as to the use of great part, at least, of the structure of this part of the nervous system. These nerves place the organs which they supply in connexion with the whole extent of the cerebro-spinal axis; we know, from the observations now stated as to the iris, that an influence may be transmitted to these organs through any of the nerves entering any one of the ganglia; we know, from such experiments as those of LE GALLOIS and Dr WILSON PHILIP, as well as from the effects of injuries on the human body, that injuries acting on any large portions of the brain or spinal cord, affect the heart at least, if not other of these organs, nearly alike; we know that, in the natural state, all these organs are peculiarly under the control of what I have called *sensorial influence*, *i. e.* an influence resulting from those changes in the nervous system which attend intense sensations and emotions of mind; we know, from various facts, some of which I have elsewhere collected,* that this sensorial influence, although often originating from an impression made on a single point, extends itself rapidly in different directions through the nervous matter, and that it can cross from the sensitive portions of the nervous matter to the motor portions, probably at any part of the spinal cord. The effect of any arrangement which brings a particular muscle into communication with many points of the cord, must be still more decided in regard to this sensorial influence, than as to the influence of volition as affected by a plexus. The purpose of the multiplied origins of the spinal accessory nerve, which appears, from the experiments of VALENTIN and others, to transmit an influence to a greater number of nerves, connected with the cervical plexus, than had been formerly suspected, and therefore to be essentially concerned in many complex actions consequent on sensation and emotion, is thus easily understood. Some observations already published by Dr REID, shew more precisely that in the case of the heart, just as in the case of the iris, the sensorial influence, or one exactly similar to it, affecting the contractile power of the muscle, may be transmitted through different nerves entering the ganglia, and so passing to the muscles; for he found that a violent blow on the head influenced the actions of the heart much less, when the sympathetic and par vagum were cut in the neck, than when these nerves were entire, shewing that a part of that influence passes through these nerves; and on the other hand, he found that when an animal in which these nerves had been cut was under the impression of fear, its heart's actions were quickened nearly in the usual way; shewing that another part of that influence must pass through

* Outlines of Physiology, p. 398.

other nerves. It seems impossible to miss the conclusion, that the arrangements and the communications of those ganglionic nerves are designed and adapted, according to the laws of nervous action,—while they intercept the direct influence of the Will,—to multiply and concentrate, on all the organs they supply, that equally certain, equally important, and more varied and extended influence which results from Sensations and Emotions of mind. And I think it appears clearly, from what has been said, that these are objects which the arrangements of this part of the nervous system must necessarily be so disposed as to secure.

IV. The last question which I shall here consider as elucidated by what we observe in the eye, relates to the mode of transmission of that Sensorial influence, resulting, in the natural state, from mental sensations and emotions, which affects the organic functions of Nutrition and Secretion, and, in all probability, the vital properties and composition of the blood itself, in all parts of the body.

It has been long known that the lacrymal gland is supplied so completely by the fifth nerve, that it must be through a branch of this nerve, almost exclusively, that the passions of the mind, or the sensation of pain excited in other parts of the body, must produce their effects on the flow of tears; and the experiments of MAGENDIE, in which inflammation and ulceration of the conjunctiva and cornea, and ultimate collapse of the eye, followed section of this nerve, and some cases presenting the same series of phenomena in the human body (of which I have myself seen two), have shewn that the nutrition of the whole eyeball, and especially the secretion of mucus on the conjunctiva, are under the control of this nerve. It is hardly necessary to say, that the common expression of this nerve "presiding over these functions," is vague and unsatisfactory; but that it is the nerve destined to affect these functions, in the way in which nature intends them to be affected by changes in the nervous system, is sufficiently obvious; and is another general principle derived from observations on the eye, and manifestly applicable to the nerves of common sensation all over the body. I have formerly stated a conjecture, which I still think the most probable explanation of the inflammation excited by disease or section of this nerve, viz. that the sensitive nerve, which Sir C. BELL has well denominated the "guard of the organ," having thus lost its power, the irritations which, in the natural state, are applied to the mucous membrane, and by an action there, attended with sensation, determine a sufficient flow of the natural protecting mucus, now lose their effect, and the membrane is reduced nearly to the condition of a serous membrane, and inflames (as all serous membranes do), merely from the contact of the air.

This influence of sensitive nerves and of sensations, and this consequence of the want of such influence, I take to be an important point in the physiology of other mucous membranes as well as this; but we are moreimmediately concern ed

with the question, in what manner the fifth nerve is qualified for transmitting downwards the effect which sensations, even in distant parts of the body, and emotions or passions of the mind, have on the circulation through the eye, and on all its secretions.

The instance of the lacrymal gland, and of the mamma, (which, according to the dissections of MÜLLER, has its nerves merely from the intercostals, to the exclusion of the sympathetic,) are enough to shew, that the most intense agency of mental emotion may take place through the nerves of common sensation.

I think Dr MARSHALL HALL has good reason for the opinion which he has stated, that as the nerves which supply most of the internal organs of secretion, and of organic life in general, are ganglionic, and as the circulation in the eye itself is liable to influence from section of the sympathetic nerve as well as of the 5th, it is probable that the Gasserian ganglion, and the ganglia on the sensitive roots of the spinal nerves generally, must be designed for the influence of these nerves on secretion and nutrition, not for their functions in regard to sensation; but it seems to me much more doubtful, whether MÜLLER is right in his conjecture, that the *grey matter* of the ganglia, and the grey fibres passing from them along the nerves, are the parts of the nervous system designed exclusively to affect the organic functions of secretion and nutrition. There are no experiments to shew any such peculiar power in the grey matter of the nervous system; and I can state one fact which shews unequivocally that if it is, as MÜLLER supposes, through the grey matter in the Gasserian ganglion, and of the branches of the sympathetic which communicate, beyond that ganglion, with the fifth nerve, that any emotions or sensations affect the secretions of the eye, that grey matter must itself be acted on by the substance of the fifth nerve behind the ganglion. For in one of the cases of palsy, affecting the fifth nerve on one side, which was long under observation in the clinical ward, it was quite obvious that neither emotions of mind, nor sensations excited in the sound nostril, or in other parts of the body, affected the eye of the palsied side, which, although inflamed, remained always dry when the other was suffused on such occasions. Now, in this case it was ultimately ascertained by dissection, that the diseased (and ultimately wasted) portion of the nerve was behind the Gasserian ganglion, between it and the origin on the crus cerebelli; from which it appears quite certain, that the influence of mental sensation and emotion must pass downwards through this portion of the nerve (which I believe hardly contains any grey fibres) on its way from the sensorium commune to the eyeball.

Whatever may be the use of the grey matter in the ganglia, or in other parts of the nervous system, I think we cannot doubt that there is here a grand exception to the principle which has been laid down by several authors, that the same nerve is never employed to convey impressions upwards to the sensorium and downwards to the extremities of the nerves. At least, if there be a set of nerves

destined solely to convey the influence of sensation and emotion downwards to the organs of organic life, these nerves are every where bound up in the same sheath with the nerves of common sensation, by which impressions are carried upwards to the brain.

Thus the study of the nerves of the eyeball enables us, I think, to give a decided opinion as to the following points:—

1. That all strictly animal muscular movement is not only excited, directly or indirectly, by Sensations producing it, but is continually guided and regulated by sensations which succeed and result from it.

2. That it is the province of these resulting sensations, *commencing or anticipated*, to determine on individual muscles the influence of the Will; and where distinct animal movements are always consentient, it is because the sensations thus guiding them are the same.

3. That neither the connections of nerves at their roots (so far as anatomy has detected them), nor the Plexuses which they form in their course, can be assigned as the cause of consentience of their movements, or of any combinations of their actions; but that the plexuses of nerves, placing both the sentient and motor nerves of the muscles of the extremities in connection with a large surface of the spinal cord, seem to be designed and fitted to render the muscular sensations more distinct, and the acts of the will more energetic than they otherwise would have been, and thereby to give power, facility, and precision to the combinations and successions of muscular contractions in all movements of the limbs.

4. That the action in nervous matter which is excited by an act of the will, can traverse a Ganglion, but is never felt to be exercised, and therefore cannot be applied to any specific object, beyond it, apparently because of a modification of the endowments, both of sensitive and motor filaments of nerves, where they are subdivided and intermixed with the grey matter of a ganglion.

5. That the motor filaments of nerves which have passed through ganglia may be affected by changes in the sensitive as well as the motor filaments which enter the ganglia; and that in this way, probably, the influence of sensations and emotions of mind (which must be transmitted through the ganglia, because it affects especially muscles which have only ganglionic nerves) is conveyed from many parts of the spinal cord, and concentrated on the muscles of organic life.

6. That the influence of changes in the nervous system, and especially of such as accompany sensations and emotions of mind, on the capillary circulation, on the functions of nutrition and secretion, and on the properties of the blood, may be transmitted downwards by the nerves of common sensation, and that it is probably with a view to this influence that the ganglia are formed on the roots of those nerves.

V.—*Notice of the Fossil Fishes found in the Old Red-Sandstone formation of Orkney, particularly of an undescribed species, Diplopterus Agassis.* By Dr T. S. Traill, *F.R.S.E.*

(Read 21st December 1840.)

It is well known to those who have paid attention to the progress of Fossil Ichthyology that, until the publication of M. Agassiz, the distinctive characters of the orders, genera, and species of Fossil Fishes were but imperfectly understood. Vague analogies were relied on to connect them with the types of living genera, and the looseness of the received specific characters rendered it difficult for the geologist to determine whether the specimens he collected were previously recognised, or still nondescript. It is obvious that useful characters of fossil species are chiefly to be obtained from those portions of their structure least subject to alteration from decay; and as the exterior scaly envelopes of the primeval fishes are usually the portions best preserved and most easily recognised in their rocky sepulchres, M. Agassiz was naturally led to study these with minute attention. This acute observer speedily discovered that, in the form and connections of the scales, he had a general character which would enable him to connect into very natural groups, species differing from each other in size and form. On this basis he has established his four *Orders* of Fossil Fishes—the *Ganoidei*, the *Placoidei*, the *Ctenoidei*, and the *Cyclodei*—divisions named from the appearance of the scales.

The bones of the body, especially of the head and the teeth, are often found in a state of high preservation, especially in our schistose rocks; in the layers of which the general form of the specimen is easily recognisable. M. Agassiz has subdivided his Orders into several *Families*, also natural groups, founded chiefly on the form and position of the teeth, the disposition of the scales around the body, the osseous or fibrous structure of the skeleton, and the general form of the body of the fish.

These Families, judging by their living analogies, present other natural groups, which he has considered as *genera*; the principal characters of which are drawn, as in existing fishes, from the number, form, and position of their fins, which are often preserved, even in the most delicate articulations of their rays, with wonderful precision,—from the structure of the tail, the principal organ of progression,—from the arrangement of the teeth,—the form of the bones of the head, and the manner in which the vertebral column is terminated.

The character of the *species* are drawn by this philosophic inquirer principally from the general form and size of the fish,—the external surface of its scales,—their relative size on the different parts of its body,—the nature of the rays of the fins, especially of the first ray,—the form and size of the opercula or gill-covers, and of the bones of the head.

When M. AGASSIZ first visited this kingdom, I submitted to him a considerable collection, which I had made, of the fossil fishes from the old red-sandstone formation of Orkney; among which he instantly recognised several new species, and at least one genus, to him then totally unknown, to which he assigned the generic name of DIPLOPTERUS; but the species has remained to this day undescribed. I have lately understood that a *Diplopterus* has been found in another part of Scotland, and one in Ireland; but whether identical with the Orkney species I am unable to decide. Assuming to myself the privilege usually conceded to the finder of a new species, and desirous of connecting with my country the name of the celebrated naturalist, who has done so much to elucidate its Fossil Ichthyology, I some time ago proposed to designate this species DIPLOPTERUS AGASSIS, under which name I have already sent specimens of it to several Geological Collections; and now beg to present a specimen of it to the Royal Society, along with some other fossil fishes from the same county.

During the late visit of the philosopher of Neufchatel to this country, I was enabled to shew him an additional series of specimens from Orkney; and he has now ascertained that my collection contains the following fossil species from those islands:—

1. Osteolepis Macrolepidotus.
2. ——— Microlepidotus.
3. Cheirolepis Traillii.
4. Cheiracanthus Minor.
5. Diplopterus Agassis.
6. Diplocanthus Crassissimus.
7. Dipterus Macrolepidotus.
8. Platygnathus Paucidens.
9. Coccosteus Latus,
10. Pterichthys—Milleri?

In a short memoir which was read to the Royal Society of Edinburgh in 1833, and to the British Association in 1834, I stated that fossil fishes were found in great number and finely preserved in a quarry at *Skaill*, on the western coast of Pomona, the largest of the Orkney Islands, at about two miles to the north of a granite ridge which traverses part of the island for six miles. This, with a small patch in the adjacent isle of Græmsey, is the only granite in Orkney.

The whole of that group, with these exceptions, consists of rocks which I consider as belonging to the old sandstone formation.

As it is important to determine the geological position of these fishes, I shall here give an abridgment of my notes on the geology of Orkney.

In this formation, massive sandstone, both red and yellowish, occurs in Hoy, in Edey, at Holland-head and Getnip in Pomona. The principal rocks in all those islands is a distinct sandstone-flag. In two or three points, as at Yesnaby in Pomona, and at How in Shapinshey, thin beds of limestone occur; and at several places the sandstone-flag passes into a slaty-clay, occasionally impregnated with bitumen, as near Skaill, at Yesnaby, in Walls, and in the rock of Ruskholm, off Westrey. The rocks are in some places intersected by trap-dykes. The largest of these occurs in Hoy, opposite to Stromness: Several are found along the coast from Breckness to Skaill, and in Shapinshey, on its southern shores, and at Longhope in Walls. From this sketch it will be seen that the geological formation of Orkney is very simple and little varied.

The granite ridge on its eastern side appears here and there to pass into gneiss, and in one point I observed a limited extent of mica-slate; but in the greatest part of its course, the rock immediately in contact with the granite is a conglomerate with a sandstone base, containing fragments of these primary rocks. This conglomerate passes by insensible shades into sandstone-flag, which has often a dark iron-grey colour, from containing bitumen and oxide of iron.

It is between the layers of this sandstone-flag that the fossil fishes are found, where it is quite schistose, and in fact is quarried in large slabs, varying from half an inch to twelve inches in thickness. The fossil fishes at Skaill do not occur in the upper layers of this rock. I observed in the quarry about three feet of soil and debris of the rock, then nine feet of solid stone-beds; and below these two other thick beds of flag, in which the fishes are found. This was the lowest point to which the quarry was wrought at the period of my visits in 1833 and 1834. I found, in the same beds with the fishes, a few fossil plants, which seem to be Algæ or Fuci.

From geological position, from its connection with the massive sandstone, and its vicinity to the granite ridge, I consider this flag to belong to the old sandstone formation; which is confirmed by its identity with the Caithness flag in appearance and in fossil remains. In fact, the organic remains would indicate that this flag is very low in the series of that formation. Since 1834, fossil fishes have been found in several other parts of Pomona. They are no less numerous at *Breckness*, six or seven miles farther south than Skaill; and fine specimens have more lately been found in a quarry at *Quoyloo*, a mile to the north of the first locality: I found scales, which I now consider as those of *Platygnathus*, near Kirkwall, a distance of fourteen miles east of that point: Very fine specimens of fossil fishes have also been found in the same species of flag at Hoxahead in the

island of South Ronaldshey, which is twenty miles south-east from Skaill in a direct line; and I found a few scales like those of Kirkwall in the little island of Papey-Westrey, which lies twenty-four miles north of the original quarry. From these facts I have no doubt that attentive examination would detect fossil fishes in many other parts of that group of islands.

The generic character of *Diplopterus* is,

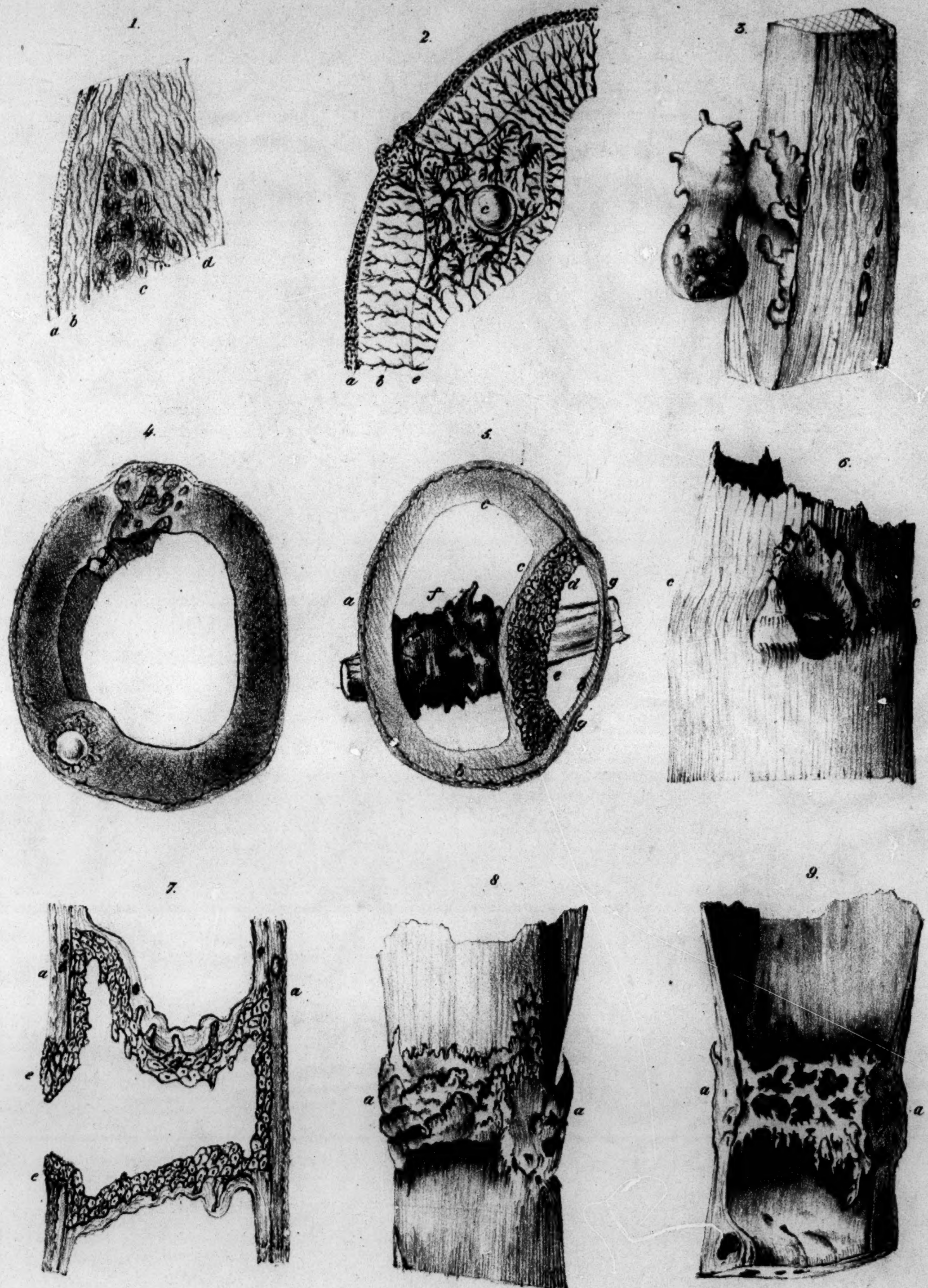
Two equal dorsal fins opposite to two similar anal fins; vertebral column continued into the upper lobe of an even tail; mouth wide, armed with strong conical teeth.

This fish belongs to the order *Ganoidei*, and to the second family of that order, the *Sauroidei*.

It is distinguished from *Dipterus* by the largeness of its mouth, and the form of its tail;—from *Palæoniscus* by the double dorsal and anal fins, and the nearly even extremity of its tail. I shall not attempt to anticipate M. Agassiz in a full account of the Orkney *Diplopterus*, which I know he is fully prepared to describe; but content myself with stating, that this species may be known by its wide mouth, rounded snout, and large head, which forms nearly one-fourth of its whole length, and is covered with large scales. A single row of moderately large trigonal scales, with posterior convex edges (giving them a somewhat hatchet shape), passes along the ridge of the back; and from their sides proceed rows of lengthened rhomboidal scales obliquely downwards, diminishing in size from the back toward the abdomen. The scales are neither groved nor granulated, but covered with a smooth shining enamel.

The dorsal and anal fins are large, rounded at their tips, and, like the lower lobe of the tail, supported by numerous slender rays.

PLATE I.



VI.—*On the Mode in which Musket-Bullets and other Foreign Bodies become inclosed in the Ivory of the Tusks of the Elephant.* By JOHN GOODSIR, Esq. *M. W. S. Communicated by Professor Syme.*

(Read 18th January 1841.)

MUSKET-BULLETS are occasionally found inclosed in ivory, and every anatomical museum contains specimens of this kind. Why bullets should be so frequently met with in this situation, it is not easy to say; the head of the animal appears to be generally aimed at, and foreign bodies when they enter the tusks, instead of being removed in the usual manner, are retained by the process, an investigation of which is to form the subject of the present paper.

My attention was directed to this subject by Mr SYME, who submitted to me for examination some highly interesting specimens of bullets in ivory, presented to the Anatomical Museum of the University by Sir JOHN ROBISON. Sir JOHN has also kindly afforded me an opportunity of examining some remarkable examples of wounded ivory, and Sir GEORGE BALLINGALL has directed my attention to preparations in his possession, which have satisfied me of the truth of those opinions on the subject, which I shall now have the honour of submitting to the Society.*

One circumstance was at once detected in all these specimens, and its importance was evident, as affording a clew to the explanation of the mode of inclosure. The circumstance to which I allude is, that in none of the specimens are the bullets or foreign bodies surrounded by regular ivory. They are in every instance inclosed in masses, more or less bulky, of a substance which, although abnormal in the tusk of the elephant, is nevertheless well known to the comparative anatomist, as occupying the interior of the teeth of some of the other mammals, and usually considered to be ossified pulp. It was evident that the pulp had ossified round the bullet, as the first step towards the separation of the latter from it. In one specimen the bullet has become enveloped in a hollow sphere of this substance, on the surface of which the orifices of medullary canals are situated. In other specimens the irregular ivory, which surrounds the balls, had become smooth on its surface, the medullary canals had disappeared, and the regular ivory had been formed in a continuous layer over the surface of the mass. In

* We are indebted for the specimens to the liberality of Mr RODGERS of Sheffield, who transmitted to Sir JOHN ROBISON for examination, these as well as many other most remarkable examples of wounded and diseased tusks.

one tusk a cicatrix was seen occupying the hole through which the ball had passed, a circumstance which, when seen in similar specimens, has greatly perplexed anatomists. It was observed, however, that, in this instance, the shot had passed through that part of the tusk which had been within the socket; and bearing in mind that the tusk is an organ of double growth, it appeared probable that the shot had been plugged up from within by the ossified pulp, and from without by the continued growth of cement, without any regeneration of the displaced ivory; a hypothesis which was afterwards verified by examination.

Before proceeding to give a more detailed account of this interesting process, I shall state very briefly the opinions of those authors who have written on the subject, so as to ascertain how near they had approached to the truth, and to point out the fallacies which had led them astray.

KLOCKNER mentions a ball of gold which was found by a turner of Amsterdam in the substance of an elephant's tusk. The longitudinal fibres of the tusk surrounded the metal in an irregular manner, and were separated from the sound ivory by a concentric chink situated at some distance from the ball.

CAMPER in the "Description Anatomique d'un Elephant Male," remarks, that it is not unusual to see foreign bodies inclosed, or as it were soldered, into the substance of the ivory. The same anatomist also figures and describes a bullet which was inclosed in a very irregular mass of ivory, covered with long appendages, which were directed parallel to the axis of the tusk. The metallic bodies in question, he remarks, must have penetrated across the alveolus into the hollow of the tusk, and must have remained for a long time in the substance of the pulpy flesh which fills that cavity, because the ivory enveloped them on all sides, and would at length have carried them beyond the alveolus by the increase of the tooth. He supposes that the nodules which are formed around the balls, and the very incomplete union of their fibres with the sound ivory, add weight to this conjecture.

RUYSCH in his X. Thesaurus, Plate II., figures brass and iron bullets inclosed in isolated nodules of irregular ivory.

BLUMENBACH considers the tusks of the elephant to differ from other teeth, more particularly in the remarkable pathological phenomenon of bullets, with which the animal has been shot, being found, on sawing through the tusk, imbedded in its substance in a peculiar manner. He looks upon this fact as important in reference to the doctrine of a "nutritio ultra vasa." He mentions a tusk, equal in size to a man's thigh, in which an unflattened leaden bullet lay close to the cavity of the tooth, surrounded by a peculiar covering, and the entrance from without closed as it were by a cicatrix. From these facts BLUMENBACH concludes that the elephant's tusk, when fractured or perforated, can pour out an ossific juice to repair the injury.

Mr LAWRENCE, in his Notes to BLUMENBACH's Comparative Anatomy, over-

looking those cases (one of which is given in the text of his author) in which cicatrices have been seen filling up the orifices produced by balls, explains satisfactorily enough those instances in which no such cicatrices exist, and concludes by denying the power of the ivory to throw out ossific matter as asserted by BLUMENBACH.

The author of the Ossemens Fossiles, in his chapter on the structure, development, and diseases of the tusks of the elephant, after stating that grooves and notches on the surfaces of the tusks never fill up, and only disappear from the effects of friction, allows that musket-balls are found in ivory without any apparent hole by which they could have entered. He does not believe that the holes are filled up with ossific deposition as HALLER and BLUMENBACH supposed; but maintains that they are never obliterated. He states that the ivory on the outside of the ball is natural, and that it is only the bone surrounding it which is irregular. The phenomena are to be explained, he says, by supposing the balls to penetrate the very thin bases of tusks in young elephants, so as to enter the pulps when still in a growing state.

There appear, then, to be two circumstances, regarding which great doubts still exist—first, whether a shot-hole is ever closed up; and, secondly, how this is accomplished in a non-vascular substance like ivory.

In proceeding to consider this subject, two facts must be borne in mind in reference to a tusk. The first is, that the two substances of which it is composed, ivory and cement, undergo no change of form or arrangement from vital action, after they are once deposited; the second, that it is an organ of double growth—it is endogenous as well as exogenous, the ivory being formed from without inwards, the cement from within outwards.

As there are certain processes which invariably commence when a foreign body passes through or lodges in the pulp, it will facilitate the conception of the mode in which a bullet is inclosed if these be described first.

Recent researches have proved that the regular ivory of teeth is formed by the cells on the surface of the pulp becoming solid from the deposition of earthy salts in their walls and cavities. It is evident from this that, when a portion of the surface of the tusk-pulp is destroyed by the passage of a ball, the formation of ivory at that spot must cease. But we know that the formation of irregular ivory commences, which indicates the existence of a healing process in the pulp. The mode in which the wounded pulp heals, cannot be ascertained; but it is accomplished probably by effusion and subsequent absorption of blood, deposition of lymph, and regeneration of the peculiar tissue of the pulp. So far this process is conjectural, but the irregular ivory formed by the regenerated pulp is the subject of observation. When the ball passes quite across the pulp the track heals, but does not necessarily ossify, except in the immediate neighbourhood of the ivory.

There are two exceptions, however, to the non-ossification of the track of the ball, namely, the ossification which takes place round the bullet, and that which occurs round the whole or any portion of the track, which may suppurate and form a sinus or abscess. In both these cases deposition of irregular ivory takes place, assuming the same characters as the irregular masses which appear at the two extremities of the track of the ball through the pulp.

The ossification round the ball generally assumes the form of a hollow sphere. Its surface exhibits a number of holes (which are the orifices of medullary canals), and these are occasionally prolonged through stalactitic-looking processes, which lie in the direction of the axis of the tooth. The ossification surrounding an abscess or sinus assumes the appearance of a shell of variable thickness, and directed towards one or both of the shot-holes.

When thin sections of this irregular ivory are examined under the microscope, it is seen to consist of a transparent matrix, in which exist numerous medullary canals, shewing traces of dried pulp in their interior. From these canals, which correspond to the Haversian canals of true bone, secondary medullary canals, similar to those in the teeth of certain fishes, radiate. The sides and extremities of these secondary medullary canals send off numerous minute tubes, which are true Retzian tubes, and similar to those in the regular ivory, but not so closely set. These Retzian tubes have a general radiating direction, and proceed in irregular wavy bundles, which sweep past one another without mingling, but branching particularly at their extremities. The great central medullary canals are very numerous, and each of them has its own system of secondary canals and Retzian tubes.

These individual systems, when seen in a mass of irregular ivory, appear globular or spindle-shaped; when viewed in section, they resemble circular or oval opaque spots with a hole in the centre. These individual systems, however, are not isolated; for they communicate, first, by means of the central canals, which constitute an inosculating system; and, secondly, by the ramifying extremities of the Retzian tubes, which communicate through the medium of cells more or less minute, and which are more numerous in some places than in others.

The formation of the irregular ivory does not go on indefinitely: a limit is set to its increase, and the changes which ensue at this stage of the process are highly interesting. I have already mentioned the existence of the orifices of Haversian, or medullary canals on the surface of the mass of irregular ivory. When the further formation of this is to terminate, these orifices are gradually closed, and appear like imperforated projections on the surface. It is evident, therefore, that the inclosed vascular contents of the canals, that is to say, the ramified processes of the tusk-pulp in the irregular ivory, are cut off from the system. They dry up, and the formation of ivory in the interior ceases. The peripheral surface of the irregular ivory is now, in reference to the general pulp, in

the same relation as the whole internal surface of the irregular ivory of the tusk. The pulp, therefore, becomes converted into ivory, not only on the whole internal surface of the tusk, but also on the surface of the newly-formed mass. The cause of the formation of the irregular ivory to a limited extent only, when it exists as an abnormal structure, I have not been able to ascertain; but its mode of development and limitation is highly interesting, and forms a leading distinction between a tooth and a true bone under similar circumstances.

From this description it is evident that the abnormal ivory in the elephant's tusk strongly resembles, if it be not identical with, the peculiar substance which fills the pulp-cavities of the tusks of the walrus and the teeth of the cetacea, first announced as a distinct species of dentar tissue in a paper read before this Society five years ago by Dr Knox, and since minutely described by Retzius, Owen, and Alexander Nasmyth.*

This identity of a diseased structure in one animal with a normal structure in another is remarkable, and must be looked upon as another instance indicating the existence of a system of laws regulating the relations between healthy and morbid tissues;—laws which have been speculated upon, but have never been sufficiently investigated by anatomists.

Having now given the anatomical characters of the abnormal ivory which invariably surrounds musket-bullets and other foreign bodies which lodge in the pulps of the tusks of the elephant, I shall proceed to state the various conditions under which these enter the organ, and the changes which ensue.

Foreign bodies enter the tusk in three ways: First, through the free portion of the tusk; secondly, through that part of the organ which is contained in the socket; and, thirdly, from above through the base of the pulp.

First, When the ball hits the free portion of the tusk, if it only penetrates to a certain depth of the ivory, no change whatsoever can take place. Neither the cement nor the ivory can be reproduced. In course of time the hole may be obliterated, the ball may be got rid of by wearing down of the ivory, and the ivory

* Cuvier described this species of dental tissue in the tusk of the walrus, and compared it to pudding-stone. Dr Knox, in the paper to which I have referred in the text, affirmed that, in addition to the cement, enamel, and ivory, a fourth substance, namely, the substance described by Cuvier, entered into the formation of many teeth. He stated that, in the teeth of certain fishes, this substance, or a tissue closely allied to it, constituted the greater part of their mass; the other three elements having disappeared or become greatly diminished in bulk or importance. Retzius has accurately described the microscopic structure of this class of dental substances, as existing in different animals. Mr Owen has extended and confirmed the observations of Retzius. Lastly, to Mr A. Nasmyth belongs the merit of having pointed out the resemblance which this kind of substance (which he denominates ossified pulp) bears to diseased ivory in the tusks of the elephant, and still more closely to the substance which fills the pulp cavity of the aged human tooth. In ignorance of Dr Knox's previous observations, he announced this kind of ivory as a fourth dental substance.

under the hole may be strengthened by the formation of new substance. When the ball is detained by the ivory, but penetrates so far as to wound the pulp, the latter ossifies round it, and the ossified portion sooner or later becomes enveloped in new ivory. If the ball penetrates the pulp, the latter ossifies round it, and becomes attached to the hole in the ivory. If the tusk is growing rapidly, and the nucleus of pulp-bone does not speedily adhere to it, the ball will ultimately be situated above the hole. The ball may also pass across the pulp, and become at last enveloped, along with its bony envelope, in the ivory of the opposite wall.

Second, In the second class of wounds, in which the ball enters the pulp-cavity through the socket and side of the tusk, the consequent changes seem to be the following: first, ossification of the pulp surrounding the ball, and the ultimate application of the mass to the hole in the ivory, and, as the latter is necessarily at this part of its extent very thin, the hole is closed; second, the application to the hole in the ivory, and to the surface of the ossified pulp in it, of cement formed by the internal surface of the tusk-follicle. For although the ball may have removed or at least torn the follicle opposite the hole in the ivory, yet, as the tooth advances in the socket, the ball will in time arrive at a sound portion of the latter. There is a specimen on the table which proves that the wounded portion of the follicle may perform this duty sufficiently well. In this specimen the external surface of the cement exhibits a longitudinal fissure, with smooth rounded edges, resulting from the defective formation of cement in the situation of a longitudinal rent or wound in the membrane of the follicle, through which the ball had entered the ivory. The hole in the ivory then being plugged up externally by cement, and internally by ossified pulp, the case proceeds as in the last class of wounds,—the ossified portion of the pulp surrounding the ball becoming inclosed in true ivory.

Third, When the foreign body enters from above, without wounding the tusk, the pulp ossifies round it, and true ivory envelopes the mass, in the usual manner. I have not seen any morbid ivory which could be referred to wounds of the class now under consideration; but a very interesting account is given by Mr COMB, in the Philosophical Transactions,* of a tusk in which a spear-head was found, and which could only have entered the cavity from the base of the pulp. Mr COMB describes and figures the ossified portion of the pulp, and the manner in which it had attached itself to the ivory, and become covered by it, so as to obliterate partially, and to alter the relative width of the pulp-cavity.

The description I have now given of the changes which ensue on wounds of the tusks of the elephant, explains many curious appearances in ivory, and the difficulties anatomists and physiologists have had in understanding them. It explains the drawings and descriptions of KLOCKNER, RUYSCH, and CAMPER; does away with the necessity of supposing, with BLUMENBACH, that true ivory is regenerated,

* Phil. Trans. 1801.

or that it can throw out ossific juice to produce cicatrices; and leads us to believe that CUVIER, in denying the possibility of the obliteration of a shot-hole, had allowed himself to be deceived. All difficulties are got over, and contradictions reconciled, by bearing in mind the different circumstances insisted upon in this paper, namely,

1. That a tusk is an endogenous as well as an exogenous organ.

2. That the pulp forms irregular ivory round foreign bodies, and at wounds on its surface.

3. That the membrane of the follicle is an important agent in closing up the holes produced by foreign bodies which penetrate a tusk through the socket.

EXPLANATION OF PLATE I.

Fig. 1. A portion of a section of a wounded tusk; *a* cement; *b* regular ivory deposited previous to the wound; *c* irregular ivory deposited after the wound.

Fig. 2. A diagram illustrative of the mode of connection between the Retzian tubes of the primary and secondary regular ivory, and the cells and Retzian tubes of the different mosculating systems of the irregular ivory, after inclosure of a ball; *a* cement with its osseous corpuscles; *b* primary regular ivory with its Retzian tubes; *c* the ball; *d* the irregular ivory with its systems of tubes and cells; *e* secondary regular ivory.

Fig. 3. A copper ball inclosed in a sphere of irregular ivory, on the surface of which are the orifices of Haversian canals. Some of the orifices have closed, and present the appearance of irregular projections. The mass has begun to be attached to the regular ivory of the tusk, and would in time have been inclosed in it. The ball must either have passed across from the opposite side of the tusk, or must have sunk below the level of the hole by which it entered.

Fig. 4. Section of a tusk across the cavity of which a ball has passed, and become inclosed in the ivory of the wall opposite the hole by which it entered. The hole is filled with irregular ivory, coated externally with cement. The cement over the ball has been disarranged by the shock. This section proves that the track of a ball across the pulp is not necessarily ossified.

Fig. 5. Section of a tusk across the base of which a spear-head has penetrated and remained in the wound. The weapon has therefore been separated from the pulp by deposition of irregular ivory in the form of a tube; *a* cement; *b b* irregular ivory deposited previous to the wound; *c c* regular ivory deposited after the wound; *d* irregular ivory inclosing a vacant space *e*, the seat of an abscess or sinus, and continuous with the cavity of *f*, a mass of irregular ivory (coated with regular ivory) in the form of a tube surrounding the foreign body. As irregular ivory always contracts in drying, more than any other kind of dental substance, that portion of the section marked *g g* has been bent outwards.

Fig. 6. The same section viewed in profile; *a* the broken shaft of the spear; *b* an irregular mass of cement formed round the orifice of the wound by the membrane of the tusk follicle, and which would have closed the wound had the weapon been removed. The wound inflicted has in this instance, as in many others, stunted the growth of the tusk at *c c*, so as to render the part formed after the injury narrower and weaker.

Fig. 7. A longitudinal section of a tusk in which a gun-shot wound had terminated in abscess of the pulp; *a a* cement; *b b* regular ivory deposited before the injury; *c c* regular ivory deposited after the in-

jury; *d d* irregular ivory bounding the abscess; *e e* masses of cement and irregular ivory at the margin of the shot-hole.

Fig. 8. The external aspect of a portion of a tusk, which had been transversely fractured; *a a* the line of fracture united externally by irregular masses of cement.

Fig. 9. The internal aspect of the same portion of tusk; *a a* the line of fracture united by irregular ivory, a portion of which is arranged in a reticular form. This reticular ivory is interesting, as affording a natural analysis of the peculiar arrangement of parts in the irregular ivory described in the paper. Each bar of the reticular ivory is traversed longitudinally by a medullary canal, from which radiate secondary canals and Retzian tubes, the whole being coated with regular ivory. This reticular ivory differs from the ordinary form of ossified pulp, only in the greater distance between the Haversian or medullary canals, so that portions of the pulp have remained unossified between them.

VII.—*On the Theory of Waves. Part II. By* The Rev. P. KELLAND, *M.A., F.R.SS.L. & E., F.C.P.S., late Fellow of Queens' College, Cambridge; Professor of Mathematics, &c., in the University of Edinburgh.*

(Read 18th January 1841.)

THE problems on which we were engaged in the preceding Memoir were the following:—1°, The complete determination of the velocity of transmission of a wave in a fluid of any depth, provided the depth is uniform throughout; 2°, an application of the hypothesis of parallel sections to the problem in its more complicated form, in which the depth is variable in the direction of the breadth of the canal; 3°, The investigation of the motion of a solitary wave. We propose in the following pages to continue the same subjects, and to add the more difficult problem of initial motion.

The principle on which the determination of the velocity was made to depend is this very simple one, that the variation of pressure in proceeding from point to point in the direction of motion is the same, whether we suppose the pressure a function of x and y, or a function also of z. The former hypothesis refers its determination to the general properties of fluids, the latter to the particular state of the fluid in question at the time. Art. 9.

This principle is a highly important one, but it requires a slight degree of attention in its application. I deem it, therefore, not superfluous, in discussing a question of so great importance as that now before us, to present other solutions, in which the same principle is viewed in a totally different light. Having done this, I shall give a complete solution of the motion of a wave in a canal of any section, and an approximate one of the motion in a canal of variable depth. Lastly, I shall consider the question of initial motion, not only in the case in which the depth is small, but in the most general case.

SECTION IV.

46. We retain the following notation, which corresponds with that used in Art. 20, &c.

$$u=b\,(e^{\alpha y}+e^{-\alpha y})\sin\alpha(x-c\,t)=b\,(e^{\alpha y}+e^{-\alpha y})\sin\theta \qquad (1)$$

$$v=-b\,(e^{\alpha y}-e^{-\alpha y})\cos\alpha\,(x-ct)=-b(e^{\alpha y}-e^{-\alpha y})\cos\theta \qquad (2)$$

$$z=h+\frac{b}{c\,\alpha}(e^{\alpha z}-e^{-\alpha z})\sin\theta \qquad (3)$$

The equations of motion are (Art. 8.),

$$\frac{dp}{dx}=\varrho\left(-\left(\frac{du}{dt}\right)\right) \qquad (4)$$

$$\frac{dp}{dy}=\varrho\left(-g-\left(\frac{dv}{dt}\right)\right) \qquad (5)$$

$$\frac{du}{dx}+\frac{dv}{dy}=0. \qquad (6)$$

We proceed anew to the discussion of these equations, reserving only the solution of the problem given in Art. 8.

By (4) and (5), $\frac{d}{dy}\left(\frac{du}{dt}\right)=\frac{d}{dx}\left(\frac{dv}{dt}\right)$ . . . . . (7);

the differential coefficients which have the symbols enclosed in brackets being *complete*, the others *partial*.

Hence $\quad \frac{d}{dy}\left(\frac{du}{dt}+u\frac{du}{dx}+v\frac{du}{dy}\right)=\frac{d}{dx}\left(\frac{dv}{dt}+u\frac{dv}{dx}+v\frac{dv}{dy}\right).$

But $\quad \frac{d}{dy}\,\frac{du}{dt}=\frac{d}{dt}\cdot\frac{du}{dy}$, &c.=&c.

$\therefore \quad \frac{d}{dy}\left(\frac{du}{dt}+u\frac{du}{dx}+v\frac{du}{dy}\right)$ may be written

$$\frac{d}{dt}\cdot\frac{du}{dy}+u\frac{d^2u}{dx\,dy}+\frac{du}{dx}\cdot\frac{du}{dy}+v\,\frac{d^2u}{dy^2}+\frac{du}{dy}\cdot\frac{dv}{dy}.$$

Similarly, the quantity $\frac{d}{dx}\left(\frac{dv}{dt}+u\frac{dv}{dx}+v\frac{dv}{dy}\right)$ may be written

$$\frac{d}{dt}\cdot\frac{dv}{dx}+u\frac{d^2v}{dx^2}+\frac{du}{dx}\cdot\frac{dv}{dx}+v\,\frac{d^2v}{dx\,dy}+\frac{dv}{dx}\cdot\frac{dv}{dy}.$$

Now $\left(\frac{d}{dt}\right)\frac{du}{dy}=\frac{d}{dt}\cdot\frac{du}{dy}+\frac{d}{dx}\cdot\frac{du}{dy}u+\frac{d}{dy}\cdot\frac{du}{dy}v$, which is less than the quantity

which represents $\frac{d}{dy}\left(\frac{du}{dt}\right)$ by $\frac{du}{dx}\cdot\frac{du}{dy}+\frac{du}{dy}\cdot\frac{dv}{dy}$ or by $\frac{du}{dy}\cdot\left(\frac{du}{dx}+\frac{dv}{dy}\right)$.

But $\frac{du}{dx}+\frac{dv}{dy}=0$ by (6); therefore the quantities $\frac{d}{dy}\left(\frac{du}{dt}\right)$ and $\left(\frac{d}{dt}\right)\frac{du}{dy}$ are coincident.

Again, $\left(\frac{d}{dt}\right)\frac{dv}{dx}=\frac{d}{dt}\cdot\frac{dv}{dx}+\frac{d^2v}{dx^2}\cdot u+\frac{d^2v}{dx\,dy}\cdot v$, which is less than the quantity which represents $\frac{d}{dx}\left(\frac{dv}{dt}\right)$ by $\frac{du}{dx}\,\frac{dv}{dx}+\frac{dv}{dx}\,\frac{dv}{dy}$ or by $\frac{dv}{dx}\left(\frac{du}{dx}+\frac{dv}{dy}\right)$; that is by 0 (6).

$$\therefore\quad \frac{d}{dx}\left(\frac{dv}{dt}\right) \text{ and } \left(\frac{d}{dt}\right)\frac{dv}{dx} \text{ are coincident.}$$

Hence equation (7) gives

$$\left(\frac{d}{dt}\right)\cdot\frac{du}{dy}=\left(\frac{d}{dt}\right)\cdot\frac{dv}{dx};$$

which, by integration, becomes

$$\frac{du}{dy}=\frac{dv}{dx}+\mathrm{C} \quad . \quad . \quad . \quad . \quad (8)$$

This is the condition which we obtain, therefore, when p is a complete differential of x and y. The other condition (6) is general, and must hold in all cases of fluid motion.

47. We do not purpose to solve these equations, having already done so in Art. 8. We proceed, then, to the discussion of the equations (4) and (5).

If C=0, as is the case when the solution assumes the form given it in Art. 8, or when the motion is oscillatory, we perceive that $u\,dx+v\,dy$ is a complete differential of x and y, t being considered constant. Call it $d\phi$.

$$\therefore\quad u=\frac{d\phi}{dx},\quad v=\frac{d\phi}{dy}$$

and
$$\frac{dp}{\rho}=-g\,dy-\left(\frac{du}{dt}+u\frac{du}{dx}+v\frac{du}{dy}\right)dx-\left(\frac{dv}{dt}+u\frac{dv}{dx}+v\frac{dv}{dy}\right)dy$$

$$=-g\,dy-\left(\frac{d^2\phi}{dt\,dx}+\frac{d\phi}{dx}\cdot\frac{d^2\phi}{dx^2}+\frac{d\phi}{dy}\,\frac{d^2\phi}{dx\,dy}\right)dx$$

$$-\left(\frac{d^2\phi}{dt\,dy}+\frac{d\phi}{dx}\cdot\frac{d^2\phi}{dx\,dy}+\frac{d\phi}{dy}\cdot\frac{d^2\phi}{dy^2}\right)dy$$

$$=-g\,dy-\left\{\frac{d\phi}{dx}\,\frac{d^2\phi}{dx^2}\cdot dx+\frac{d\phi}{dx}\,\frac{d^2\phi}{dy\,dx}dy+\frac{d\phi}{dy}\,\frac{d^2\phi}{dy^2}dy+\frac{d\phi}{dy}\,\frac{d^2\phi}{dx\,dy}dx\right\}$$

$$-\left(\frac{d^2\phi}{dx\,dt}dx+\frac{d^2\phi}{dy\,dt}dy\right)$$

$$=-g\,dy-\tfrac{1}{2}d\cdot\left\{\left(\frac{d\phi}{dx}\right)^2+\left(\frac{d\phi}{dy}\right)^2\right\}-d\cdot\frac{d\phi}{dt}$$

all the differentials except that of y being performed as though t were constant.

By integration, then, we obtain

$$p=f(t)-g\,y-\frac{1}{2}(u^2+v^2)-\frac{d\phi}{dt} \quad . \quad . \quad (9);$$

where $f(t)$ is an arbitrary function of the time.

To obtain the pressure at the surface, we must write z instead of y.

48. Call P the pressure at the surface corresponding to a point, whose co-ordinates are x and z; then $\mathrm{P}+\frac{d\mathrm{P}}{dx}\delta x+$ &c. is the pressure at another point of the surface where the co-ordinates are $x+\delta x$, $z+\frac{dz}{dx}\delta x+$ &c.

But the pressure at the surface is constant; consequently $\frac{d\mathrm{P}}{dx}=0$.

Let $u_{\prime}$, $v_{\prime}$, $\frac{d\phi_{\prime}}{dt}$ &c., represent the values of u, v, $\frac{d\phi}{dt}$ &c. at the surface, or when z is substituted for y.

then
$$\frac{d\mathrm{P}}{dx}=-g\frac{dz}{dx}-\left(u_{\prime}\frac{du_{\prime}}{dx}+u_{\prime}\frac{du_{\prime}}{dz}\frac{dz}{dx}+v_{\prime}\frac{dv_{\prime}}{dx}\right.$$
$$\left.+v_{\prime}\frac{dv_{\prime}}{dz}\cdot\frac{dz}{dx}\right)-\frac{d}{dx}\cdot\frac{d\phi_{\prime}}{dt}-\frac{d}{dz}\frac{d\phi_{\prime}}{dt}\cdot\frac{dz}{dx}.$$

Hence
$$\left(g+u_{\prime}\frac{du_{\prime}}{dz}+v_{\prime}\frac{dv_{\prime}}{dz}+\frac{d}{dz}\cdot\frac{d\phi_{\prime}}{dt}\right)\frac{dz}{dx}+u_{\prime}\frac{du_{\prime}}{dx}+v_{\prime}\frac{dv_{\prime}}{dx}+\frac{d}{dx}\cdot\frac{d\phi_{\prime}}{dt}=0;$$

an equation evidently coinciding with equation (7) Art. 9.

49. We have still another mode of obtaining the same result.

In fact, let us consider the pressure as a function of t, the time. If then we admit that the solution of the problem is that given in equations (1), (2), and (3), we must have $\frac{d\phi}{dx}=b(e^{\alpha y}+e^{-\alpha y})\sin\theta$, and consequently

$$\phi=-\frac{b}{a}(e^{\alpha y}+e^{-\alpha y})\cos\theta+\mathrm{F}(t) \quad . \quad . \quad . \quad (10.)$$

But as ϕ enters merely as a symbolical abbreviation of a certain quantity, we can take it of such a value that $\mathrm{F}(t)=0$.

With this value of ϕ, it is evident that the value of P, given by equation (9), will become $\mathrm{P}=f(t)-gh+$ a circular function of $x-ct$.

But the pressure at the surface must of necessity be constant; we obtain, therefore, $f(t)-gh+\mathrm{F}\left\{\begin{matrix}\sin\\ \cos\end{matrix}(x-ct)\right\}=$ a constant, for all values of x and t. Now this cannot be the case unless $f(t)$ is a constant quantity; for by giving to x such values that $x-ct$ shall remain constant or differ by multiples of 2π, we may render all the expression except $f(t)$ constant for *all* values of t. Hence for such values of x the expression assumes the form $f(t)$ + a const. = a const. for all values of t. But this requires that $f(t)$ be constant for all values of t.

We have therefore $\quad \mathrm{P}=\mathrm{C}-gz-\frac{1}{2}(u_{\prime}^2+v_{\prime}^2)-\frac{d\phi_{\prime}}{dt}.$

Differentiating this with respect to t we obtain

$$\frac{d\mathrm{P}}{dt}=-g\,v_{,}-\left\{u_{,}\left(\frac{du_{,}}{dt}\right)+v_{,}\left(\frac{dv_{,}}{dt}\right)\right\}-\left(\frac{d}{dt}\right)\frac{d\phi_{,}}{dt}=0$$

$$\therefore\qquad g\,v_{,}+u_{,}\frac{du_{,}}{dt}+u_{,}^2\frac{du_{,}}{dx}+u_{,}v_{,}\frac{du_{,}}{dz}+v_{,}\frac{dv_{,}}{dt}+u_{,}v_{,}\frac{dv_{,}}{dz}$$

$$+v_{,}^2\frac{dv_{,}}{dz}+\frac{d^2\phi_{,}}{dt^2}+\frac{d^2\phi_{,}}{dx\,dt}u_{,}+\frac{d^2\phi_{,}}{dz\,dt}v_{,}=0$$

or

$$g\,v_{,}+2\,u_{,}\frac{du_{,}}{dt}+2\,v_{,}\frac{dv_{,}}{dt}+2\,u_{,}v_{,}\frac{dv_{,}}{dx}+(u_{,}^2-v_{,}^2)\frac{du_{,}}{dx}+\frac{d^2\phi_{,}}{dt^2}=0 \qquad . \quad . \qquad (11.)$$

The last equation is obtained by substituting for $\frac{d^2\phi_{,}}{dx\,dt}$, $\frac{d^2\phi_{,}}{dz\,dt}$, their values $\frac{du_{,}}{dt}$, $\frac{dv_{,}}{dt}$, and by writing $-\frac{du_{,}}{dx}$ for $\frac{dv_{,}}{dz}$, and $\frac{dv_{,}}{dx}$ for $\frac{du_{,}}{dz}$ . . . (6 and 8).

It is remarkable that this equation, which is the condition that the pressure at the surface shall be constant, does not contain any differential with respect to z explicitly.

By taking the small terms of an analogous equation, M. CAUCHY has obtained results corresponding to the particular hypothesis, that the depth is infinite.

50. Let us substitute in our equation (11) the values of $u_{,}$ and $v_{,}$ given by equations (1) and (2), and that of $\phi_{,}$ given by (10). By denoting $e^{az}+e^{-az}$ by S and $e^{az}-e^{-az}$ by D, we get

$$-g\,b\,\mathrm{D}\cos\theta-b^2 a\,.\,4\sin 2\theta\,.\,c-2\,b^3 a\,\mathrm{S}\,.\,\mathrm{D}^2\sin^2\theta\cos\theta$$

$$-b^3(e^{2az}-e^{-2az})\,\mathrm{S}\,a\cos 2\theta\cos\theta+b\,a\,c^2\,\mathrm{S}\cos\theta+2\,b^3 a\,\mathrm{S}\cos\theta=0\,;$$

or

$$c^2\mathrm{S}-8\,b\,c\sin\theta-b^2\mathrm{S}\,(e^{2az}+e^{-2az}-2)\,(1-\cos 2\theta)$$

$$-b^2(e^{2az}+e^{-2az})\,\mathrm{S}\cos 2\theta+2\,b^2\mathrm{S}=\frac{g}{a}\mathrm{D}$$

or

$$c^2\mathrm{S}-8\,b\,c\sin\theta-b^2\mathrm{S}\,(e^{2az}+e^{-2az})+4\,b^2\mathrm{S}-2\,b^2\mathrm{S}\cos 2\theta=\frac{g}{a}\mathrm{D}$$

or

$$c^2\mathrm{S}-8\,b\,c\sin\theta-b^2(e^{3az}+e^{-3az})+3\,b^2\mathrm{S}-2\,b^2\mathrm{S}\cos 2\theta=\frac{g}{a}\mathrm{D}$$

which equation is identical with that at the end of Art. 20.

51. Thus we have three distinct but equivalent processes, by means of which the same equation may be arrived at. It will not be worth while to follow the different processes through the solution of the general problem. The result in Art. 19 is in a form sufficiently simple. We propose rather to apply the formula (11) to another case, that in which the velocity is expressed to two terms, of which the second is not in the same phase as the first. Let us in fact assume

$$u=b\,(e^{\alpha y}+e^{-\alpha y})\sin\theta+a\,(e^{3\alpha y}+e^{-3\alpha y})\sin(3\theta+k)$$

$$v=-b\,(e^{\alpha y}-e^{-\alpha y})\cos\theta-a\,(e^{3\alpha y}-e^{-3\alpha y})\cos(3\theta+k)$$

then
$$\phi=-\frac{b}{\alpha}(e^{\alpha y}+e^{-\alpha y})\cos\theta-\frac{a}{3\alpha}(e^{3\alpha y}+e^{-3\alpha y})\cos(3\theta+k).$$

By substituting these values of u, v, ϕ when $y=z$, *i. e.* of $u_{,}$, $v_{,}$, $\phi_{,}$ in equation (11) and writing S_2 for $e^{2\alpha z}+e^{-2\alpha z}$, &c. we get after dividing by α;

$$-\frac{g}{\alpha}\{b\,D\cos\theta+a\,D_3\cos(3\theta+k)\}-2c\{b\,S\sin\theta+a\,S_3\sin(3\theta+k)\}\{b\,S\cos\theta+3a\,S_3\cos(3\theta+k)\}$$
$$+2c\{b\,D\cos\theta+a\,D_3\cos(3\theta+k)\}\{b\,D\sin\theta+3a\,D_3\sin(3\theta+k)\}$$
$$-2\{b\,S\sin\theta+a\,S_3\sin(3\theta+k)\}\{b\,D\cos\theta+a\,D_3\cos(3\theta+k)\}\times\{b\,D\sin\theta+3a\,D_3\sin(3\theta+k)\}$$
$$+\{b^2S^2\sin^2\theta+2\,b\,a\,S\,S_3\sin\theta\sin(3\theta+k)-b^2D^2\cos^2\theta-2\,b\,a\,D\,D_3\cos\theta\cos(3\theta+k)$$
$$\times\{b\,S\cos\theta+3\,a\,S_3\cos(3\theta+k)\}$$
$$+b\,c^2\,S\cos\theta+3\,a\,c^2\,S_3\cos(3\theta+k)=0,\ \text{omitting } a^2\,b.$$

That is,

$$-\frac{g}{\alpha}\{b\,D\cos\theta+a\,D_3\cos(3\theta+k)\}$$
$$-2\,c\{4\,b^2\sin\theta\cos\theta+S\,S_3\,a\,b\,(3\sin\theta\cos\overline{3\theta+k}+\cos\theta\sin\overline{3\theta+k})$$
$$-D\,D_3\,a\,b\,(3\cos\theta\sin\overline{3\theta+k}+\sin\theta\cos\overline{3\theta+k})+12\,a^2\sin\overline{3\theta+k}\cos\overline{3\theta+k}\}$$
$$-2\,b^3\,S\,D^2\sin^2\theta\cos\theta-2\,a\,b^2\,D\sin\theta\,(S\,D_3\sin\theta\cos\overline{3\theta+k}+S_3\,D\cos\theta\sin\overline{3\theta+k})$$
$$-6\,a\,b^2\,S\,D\,D_3\sin\theta\cos\theta\sin(3\theta+k)+b^3\,S^3\sin^2\theta\cos\theta-b^3\,D^2\,S\cos^3\theta$$
$$+2\,b^2\,a\,S^2\,S_3\sin\theta\cos\theta\sin(3\theta+k)-2\,b^2\,a\,S\,D\,D_3\cos^2\theta\cos\overline{3\theta+k}+3\,b^2\,a\,S^2\,S_3\sin^2\theta\cos\overline{3\theta+k}$$
$$-3\,b^2\,a\,D^2\,S_3\cos^2\theta\cos\overline{3\theta+k}+b\,c^2\,S\cos\theta+3\,a\,c^2\,S_3\cos\overline{3\theta+k}=0.$$

52. In this expression, we will seek out the different coefficients of b^2, $b^3\,a$, &c. and then collect them into one sum.

The coefficient of b^2 is $-4\,c\sin 2\theta$;

that of b^3 is
$$\begin{aligned}
&-2\,S\,D^2\sin^2\theta\cos\theta+S^3\sin^2\theta\cos\theta-D^2\,S\cos^3\theta\\
&=S\sin^2\theta\cos\theta\,(S^2-D^2)-D^2\,S\cos\theta\,(\cos^2\theta+\sin^2\theta)\\
&=4\,S\sin^2\theta\cos\theta-D^2\,S\cos\theta\\
&=2\,S\sin 2\theta\sin\theta-D^2\,S\cos\theta\\
&=S\,(\cos\theta-\cos 3\theta)-D^2\,S\cos\theta\\
&=S\cos\theta-S\cos 3\theta-S_3\cos\theta+S\cos\theta\\
&=(2\,S-S_3)\cos\theta-S\cos 3\theta;
\end{aligned}$$

that of $a\,b\,c$ is $\quad -2\,S\,S_3\,(3\sin\theta\cos\overline{3\,\theta+k}+\cos\theta\sin\overline{3\,\theta+k})$

$$+2\,D\,D_3\,(3\cos\theta\sin\overline{3\,\theta+k}+\sin\theta\cos\overline{3\,\theta+k})$$

$$=-2\,(S_4+S_2)\,(3\sin\theta\cos\overline{3\,\theta+k}+\cos\theta\sin\overline{3\,\theta+k})$$

$$+2\,(S_4-S_2)\,(3\cos\theta\sin\overline{3\,\theta+k}+\sin\theta\cos\overline{3\,\theta+k})$$

$$=6\,S_4\sin\overline{2\,\theta+k}-6\,S_2\sin\overline{4\,\theta+k}-2\,S_4\sin\overline{2\,\theta+k}-2\,S_2\sin\overline{4\,\theta+k}$$

$$=4\,S_4\sin(2\,\theta+k)-8\,S_2\sin(4\,\theta+k)\,;$$

and, lastly, that of $a\,b^2$ is

$$-2\,D\sin\theta\,(S\,D_3\sin\theta\cos\overline{3\,\theta+k}+S_3\,D\cos\theta\sin\overline{3\,\theta+k})-6\,S\,D\,D_3\sin\theta\cos\theta\sin\overline{3\,\theta+k}$$

$$+2\,S^2\,S_3\sin\theta\cos\theta\sin\overline{3\,\theta+k}-2\,S\,D\,D_3\cos^2\theta\cos\overline{3\,\theta+k}$$

$$+3\,S^2\,S_3\sin^2\theta\cos\overline{3\,\theta+k}-3\,D^2\,S_3\cos^2\theta\cos\overline{3\,\theta+k}$$

$$=-(S_5-S)\,2\sin^2\theta\cos\overline{3\,\theta+k}-2\,(S_5+S-2\,S_3)\sin\theta\cos\theta\sin\overline{3\,\theta+k}$$

$$-6\,(S_5-S)\sin\theta\cos\theta\sin\overline{3\,\theta+k}+2\,(S_5+S+2\,S_3)\sin\theta\cos\theta\sin\overline{3\,\theta+k}$$

$$-2\,(S_5-S)\cos^2\theta\cos\overline{3\,\theta+k}$$

$$+3\,(S_5+2\,S_3+S)\sin^2\theta\cos\overline{3\,\theta+k}-3\,(S_5-2\,S_3+S)\cos^2\theta\cos\overline{3\,\theta+k}$$

$$=-2\,(S_5-S)\cos\overline{3\,\theta+k}-(6\,S_5-8\,S_3-6\,S)\sin\theta\cos\theta\sin\overline{3\,\theta+k}$$

$$+6\,S_3\cos\overline{3\,\theta+k}-3\,(S_5+S)\cos 2\,\theta\cos\overline{3\,\theta+k}$$

$$=-2\,(S_5-3\,S_3-S)\cos\overline{3\theta+k}-(3\,S_5-4\,S_3-3\,S)\sin 2\theta\sin\overline{3\theta+k}-3\,(S_5+S)\cos 2\theta\cos\overline{3\theta+k}$$

$$=-2\,(S_5-3\,S_3-S)\cos\overline{3\,\theta+k}-3\,S_5\cos\overline{\theta+k}-3\,S\cos\overline{5\,\theta+k}+2\,S_3\,(\cos\overline{\theta+k}-\cos\overline{5\,\theta+k})$$

$$=-2\,(S_5-3\,S_3-S)\cos\overline{3\,\theta+k}-(3\,S_5-2\,S_3)\cos\overline{\theta+k}-(2\,S_3+3\,S)\cos\overline{5\,\theta+k}$$

Hence, collecting all the terms, we get

$$-\frac{g}{a}(b\,D\cos\theta+a\,D_3\cos\overline{3\,\theta+k})-4\,b^2\,c\sin 2\,\theta+\{(2\,S-S_3)\cos\theta-S\cos 3\,\theta\}\,b^3$$

$$-a\,b^2\,\{2\,(S_5-3\,S_3+S)\cos\overline{3\,\theta+k}+(3\,S_5-2\,S_3)\cos\overline{\theta+k}+(2\,S_3+3\,S)\cos\overline{5\,\theta+k}\}$$

$$+4\,a\,b\,c\,\{S_4\sin\overline{2\,\theta+k}-2\,S_2\sin\overline{4\,\theta+k}\}+b\,c^2\,S\cos\theta+3\,a\,c^2\,S_3\cos\overline{3\,\theta+k}=0$$

53. If in this expression we write $\theta=0$, we obtain

$$-\frac{g}{a}\,b\,D-\frac{a}{g}\,a\,D_3\cos k-(S_3-S)\,b^3+4\,a\,b\,c\sin k\,(S_4-2\,S_2)$$

$$-a\,b^2\cos k\,\{5\,S_5-6\,S_3+5\,S\}+b\,c^2\,S+3\,a\,c^2\,S_3\cos k=0.$$

If we write $\theta=\pi$, and dash the sums and differences

$$-\frac{g}{a}\,b\,D'-\frac{g}{a}\,a\,D_3'\cos k-(S_3'-S')\,b^3-4\,a\,b\,c\,\{S_4'-2\,S_2'\}\sin k-\&c.=0$$

$$\therefore \quad 4\,a\,b\,c\,\{S_4-2\,S_2+S_4'-2\,S_2'\}\sin k$$

$$=\frac{g}{a}\,b\,(D-D')+\frac{g}{a}\,a\cos k\,(D_3-D_3')$$

$$+a\,b^2\cos k\,(5\,S_5-6\,S_3+5\,S_1-\overline{5\,S_5'-6\,S_3'+5\,S'})-b\,c^2\,\overline{S-S'}-3\,a\,c^2\cos k\,\overline{S_3-S_3'}$$

or

$$4\,a\,b\,c\,\{2\,S_4-4\,S_2\}\sin k$$

$$=\frac{g}{a}\,b\,.\,\frac{2}{3c}\,a\,S\,D_3\sin k+\&c.+\&c.-b\,c\,\frac{2\,a}{3\,c}\,D\,D_3\sin k-\&c.$$

if $\sin k$ is not equal to 0; then

$$8\,c^2\,(S_4-2\,S_2)=\frac{g}{a}\,.\,\frac{2}{3}\,S\,D_3-\frac{2}{3}\,c^2\,D\,D_3$$

$$8\,(S_4-2\,S_2)=\frac{2}{3}\,.\,\frac{S^2\,D_3}{D}-\frac{2}{3}\,D\,D_3$$

$$=\frac{8}{3}\,\frac{D_3}{D}$$

$$3\,(S_4-2\,S_2)\,D=D_3.$$

or, if we choose to retain all the terms to the 2d order

$$-\frac{g}{a}\,b\,D-\frac{g}{a}\,a\,D_3\cos k-(S_3-S)\,b^3+4\,a\,b\,c\sin k\,(S_4-2\,S_2)$$

$$-a\,b^2\cos k\,\{5\,S_5-6\,S_3+5\,S\}+b\,c^2\,S+3\,a\,c^2\,S_3\cos k=0.$$

If we write $\theta=\pi$ and accent the letters

$$\frac{g}{a}\,b\,D'+\frac{g}{a}\,a\,D_3'\cos k+(S_3'-S')\,b^3+4\,a\,b\,c\sin k\,(S_4'-2\,S_2')$$

$$+a\,b^2\cos k\,\{5\,S_5'-6\,S_3'+5\,S'\}-b\,c^2\,S'-3\,a\,c^2\,S_3'\cos k=0$$

$$\therefore \quad -\frac{g}{a}\,b\,(D-D')-\frac{g}{a}\,a\cos k\,(D_3-D_3')-b^3\,(S_3-S-S_3'+S')$$

$$-a\,b^2\cos k\,\{5\,S_5-6\,S_3+5\,S-5\,S_5'+6\,S_3'-5\,S'\}+b\,c^2\,(S-S')$$

$$+3\,a\,c^2\cos k\,(S_3-S_3')+4\,a\,b\,c\sin k\,(S_4-2\,S_2+S_4'-2\,S_2')=0.$$

Now $z=h+\dfrac{b}{c\,\alpha}\,(e^{\alpha z}-e^{-\alpha z})\sin\theta+\dfrac{a}{3\,c\,\alpha}\,(e^{3\,\alpha z}-e^{-3\,\alpha z})\sin(3\,\theta+k)$,

$$\therefore \quad D=e^{\alpha\left(h+\frac{a}{3\,c\,\alpha}\,D_3\sin k\right)}-e^{-\alpha\left(h+\frac{a}{3\,c\,\alpha}\,D_3\sin k\right)}$$

$=D_\circ+S_\circ\dfrac{a\,D_3\sin k}{3\,c}$, if $D_\circ$, $S_\circ$ are the values of D and S when we omit k,

$$\therefore \quad D-D'=\frac{2\,S\,a\,D_3}{3\,c}\sin k \text{ nearly,}$$

$$S-S'=\frac{2\,D\,a\,D_3}{3\,c}\sin k$$

$$S_5-S_5'=\frac{10\,D_5\,a\,D_3}{3\,c}\sin k$$

&c. &c.

$$\therefore \qquad \sin k \left\{ -\frac{g}{a} b \frac{2 \mathrm{S} a \mathrm{D}_3}{3c} - b^3 \overline{\frac{2 a \mathrm{D}_3^2}{c} - \frac{2 \mathrm{D} a \mathrm{D}_3}{3c}} \right.$$

$$\left. + \frac{2bc \mathrm{D} a \mathrm{D}_3}{3} + 8abc\, \overline{\mathrm{S}_4 - 2\,\mathrm{S}_2} \right\} = 0 \text{ nearly,}$$

whence, omitting small quantities, $3\,\mathrm{S}_4 - 6\,\mathrm{S}_2 = \mathrm{D}_3$.

In order that this may hold, it is necessary that λ be much longer than $2\pi h$, a circumstance which appears not likely ever to be satisfied. We conclude therefore that $k=0$ for all waves in which the pressure is a complete differential of x and y, and the motion oscillatory.

Writing $k=0$ the expression becomes

$$-\frac{g}{a} b \mathrm{D} \cos\theta - \frac{g}{a} a \mathrm{D}_3 \cos 3\theta - 4 b^2 c \sin 2\theta + \{2\,\overline{\mathrm{S} - \mathrm{S}_3} \cos\theta - \mathrm{S} \cos 3\theta\}\, b^2$$

$$+ 4abc\,\mathrm{S}_4 \sin 2\theta - 8abc\,\mathrm{S}_2 \sin 4\theta$$

$$-ab^2\{2\,\overline{\mathrm{S}_5 - 3\,\mathrm{S}_3 + \mathrm{S}} \cos 3\theta + 3\,\overline{\mathrm{S}_5 - 2\,\mathrm{S}_3} \cos\theta + 2\,\overline{\mathrm{S}_3 + 3\,\mathrm{S}} \cos 5\theta\}$$

$$+4abc\,\mathrm{S}_4 \sin 2\theta - 8abc\,\mathrm{S}_2 \sin 4\theta + bc^2\,\mathrm{S} \cos\theta + 3ac^2\,\mathrm{S}_3 \cos 3\theta = 0.$$

We have already obtained this equation in a more general form, in the former Part.

SECTION V.

54. We turn our attention next to the case of waves proceeding along a canal of given or of infinite width, but of variable depth, *in the direction of motion.* We will commence with the case in which the depth gradually diminishes, so that the bottom of the fluid is an inclined plane. It will be more simple in this case to assume the plane which passes through the centre of the wave to be the plane of xz. If, then, we write the values of u and v as originally obtained, they will be

$$u = b\left(e^{\alpha\,\overline{y+h-ax}} + e^{-\alpha\,\overline{y+h-ax}}\right) \sin a(x-ct)$$

$$v = -b\left(e^{\alpha\,\overline{y+h-ax}} - e^{-\alpha\,\overline{y+h-ax}}\right) \cos a(x-ct),$$

where $h-ax$ is the height of the plane of xz above the bottom of the fluid, or is the statical depth at the point in question.

Let us conceive, then, that the above equations represent the velocities in the case in question at the surface of the fluid when z is written for y; z being now very small. As long as the waves retain their *form*, this must be true very nearly at least.

It must be observed that the same thing is not true to all depths, for the very obvious reason, that at the bottom of the fluid the motion is no longer perfectly *horizontal.* There will, therefore, be a perpetual impulse tending slowly to alter the form of the wave. But, whatever be the form, it is possible to expand it in a series of the following nature,

$$z = \left(e^{\alpha\,\overline{z+h-ax}} - k e^{-\alpha\,\overline{z+h-ax}}\right) \sin\theta + \ldots.$$

But $\frac{dz}{dt}$ = the vertical velocity at the surface, which is a particular value of the quantity v, and as v is very nearly equal to 0 when $y+h-ax=0$, it follows that k is very small, and for our present purpose may be neglected.

We may state the results at which we have arrived, by saying that u, and v do not represent the velocities parallel to x and y in any case, but approach nearer and nearer to those velocities, as the points to which they correspond recede further from the bottom of the fluid. The quantity θ is always supposed to be less than 2π, a supposition which is required by the circumstance, that we are about to remove it from beneath the circular sign.

We suppose, then, that the above equations give the values of u and v at the surface of the fluid, and shall apply the method of parameters to deduce from them the variations of λ, &c. The quantities λ, b, &c. are now functions of x.

Since $\frac{du}{dx}+\frac{dv}{dy}=0$, we get, calling $a\,(z+h-ax)$ ϕ;

$$(e^{\phi}+e^{-\phi})\frac{\sin\theta}{b}\,\frac{db}{dx}+(e^{\phi}+e^{-\phi})\left(\overline{x-ct}\frac{da}{dx}-a\,t\frac{dc}{dx}\right)\cos\theta$$

$$+(e^{\phi}-e^{-\phi})\left(\overline{z+h-ax}\,\frac{da}{dx}-a\,\alpha\right)\sin\theta=0 \quad . \quad . \quad . \quad . \quad (1)$$

And since $\frac{du}{dy}=\frac{dv}{dx}$;

$$(e^{\phi}-e^{-\phi})\frac{\cos\theta}{b}\,\frac{db}{dx}-(e^{\phi}-e^{-\phi})\left(\overline{x-c\,t}\frac{da}{dx}-a\,t\frac{dc}{dx}\right)\sin\theta$$

$$+(e^{\phi}+e^{-\phi})\cos\theta\left(\overline{z+h-ax}\,\frac{da}{dx}-a\,\alpha\right)=0 \quad . \quad . \quad . \quad . \quad (2)$$

By eliminating $b,\overline{x-ct}\frac{da}{dx}-a\,t\frac{dc}{dx}$ and $\frac{d}{dx}(z+h-ax)\,a$, successively, we obtain

$$(e^{2\phi}-e^{-2\phi})\left(\overline{x-c\,t}\frac{da}{dx}-a\,t\frac{dc}{dx}\right)-4\frac{d}{dx}\,.\,a\,(z+h-ax)\sin\theta\cos\theta=0 \quad (3)$$

$$(e^{2\phi}-e^{-2\phi})\frac{db}{b\,dx}+(e^{2\phi}+e^{-2\phi}+2\cos 2\theta)\frac{d}{dx}\left(\overline{z+h-ax}\,.\,a\right)=0 \quad . \quad (4)$$

$$4\sin\theta\cos\theta\,\frac{db}{b\,dx}+(e^{2\phi}+e^{-2\phi}+2\cos 2\theta)\left(\overline{x-ct}\frac{da}{dx}-a\,t\frac{dc}{dx}\right)=0 \quad . \quad . \quad (5)$$

Now, our object is not to obtain the variation of any of the quantities through a small space, but through a large one. The variation of each of the quantities will consist of two parts, the one depending on x, the other on a circular function of x. With the former only we are concerned; the latter goes through all its values, and is as much additive as subtractive in the space occupied by one wave. Confining our attention to the former variations only, we obtain, as the non-periodic part of equation (3),

$$(x-ct)\frac{da}{dx}-at\frac{dc}{dx}=0 \quad . \quad . \quad . \quad (6).$$

and similarly of the others.

$$\frac{db}{dx}=0 \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

$$\frac{d(a\,\overline{z+h-ax})}{dx}=0 \quad . \quad . \quad . \quad . \quad . \quad (8)$$

Thus it appears that b is constant, or at least varies only *periodically*.

Also $a\,(z+h-ax)=f(z)$ by integrating 8.

$$\therefore \qquad a=\frac{f(z)}{z+h-ax}$$

$$\lambda=\frac{2\pi}{f(z)}(z+h-ax)$$

Let $\lambda_{\circ}$ be the value of λ when $x=0$

$$\therefore \qquad \lambda=\lambda_{\circ}\left(1-\frac{ax}{z+h}\right)$$

from which equation we learn that the length of the wave diminishes directly as the depth diminishes.

Cor. The length of the wave varies as the depth, in the case in which motion extends throughout the fluid.

55. Again, from equation (6) we obtain

$$\frac{dc}{dx}=\frac{(x-ct)\frac{da}{dx}}{at}$$

$$=\frac{x-ct}{t}\frac{d}{dx}\log a$$

$$=\frac{x-ct}{t}\frac{d}{dx}\log\frac{f(z)}{z+h-ax}$$

$$=\frac{x-ct}{t}\cdot\frac{a}{z+h-ax}$$

$$\therefore \qquad \frac{dc}{dx}+\frac{a}{z+h-ax}c=\frac{ax}{t(z+h-ax)}$$

or

$$\frac{c}{z+h-ax}=\frac{a}{t}\int\frac{x\,dx}{(z+h-ax)^2}+\phi(z)$$

$$=\frac{1}{at}\log\frac{z+h-ax}{z+h}+\frac{z+h}{at}\cdot\frac{1}{z+h-ax}+\mathrm{F}(z)$$

$$\therefore \qquad c=\frac{1}{at}(z+h-ax)\log\frac{z+h-ax}{z+h}+\frac{z+h}{at}+\overline{z+h-ax}\;\mathrm{F}(z)$$

$$c_{\circ}=\frac{z+h}{at}+(z+h)\;\mathrm{F}(z)$$

$$c=\frac{1}{at}(z+h-ax)\log\left(1-\frac{ax}{z+h}\right)+\frac{z+h}{at}+\left(c_{\circ}-\frac{z+h}{at}\right)\cdot\frac{z+h-ax}{z+h}$$

$$=\frac{z+h}{a\,t}\left(1-\frac{a\,x}{z+h}\right)\log\left(1-\frac{a\,x}{z+h}\right)+c_{\circ}\left(1-\frac{a\,x}{z+h}\right)+\frac{x}{t}.$$

$$c\,t=c_{\circ}t\left(1-\frac{a\,x}{z+h}\right)+x+\frac{1}{a}(z+h-a\,x)\log\left(1-\frac{a\,x}{z+h}\right)$$

$c_{\circ}$ being the value of c when $x=0$ or $a=0$; that is, the space described in a given time t in the actual case is less than would be described in the same time with an uniform velocity, on two accounts, 1*st*, because $c_{\circ}$ is changed to $c_{\circ}\left(1-\frac{a\,x}{z+h}\right)$, 2*d*, because by the continual change of phase of the wave during the motion from 0 to x, the motion will not begin from the same place.

We have, in fact, $$\lambda=\lambda_{\circ}\left(1-\frac{a\,x}{z+h}\right)$$

$$c\,t=c_{\circ}t\left(1-\frac{a\,x}{z+h}\right)+\frac{a\,x^2}{2\,(z+h)}\text{ nearly,}$$

$$=c_{\circ}t\left(1-\frac{a\,x}{z+h}\right)+\frac{a\,x\,c_{\circ}t}{2\,(z+h)}$$

$$=c_{\circ}t\left(1-\frac{1}{2}\,\frac{a\,x}{z+h}\right)=c_{\circ}t\sqrt{1-\frac{a\,x}{z+h}}\text{ nearly,}$$

that is, $$c=c_{\circ}\sqrt{1-\frac{a\,x}{z+h}}$$

$$=c_{\circ}\sqrt{\frac{\lambda}{\lambda_{\circ}}}\propto\sqrt{\lambda}.$$

From these formulæ we learn two things, viz. that the velocity of transmission at *any* point varies as the square root of the depth, or as the square root of the length of the wave.

56. We might proceed by another method to obtain the variation of c from the variation of λ: thus,

$$c^2=\frac{g}{a}\,\frac{\mathrm{D}}{\mathrm{S}}\text{ approximately;}$$

taking the logarithm of each side, and differentiating it with respect to x, the result is,

$$\frac{2}{c}\,\frac{d\,c}{d\,x}=\frac{d\,\mathrm{D}}{\mathrm{D}\,d\,x}-\frac{d\,\mathrm{S}}{\mathrm{S}\,d\,x}-\frac{d\,a}{a\,d\,x}.$$

But by virtue of equation (8); $\frac{d\,\mathrm{D}}{d\,x}=0,\ \frac{d\,\mathrm{S}}{d\,x}=0;$

$$\therefore\qquad 2\,\frac{d\,c}{c\,d\,x}=-\frac{d\,a}{a\,d\,x}$$

$$=\frac{d\,\lambda}{\lambda\,d\,x}$$

$$2\log c=\log\lambda+\mathrm{C}$$

$$2\log\frac{c}{c_{\circ}}=\log\frac{\lambda}{\lambda_{\circ}}$$

$$\text{or} \qquad c^2 = \frac{c_0^2}{\lambda_0} \cdot \lambda$$

the same result as we obtained before.

To find the time of describing a given *large* space, we have

$$\frac{dx}{dt} = c$$

$$t = \int \frac{dx}{c_0 \sqrt{1 - \frac{ax}{x+h}}}$$

If l be the whole length from the origin to the end $\frac{l}{x+h} = \frac{l-x}{x+h-ax}$

$$\therefore \qquad t = \int \frac{dx \sqrt{l}}{c_0 \sqrt{l-x}} = \frac{2}{c_0}\left(l - \sqrt{l(l-x)}\right)$$

Cor. If x be very small $t = \frac{x}{c_0}$, or the variation of depth introduces no variation in the space described.

If $x = l$; $t = \frac{2l}{c_0}$; or the time occupied by the wave in travelling to the end of the fluid, is exactly double what it would be if the depth were uniformly the same as where the motion is stated to commence.

57. We are desirous of testing these results by experiment, but find our materials rather scanty. The length of the wave, which is the most simply found from theoretical considerations, is the least easily observed. In lieu thereof we find the height of the wave given. The experiments to which I allude, those of Mr Russell, printed in the Seventh Report of the British Association, contain the variation of height for a number of waves, and the velocity of transmission for a number of others, all of which are what the author designates *primary waves;* that is, waves of translation. We think we are justified in assuming that, for such waves, the whole *wave* is transferred forwards. By means of this hypothesis, we are able to determine the height of the wave in terms of the length. Approximately, the following process will suffice.

$$\begin{aligned} \text{Volume} \qquad &= 2\int_0^\pi z\,dx = 2\int_0^\pi f \mathrm{D} \sin\theta\, dx \\ &= \frac{2f\mathrm{D}}{a}(1 - \cos\pi) \\ &= \frac{4f\mathrm{D}}{a} \\ &= \frac{2f\mathrm{D}\lambda}{\pi} \\ &= \frac{\epsilon\lambda}{\pi} \end{aligned}$$

where ϵ is the whole depth of the wave above its hollow.

Therefore $\epsilon\lambda$ remains constant during the motion,

and $$\frac{\epsilon}{\epsilon_\circ}=\frac{\lambda_\circ}{\lambda}=\frac{z+h}{z+h-ax}=\frac{\text{depth at first}}{\text{depth at } x},$$

that is, the elevation of the crest of the wave varies reciprocally, as the total depth of the fluid.

We shall not attempt to form a table exhibiting a comparison of this with observation; we are sure that a comparison cannot be hoped for at present, owing to the want of a sufficient number of observations, or perhaps to the want of some element in the tables referred to. So far as we can see, many waves differ widely in the results to which they give rise, whilst the elements of the waves themselves are identically the same. We may mention waves 112, 123, and 126 (Report, p. 494). The second and third commence similarly, and end similarly as to position, whilst the one continues unchanged in depth, and the other varies from .5 to .7, every thing else remaining the same. The discrepancy is more obvious in waves 113 and 131. In the second table given by Mr Russell (p. 494), the original height of the wave is wanting. We have restored it roughly, on the hypothesis that waves of the same depth will break at the same distance from the extremity of the canal. By calculating t' from the expression

$t'=\frac{2}{c_\circ}\left(l-\sqrt{l(l-x)}\right)$ there results the following table.

No.	No′.	Ht.	$l-x$	t	No″.	$c_\circ$	t'
133	131	1.5	9.3	2.	19	$\frac{40}{10.5}$	2.31
134, 135 } 144, 145	130	2.	10.				
136	107	.9	6.5	3.5	12	$\frac{40}{11}$	3.55
137, 140	112	.5	5.	4	6	$\frac{40}{11}$	4.28
138, 141, 142	126	.5	4.	5	6	$\frac{40}{11}$	4.81
139		.2?	3.	6	7	$\frac{40}{12}$	5.92
143		.1?	1.5	6.5	1	$\frac{40}{12}$	7.2
146		3. ?	16	.5	19	$\frac{40}{10}$	.25

In this table No. represents the number of the wave; No′. that of the wave in the preceding table, which breaks at the same point, and which is therefore presumed to have the same height; No″. that of a wave giving the value of $c_\circ$; t is the observed, t' the computed value of the time.

It will be seen that the last wave was observed to take a half second, whilst theory makes it only a quarter of a second in proceeding one foot, the slight variation of depth in one foot of length producing no appreciable effect.

SECTION VI.

58. We proceed to investigate the translation of waves, on the hypothesis that the section of the fluid, perpendicular to the direction of transmission, is not a rectangle. By reference to Art. 28, Part I., it will be seen that we have obtained an approximate solution on the hypothesis of parallel sections. The simplicity of the formula is such, that we are enabled to perceive at a glance its connexion with the hypothesis, and are thus led to suspect that the approximation is an approximation depending on the applicability of the hypothesis. In other words, we conceive that in proportion as this hypothesis approaches nearer to the truth, so does the formula also. Of the applicability of this hypothesis to the waves whose velocity we determined by it, we had great *a priori* confidence from the circumstance that the fluid was put in motion, in most cases, with an uniform velocity from top to bottom. Were this not the case, our hypothesis would certainly have been violated in the early part of the motion, and it is difficult to see how it could have been satisfied with any degree of accuracy at all. We propose, therefore, to give another solution of this problem on another hypothesis.

Let us now take account of the variation of motion in three dimensions.

Take x as the axis along which the horizontal motion of transmission takes place, and z vertical. Suppose also, that the origin is at the bottom of the fluid. We have as the equations for determining the pressure,

$$\frac{dp}{dx}=\varrho\left(-\left(\frac{du}{dt}\right)\right)$$

$$\frac{dp}{dy}=\varrho\left(-\left(\frac{dv}{dt}\right)\right)$$

$$\frac{dp}{dz}=\varrho\left(-g-\left(\frac{dw}{dt}\right)\right)$$

and on the hypothesis that p is a complete differential of x, y, and z, we are presented with the following equations, which contain the solution of the general problem,

$$\frac{du}{dx}+\frac{dv}{dy}+\frac{dw}{dz}=0 \quad . \quad . \quad . \quad . \quad (1)$$

$$\frac{d}{dy}\left(\frac{du}{dt}\right)=\frac{d}{dx}\left(\frac{dv}{dt}\right) \quad . \quad . \quad . \quad (2)$$

$$\frac{d}{dz}\left(\frac{du}{dt}\right)=\frac{d}{dz}\left(\frac{dw}{dt}\right) \quad . \quad . \quad . \quad (3).$$

From equation (2) we get

$$\frac{d}{dy}\left(\frac{du}{dt}+u\frac{du}{dx}+v\frac{du}{dy}+w\frac{du}{dz}\right)=\frac{d}{dx}\left(\frac{dv}{dt}+u\frac{dv}{dx}+v\frac{dv}{dy}+w\frac{dv}{dz}\right)$$

or

$$\frac{d^2u}{dtdy}+u\frac{d^2u}{dxdy}+\frac{du}{dx}\cdot\frac{du}{dy}+\frac{dv}{dy}\cdot\frac{du}{dy}+v\frac{d^2u}{dy^2}+\frac{dw}{dy}\frac{du}{dz}$$

$$+w\frac{d^2u}{dydz}=\frac{d^2v}{dxdt}+u\frac{d^2v}{dx^2}+\frac{du}{dx}\cdot\frac{dv}{dx}+\frac{dv}{dx}\cdot\frac{dv}{dy}+v\frac{d^2v}{dxdy}+\frac{dw}{dx}\cdot\frac{dv}{dz}+w\frac{d^2v}{dxdz}.$$

But

$$\left(\frac{d}{dt}\right)\cdot\frac{du}{dy}=\frac{d^2u}{dydt}+u\frac{d^2u}{dxdy}+v\frac{d^2u}{dy^2}+w\frac{d^2u}{dydz}$$

$$\left(\frac{d}{dt}\right)\cdot\frac{dv}{dx}=\frac{d^2v}{dxdt}+u\frac{d^2v}{dx^2}+v\frac{d^2v}{dxdy}+w\frac{d^2v}{dxdz}$$

$$\therefore \quad \left(\frac{d}{dt}\right)\cdot\frac{du}{dy}+\frac{du}{dx}\cdot\frac{du}{dy}+\frac{dv}{dy}\cdot\frac{du}{dy}+\frac{dw}{dy}\cdot\frac{du}{dz}$$

$$=\left(\frac{d}{dt}\right)\frac{dv}{dx}+\frac{du}{dx}\cdot\frac{dv}{dx}+\frac{dv}{dx}\cdot\frac{dv}{dy}+\frac{dw}{dx}\cdot\frac{dv}{dz}$$

or from equation (1),

$$\left(\frac{d}{dt}\right)\cdot\frac{du}{dy}-\frac{dw}{dz}\cdot\frac{du}{dy}+\frac{dw}{dy}\cdot\frac{du}{dz}$$

$$=\left(\frac{d}{dt}\right)\frac{dv}{dx}-\frac{dw}{dz}\cdot\frac{dv}{dx}+\frac{dw}{dx}\cdot\frac{dv}{dz}$$

or

$$\left(\frac{d}{dt}\right)\left(\frac{du}{dy}-\frac{dv}{dx}\right)=\frac{dw}{dz}\left(\frac{du}{dy}-\frac{dv}{dx}\right)+\frac{dw}{dx}\cdot\frac{dv}{dz}-\frac{dw}{dy}\cdot\frac{du}{dz}$$

$$\left(\frac{d}{dt}\right)\left(\frac{du}{dz}-\frac{dw}{dx}\right)=\frac{dv}{dy}\left(\frac{du}{dz}-\frac{dw}{dx}\right)+\frac{dv}{dx}\cdot\frac{dw}{dy}-\frac{dv}{dz}\cdot\frac{du}{dy}$$

$$\left(\frac{d}{dt}\right)\left(\frac{dv}{dz}-\frac{dw}{dy}\right)=\frac{du}{dx}\left(\frac{dv}{dz}-\frac{dw}{dy}\right)+\frac{du}{dy}\cdot\frac{dw}{dx}-\frac{du}{dz}\cdot\frac{dv}{dx}.$$

If

$$\frac{dv}{dz}-\frac{dw}{dy}=\mathrm{M}$$

$$\frac{dw}{dx}-\frac{du}{dz}=\mathrm{N}$$

$$\frac{du}{dy}-\frac{dv}{dx}=\mathrm{P} \text{ we get}$$

$$\left(\frac{d\mathrm{P}}{dt}\right)=\mathrm{P}\frac{dw}{dz}+\frac{dw}{dx}\cdot\frac{dv}{dz}-\frac{dw}{dy}\cdot\frac{du}{dz}$$

$$\left(\frac{d\mathrm{N}}{dt}\right)=\mathrm{N}\frac{dv}{dy}+\frac{dv}{dz}\cdot\frac{du}{dy}-\frac{dv}{dx}\cdot\frac{dw}{dy}$$

$$\left(\frac{d\,\mathrm{M}}{d\,t}\right)=\mathrm{M}\frac{d\,u}{d\,x}+\frac{d\,u}{d\,y}\,\frac{d\,w}{d\,z}-\frac{d\,u}{d\,z}\,\frac{d\,v}{d\,x}.$$

All these equations are satisfied by the hypothesis that $\frac{d\,u}{d\,y}-\frac{d\,v}{d\,x}=0$, $\frac{d\,u}{d\,z}-\frac{d\,w}{d\,x}=0$ and $\frac{d\,u}{d\,z}-\frac{d\,w}{d\,y}=0$; in which case $u\,d\,x+v\,d\,y+w\,d\,z$ is a complete differential of some function ϕ with respect to x, y, and z.

59. Let us proceed to apply the conditions just obtained to the case of wave motion. We shall have $u=f\,.\,\sin a\,(x-ct)$, $v=F\,.\,\cos a\,(x-ct)$, $w=\mathrm{F}\,.\,\cos a\,(x-ct)$ where f, F and F are functions of y and z.

Then since $\mathrm{P}=0$; $\frac{d\,f}{d\,y}+a\,F=0$ . . . . . (4)

since $\mathrm{N}=0$; $\frac{d\,f}{d\,z}+a\,\mathrm{F}=0$ . . . . . (5)

since $\mathrm{M}=0$; $\frac{d\,F}{d\,z}-\frac{d\,\mathrm{F}}{d\,y}=0$ . . . . . (6)

and since $\frac{d\,u}{d\,x}+\frac{d\,v}{d\,y}+\frac{d\,w}{d\,z}=0$; $a\,f+\frac{d\,F}{d\,y}+\frac{d\,\mathrm{F}}{d\,z}=0$ . . . . (7).

These form but three distinct equations; inasmuch as equation (6) is merely a consequence of (4) and (5).

By substituting in (7) the values of F and F deduced from (4) and (5), we get

$$a^2 f-\frac{d^2 f}{d\,y^2}-\frac{d^2 f}{d\,z^2}=0 \text{ or } \frac{d^2 f}{d\,y^2}+\frac{d^2 f}{d\,z^2}=a^2 f.$$

This equation admits of the following solution,

$$f=\Sigma\, e^{a\,(my+nz)}\,.\,b, \text{ where } m^2+n^2=1,$$

and Σ embraces all possible values of n and b.

Equations (4) and (5) give us also,

$$F=-\Sigma\, e^{a\,(my+nz)}\,.\,m\,b,\ \mathrm{F}=-\Sigma\, e^{a\,(my+nz)}\,.\,n\,b.$$

We must obtain the particular values of n and b, which satisfy our problem from the restrictions which the problem itself imposes on us.

Let us place the origin in such a point that when $z=0$, $w=0$, and when $y=0$, $v=0$, whatever be the value of x or t.

If, as is commonly the case, the canal be symmetrical with respect to a vertical plane running along its length, the origin will be situated at the bottom of this plane, and the line in which this plane cuts the surface of the fluid, will be the *middle* of the canal. Let the equation to a section of the surface of the canal made by a plane perpendicular to this line, or to the direction of motion, be $y=\psi\,(z)$;

then since w or $F=0$ when $z=0$; $\Sigma e^{\alpha m y} . n b=0$ . . . . . (8)

and since v or $F=0$ when $y=0$; $\Sigma e^{\alpha n z} . m b=0$ . . . . . (9).

Also at the side of the canal, it is evident that $\frac{v_{\prime}}{w_{\prime}}=\frac{d y}{d z}=\psi'(z)$.

Now $v_{\prime}$ which is the value of v at the side of the canal, may not be obtainable from the value of v given by the equations (4), (5), (6), (7), since it is possible that the discontinuity of the fluid may require a discontinuous function as the expression for its motion in the neighbourhood of the sudden transition from the fluid to the surface of the canal. Provided, however, the canal be not abrupt in its curvature, and the motion be not very great in comparison with the magnitude of the canal, it is clear that the values of $v_{\prime}$ and $w_{\prime}$ will approach very near to the values of v and w at the surface of the fluid. In fact, we may safely argue that the conditions of continuity are not more violated by putting v and w for $v_{\prime}$ and $w_{\prime}$ at the surface, than they are by putting v and w themselves for the velocities obtained from conditions belonging to the *interior* of the mass. With these observations we shall adopt the following equation:

$$\frac{v}{w}=\psi'(z) \text{ at the surface,}$$

or

$$\frac{F(z, \psi z)}{\mathrm{F}(z, \psi z)}=\psi'(z) \text{ at the surface,} \quad . \quad . \quad . \quad . \quad (10).$$

We have written $F(z, y)$ for F, &c.

Also, if we adopt only the large terms of equation 11, Art. 50, we shall have

$$w g+\frac{d^2 \phi}{d t^2}=0 \text{ at the surface,}$$

or

$$g \mathrm{F}+c^2 . f . a=0$$

$$\therefore \quad c^2=-\frac{g}{a} . \frac{\mathrm{F}}{f} \text{ at the surface} \quad . \quad . \quad . \quad . \quad (11).$$

Condition (9) is satisfied by giving to m two equal values with opposite signs for every value of b and n. But since $m^2+n^2=1$, the value of m is $\pm\sqrt{1-n^2}$, and consequently this condition merely directs that *both* values must be retained. We may now eliminate m, and write $f=\Sigma b\{e^{\alpha(n z+\sqrt{1-n^2} y)}+e^{\alpha(n z-\sqrt{1-n^2} y)}\}$

$$F=-\Sigma b\{\sqrt{1-n^2}\, e^{\alpha(n z+\sqrt{1-n^2} y)}-\sqrt{1-n^2}\, e^{\alpha(n z-\sqrt{1-n^2} y)}\}$$

$$\mathrm{F}=-\Sigma b n\{e^{\alpha(n z+\sqrt{1-n^2} y)}+e^{\alpha(n z-\sqrt{1-n^2} y)}\}.$$

Condition (8) requires that $\mathrm{F}=0$ when $z=0$. If $y=\eta$ when $z=0$, this gives

$$\Sigma b n\{e^{\alpha \eta \sqrt{1-n^2}}+e^{-\alpha \eta \sqrt{1-n^2}}\}=0.$$

To confine ourselves to the most simple case, let us suppose $\eta=0$; then we

have $\Sigma b n=0$ (12). This is satisfied by giving to n pairs of equal values positive and negative; and thus we get as the general solution of the equation, subject to our conditions

$$f=\Sigma b\{e^{\alpha(nz+\sqrt{1-n^2}y)}+e^{\alpha(nz-\sqrt{1-n^2}y)}+e^{\alpha(-nz+\sqrt{1-n^2}y)}+e^{-\alpha(nz+\sqrt{1-n^2}y)}\}$$

$$=\Sigma b\{e^{\alpha nz}+e^{-\alpha nz}\}\{e^{\alpha\sqrt{1-n^2}y}+e^{-\alpha\sqrt{1-n^2}y}\}$$

the Σ embracing all the *positive* values of n.

By equations (4) and (5) we obtain the values of F and F thus:

$$F=-\Sigma b\sqrt{1-n^2}\{e^{\alpha nz}+e^{-\alpha nz}\}\{e^{\alpha\sqrt{1-n^2}y}-e^{-\alpha\sqrt{1-n^2}y}\}$$

$$\mathrm{F}=-\Sigma b n\ \{e^{\alpha nz}-e^{-\alpha nz}\}\{e^{\alpha\sqrt{1-n^2}y}+e^{-\alpha\sqrt{1-n^2}y}\}$$

Thence equation (10) gives

$$\frac{\Sigma b\sqrt{1-n^2}\{e^{\alpha nz}+e^{-\alpha nz}\}\{e^{\alpha\sqrt{1-n^2}\psi z}-e^{-\alpha\sqrt{1-n^2}\psi z}\}}{\Sigma b n\quad\{e^{\alpha nz}-e^{-\alpha nz}\}\{e^{\alpha\sqrt{1-n^2}\psi z}+e^{-\alpha\sqrt{1-n^2}\psi z}\}}=\psi'(z) \qquad (13)$$

at the surface.

Also equation (11) gives

$$c^2=\frac{g}{\alpha}\,\frac{\Sigma b n\{e^{\alpha nz}-e^{-\alpha nz}\}\{e^{\alpha\sqrt{1-n^2}y}+e^{-\alpha\sqrt{1-n^2}y}\}}{\Sigma b\{e^{\alpha nz}+e^{-\alpha nz}\}\{e^{\alpha\sqrt{1-n^2}y}+e^{-\alpha\sqrt{1-n^2}y}\}} \quad .\quad .\quad .\quad . \quad (14)$$

at the surface.

Equations (13) and (14) contain the solution of the problem.

60. Application. The most simple type which a wave can have is that which is expressed by one function only. In this case Σ may be omitted, and the value of c^2 is independent of y, or is the same throughout the whole mass; all our processes are consequently applicable without any limitation to this case, and we may regard our solution as a complete one.

As an approximation, let us expand the exponentials contained in these equations: by this means we get

$$\frac{2b\sqrt{1-n^2}\,2\alpha\sqrt{1-n^2}\psi(z)+\&c.}{2bn\,2\alpha nz+\&c.}=\psi'(z)$$

or $$\frac{1-n^2}{n^2}\,\frac{\psi(z)}{z}=\psi'(z)\ \text{nearly,}$$

and $$c^2=\frac{g}{\alpha}\,\frac{2bn^2\alpha z}{2b}=gn^2z.$$

Hence $$n^2=\frac{\psi(z)}{\psi(z)+z\psi'(z)}$$

$$c^2=\frac{gz\psi(z)}{\psi(z)+z\psi'(z)} \quad .\quad .\quad . \quad (14)$$

Cor. If $y=a^n z^m$, which case includes the triangle, parabola, &c. we have

$$c^2 = \frac{g z^{\frac{m}{n}+1} a}{a z^{\frac{m}{n}} + \frac{m}{n} a z^{\frac{m}{n}}} = g z \frac{1}{1+\frac{m}{n}}$$

But area of vertical section $= \int_0^z dz\, a z^{\frac{m}{n}}$

$$= \frac{a z^{\frac{m}{n}+1}}{1+\frac{m}{n}}$$

$$\therefore \quad \frac{z}{1+\frac{m}{n}} = \frac{\text{area of vertical section}}{a z^{\frac{m}{n}}\ (=y)}$$

$$= \frac{\text{area of vertical section}}{\text{breadth at surface}}$$

and
$$c^2 = g \cdot \frac{\text{area of vertical section}}{\text{breadth at surface}}.$$

This is the result obtained in Art. 28, Part I. It may be doubted whether our present result is more approximate even in other cases than this.

61. One remarkable circumstance is pointed out by the form of the result, viz. that the velocity of transmission is independent of y, provided one value of n is sufficient to satisfy all the conditions. The ridge of the wave is, consequently, a line extending directly across the channel, and not curved so as to be more advanced in the middle or deepest part of the channel than at the sides. This circumstance I supposed to be altogether in opposition to fact, from the form which Mr Whewell gives the tide curves in his excellent essay on the Approximate Determination of the Cotidal Lines (Phil. Trans. 1832). I attributed this to the approximation which had been used, and in part to the fact, that in all probability the form of the wave is not the most simple possible, and cannot be satisfied by one value of n. On mentioning the circumstance to Mr Russell, he informed me that, in experimenting on perfect triangular channels, he had found that the wave is *not* retarded, and stated that this fact astonished him very much on making the experiment. As far, therefore, as simple cases are concerned, it appears that theory and experiment are perfectly at one.

We may remark that the result of the approximation is the more to be depended on, inasmuch as the results obtained by applying the same process of approximation to the simple case of a perfectly uniform canal, produces results remarkably conformable with experiment, and such as have been long recognised as expressing the laws of the phenomena.

62. Equations (13) and (14) cannot be reduced without approximation, ex-

cept in one very simple case. If the canal have a triangular section such that the breadth is double the depth, or if one side of the triangle be vertical, and the breadth equals the depth $\psi(z)=z$ and $\psi'(z)=1$. In this case

$$\frac{\sqrt{1-n^2}\left(e^{anz}+e^{-anz}\right)\left(e^{a\sqrt{1-n^2}\,z}-e^{-a\sqrt{1-n^2}\,z}\right)}{n\left(e^{anz}-e^{-anz}\right)\left(e^{a\sqrt{1-n^2}\,z}+e^{-a\sqrt{1-n^2}\,z}\right)}=1$$

$$\therefore \qquad n=\sqrt{1-n^2}=\frac{1}{\sqrt{2}} \text{ and}$$

$$c^2=\frac{g}{a}\,\frac{n\left(e^{anz}-e^{-anz}\right)}{\left(e^{anz}+e^{-anz}\right)}=\frac{g}{a\sqrt{2}}\,\frac{e^{\frac{az}{\sqrt{2}}}-e^{-\frac{az}{\sqrt{2}}}}{e^{\frac{az}{\sqrt{2}}}+e^{-\frac{az}{\sqrt{2}}}}$$

If $\qquad a\sqrt{2}=a', \quad \frac{z}{2}=z'$; this gives

$c^2=\frac{g}{a'}\cdot\frac{e^{a'z'}-e^{-a'z'}}{e^{a'z'}+e^{-a'z'}}$, which is the expression for c^2 in a rectangular channel of which the depth is one-half that of the greatest depth in the triangular channel, Art. 10.

Hence we conclude, 1. That the velocity is that due to a rectangular channel of half the depth: 2. That the length of the wave $\left(\lambda=\frac{2\pi}{a}\right)$ is to that in a rectangular channel of half the depth as $\sqrt{2}:1$, or to that of a wave in a rectangular channel of the same depth as $1:\sqrt{2}$. The former of these results is amply confirmed by experiment: the latter has not, so far as I know, been examined experimentally.

63. Let us, in the next place, determine the relations which exist amongst the functions for that particular case in which the functions v and w are *continuous*. We cannot suppose that this hypothesis is very greatly erroneous in a channel of triangular section, but we hesitate to assert that it is *strictly* true even in that case. Here we may put h for the statical depth of the surface $h+\xi$ for the depth at the time t, and expand the functions in terms of ξ.

Let us denote $e^{anh}+e^{-anh}$ by S_n, $e^{anh}-e^{-anh}$ by D_n, $e^{a\sqrt{1-n^2}\,\psi h}+e^{-a\sqrt{1-n^2}\,\psi h}$ by S_m, $e^{a\sqrt{1-n^2}\,\psi h}-e^{-a\sqrt{1-n^2}\,\psi h}$ by D_m; then substituting these values in equation (13) we obtain

$$\frac{\Sigma b\{S_n+D_n a n\xi+\ldots\}\{D_m+S_m a\sqrt{1-n^2}\,(\psi' h\xi+\ldots)+\ldots\}\sqrt{1-n^2}}{\Sigma b\{D_n+S_n a n\xi+\ldots\}\{S_m+D_m a\sqrt{1-n^2}\,(\psi' h\xi+\ldots)+\ldots\}n}$$
$$=\psi' h+\psi'' h\,.\,\xi+\ldots.$$

and by (14)

$$c^2=\frac{g}{a}\cdot\frac{\Sigma b\{D_n+S_n a n\xi+\ldots\}\left(e^{a\sqrt{1-n^2}\,y}+e^{-a\sqrt{1-n^2}\,y}\right)n}{\Sigma b\{S_n+D_n a n\xi+\ldots\}\left(e^{a\sqrt{1-n^2}\,y}+e^{-a\sqrt{1-n^2}\,y}\right)}.$$

This last equation will give no results except those which arise from equating to each other parts independent of ξ, inasmuch as we obtain the approxima-

tion from the general equation, by omitting all terms which involve the product of two small quantities.

No such restriction applies to the former of the equations, and consequently we may deduce conditions from that equation which shall give relations between the different values of b. We do not purpose to pursue this investigation. Very little consideration will shew that the first value of b will be much greater than the second, and that, provided we omit powers of ξ higher than the first, we may neglect Σ even in the more general case. We obtain, by this process,

$$(S_n + D_n a n \xi)(D_m + S_m a\sqrt{1-n^2}\,\psi' h \xi)\sqrt{1-n^2} =$$
$$(D_n + S_n a n \xi)(S_m + D_m a\sqrt{1-n^2}\,\psi' h \xi)\,n(\psi' h + \psi'' h \xi)$$

or
$$S_n D_m\sqrt{1-n^2} = n D_n S_m \psi' h \quad . \quad . \quad . \quad . \quad . \quad (15)$$

and
$$S_n S_m(1-n^2)\psi' h + D_n D_m n\sqrt{1-n^2} = D_n D_m n\sqrt{1-n^2}(\psi' h)^2$$
$$+ S_n S_m n^2 \psi' h + D_n S_m n \psi'' h \quad . \quad . \quad . \quad (16)$$

If, for the sake of abbreviation, we write a for $\psi' h$, and a' for $\psi'' h$, and q for $\frac{a'}{a}$, we shall have

by means of equation (15) $\sqrt{1-n^2} = \frac{a D_n S_m n}{S_n D_m}$, which, being substituted in (16), reduces it to

$$\frac{S_n S_m \sqrt{1-n^2}\,a + D_n D_m n}{S_n D_m} \cdot D_n S_m = a D_n D_m \sqrt{1-n^2} + S_n S_m n + D_n S_m \frac{a'}{a}$$

or
$$S_n D_n S_m^2 \sqrt{1-n^2}\,a + S_m D_m D_n^2 n =$$
$$S_n D_n D_m \sqrt{1-n^2}\,a + S_m D_m S_n^2 n + S_n D_n S_m D_m q,$$

which is equivalent to

$$S_n D_n \sqrt{1-n^2} \cdot a = S_m D_m n + \frac{S_n D_n S_m D_m q}{4}$$

since $\quad S_m^2 - D_m^2 = S_n^2 - D_n^2 = 4.$

By means of this last equation and (15) we finally obtain

$$S_n^2(1-n^2) = S_m^2 \,.\, n^2 + \frac{S_m^2 S_n D_n n q}{4}$$

$$D_m^2 = a^2 D_n^2 - \frac{D_m^2 S_n D_n q}{4n}.$$

The first of these equations gives

$$S_n^2(1-n^2) = n^2 D_m^2 + 4n^2 + (D_m^2 + 4)\frac{S_n D_n n q}{4}$$

and the second
$$D_m^2 = \frac{a^2 D_n^2}{1 + \frac{S_n D_n q}{4n}}$$

$$\therefore \quad S_n(1-n^2) - 4n^2 - S_n D_n n q = \left(n^2 + \frac{S_n D_n n q}{4}\right)\frac{a^2 D_n^2}{1 + \frac{S_n D_n q}{4n}}$$
$$= n^2 a^2 D_n^2$$

or $$S_n^2(1-n^2)-n^2(S_n-D_n^2)-S_n^2 D_n n q=n^2 a^2 D_n^2$$

$$(1-2n^2)\left(\frac{S_n}{D_n}\right)^2-nq\frac{S_n}{D_n}=n^2(a^2-1)$$

$$\frac{S_n}{D_n}=\frac{nq}{2(1-2n^2)}\pm\sqrt{\left\{\frac{4n^2(a^2-1)(1-2n^2)+n^2q^2}{4(1-2n^2)^2}\right\}} \quad (17)$$

Again, by the same two equations,

$$S_m=\frac{\sqrt{1-n^2}}{n}\quad\frac{S_n}{\sqrt{1+\frac{S_n D_n q}{4n}}}$$

$$D_m=\quad a\ .\ \frac{D_n}{\sqrt{1+\frac{S_n D_n q}{4n}}}$$

$$\therefore\quad 2e^{\alpha\sqrt{1-n^2}\psi h}=\frac{1}{\sqrt{1+\frac{S_n D_n q}{4n}}}\left\{\frac{\sqrt{1-n^2}}{n}S_n+aD_n\right\}$$

$$2e^{-\alpha\sqrt{1-n^2}\psi h}=\frac{1}{\sqrt{1+\frac{S_n D_n q}{4n}}}\left\{\frac{\sqrt{1-n^2}}{n}S_n-aD_n\right\}$$

$$\therefore\quad 4=\frac{1}{1+\frac{S_n D_n q}{4n}}\left\{\frac{1-n^2}{n^2}S_n^2-a^2D_n^2\right\}$$

$$=\frac{1}{1+\frac{S_n D_n q}{4n}}\left(\frac{1-n^2}{n^2}D_n^2+4\frac{1-n^2}{n^2}-a^2D_n\right)$$

$$\therefore\quad D_n^2=\quad 4\quad\frac{1+\frac{S_n D_n q}{4n}-\frac{1-n^2}{n^2}}{\frac{1-n^2}{n^2}-a^2}$$

$$D_n=\quad 2\sqrt{\frac{1-2n^2-\frac{S_n D_n q n}{4}}{n^2(a^2+1)-1}} \quad (18)$$

From the same analysis we obtain

$$4\left(1+\frac{S_n D_n q}{4n}\right)=\frac{1-n^2}{n^2}S_n^2-a^2S_n^2+4a^2$$

$$S_n^2=\quad 4\quad\frac{1+\frac{S_n D_n q}{4n}-a^2}{\frac{1-n^2}{n^2}-a^2}$$

$$S_n=\quad 2\sqrt{\frac{n^2(a^2-1)-\frac{S_n D_n q n}{4}}{n^2(a^2+1)-1}} \quad (19)$$

The equations (17), (18), and (19), give the limits to the value of n.

COR. For the case in which the section is triangular $q=0$. In this case equation (17) shews that a^2-1 and $1-2n^2$ must have the same sign: and equation (18) that $1-2n^2$ and $n^2(a^2+1)-1$ must have the same sign.

Therefore the three quantities a^2-1, $1-2n^2$, and $n^2(a^2+1)-1$ are positive, zero, and negative together.

Now, the result obtained by an approximation was, that $1-2n^2=0$. This result, then, corresponds to that triangular section for which $a=1$. In other cases we find that, *on the hypothesis* of continuity which we now adopt,

$$\text{if} \quad a < 1 \quad n > \frac{1}{2} < \frac{1}{a^2+1};$$

$$\text{if} \quad a > 1 \quad n < \frac{1}{2} > \frac{1}{a^2+1}.$$

In the case for which we are furnished with experiments, viz. that given by Mr RUSSELL, the value of a was $\frac{3}{2}$ (Report of British Association for 1837, p. 442). For this case, then, we ought to have $n < \frac{1}{2} > \dfrac{1}{\frac{9}{4}+1} > \frac{4}{13}$.

Mr RUSSELL himself is of opinion that $n=\frac{1}{3}$. I do not think that his experiments warrant this opinion, and whilst I am not disinclined to admit that a quantity a little less than $\frac{1}{2}$ may suffice, I am still more confident in the truth of results obtained from approximation, than in those obtained from the hypothesis of continuity.

64. With respect to the height of the wave, we determine its value from the equation

$$F=-\frac{1}{a}\frac{df}{dz}=-\Sigma b\left(e^{anz}-e^{-anz}\right)\left(e^{a\sqrt{1-n^2}\,y}+e^{-a\sqrt{1-n^2}\,y}\right)n,$$

but $$w=\frac{dz}{dt}$$

$$\therefore \quad z=h+\Sigma\frac{bn}{ca}\left(e^{anz}-e^{-anz}\right)\left(e^{a\sqrt{1-n^2}\,y}+e^{-a\sqrt{1-n^2}\,y}\right)\sin\theta.$$

Hence the elevation of the wave is

$$\frac{2}{ca}\Sigma b\left(e^{anz}-e^{-anz}\right)\left(e^{a\sqrt{1-n^2}\,y}+e^{-a\sqrt{1-n^2}\,y}\right)n.$$

COR. 1. If the form of the wave can be expressed by one function,

$$\text{height}=\frac{2nb}{ca}\left(e^{anz}-e^{-anz}\right)\left(e^{a\sqrt{1-n^2}\,y}+e^{-a\sqrt{1-n^2}\,y}\right).$$

This expression shews that the crest of the wave is higher at the sides of the canal than in the middle.

Cor. 2. If $n^2=\frac{1}{2}$ and ϵ be the elevation in the middle of the canal, ϵ' at any point

$$\frac{\epsilon'}{\epsilon}=\frac{\epsilon^{\frac{\alpha y}{\sqrt{2}}}+e^{\frac{-\alpha y}{\sqrt{2}}}}{2}$$

Cor. 3. If y be great

$$\epsilon' : \epsilon :: e^{\frac{\alpha y}{\sqrt{2}}} : 2 \text{ nearly,}$$

that is the elevations increase in geometrical progression.

65. We have, in the next place, to give the general solution of this problem, which, lest we trespass too long, we shall do with as much brevity as is consistent with intelligibility.

In the first place, if ϕ be that function of which the partial differential coefficients with respect to the three co-ordinates, represent the velocities in their direction, we have

$$\phi=\Sigma\int_o^\infty\int_o^\infty \cos p\,x\,e^{my}\,.\,e^{nz}f(m,n,t)\,dm\,dn$$

subject to the condition $m^2+n^2-p^2=0$

$$\therefore \quad v=\Sigma\int_o^\infty\int_o^\infty \cos p\,x\,e^{my}e^{nz}mf(m,n,t)\,dm\,dn$$

$$w=\Sigma\int_o^\infty\int_o^\infty \cos p\,x\,e^{my}e^{nz}nf(m,n,t)\,dm\,dn$$

And the conditions (8) and (9) become

$$\Sigma\int_o^\infty\int_o^\infty \cos p\,x\,e^{my}mf(m,n,t)\,dm\,dn=0 \quad . \quad . \quad . \quad (8')$$

$$\Sigma\int_o^\infty\int_o^\infty \cos p\,x\,e^{nz}mf(m,n,t)\,dm\,dn=0 \quad . \quad . \quad . \quad (9')$$

Also equation (10) gives at the surface,

$$\frac{\Sigma\int_o^\infty\int_o^\infty m\cos p\,x\,e^{m\psi z}e^{nz}f(m,n,t)\,dm\,dn}{\Sigma\int_o^\infty\int_o^\infty n\cos p\,x\,e^{m\psi z}e^{nz}f(m,n,t)\,dm\,dn}=\psi'(z) \quad . \quad . \quad (10')$$

The equations (8′) and (9′) will be satisfied by supposing that n and m admit each of equal values with opposite signs. This therefore gives us the following values of u, v, w.

$$u=\Sigma\int_o^\infty\int_o^\infty p\sin p\,x\,(e^{my}+e^{-my})(e^{nz}+e^{-nz})f(m,n,t)\,dm\,dn$$

$$v=\Sigma\int_o^\infty\int_o^\infty m\cos p\,x\,(e^{my}-e^{-my})(e^{nz}+e^{-nz})f(m,n,t)\,dm\,dn$$

$$w=\Sigma\int_0^\infty\int_0^\infty n\cos px\,(e^{my}+e^{-my})\,(e^{nz}-e^{-nz})f(m,n,t)\,dm\,dn$$

The equation $w_{,}g+\frac{d^2\phi'}{dt^2}=0$ gives for the surface,

$$\Sigma\int_0^\infty\int_0^\infty\{n\,(e^{nz}-e^{-nz})gf(m,n,t)+\frac{d^2f(m,n,t)}{dt^2}(e^{nz}+e^{-nz})\}\times\cos px\,(e^{my}+e^{-my})\,dm\,dn=0$$

which is satisfied by making

$d^2f(m,n,t)+gn\dfrac{e^{nz_{,}}-e^{-nz_{,}}}{e^{nz_{,}}+e^{-nz_{,}}}=0$; $z_{,}$ being the value of z for the surface. The value of $f(m,n,t)$ deduced from this equation is

$f(m,n,t)=\mathrm{A}\cos\sqrt{\mathrm{N}}\,.\,t+\mathrm{B}\sin\sqrt{\mathrm{N}}\,t$; where

$$\mathrm{N}=gn\frac{e^{nz_{,}}-e^{-nz_{,}}}{e^{nz_{,}}+e^{-nz_{,}}}.$$

Now equation (10′) is reduced by multiplying up the denominator, and bringing both terms to the same side. It becomes by this process

$$\Sigma\int_0^\infty\int_0^\infty\cos px\,\{m\,(e^{m\psi z_{,}}-e^{-m\psi z_{,}})\,(e^{nz_{,}}+e^{-nz_{,}})$$

$$-n\psi'(z_{,})\,(e^{m\psi z_{,}}+e^{-m\psi z_{,}})\,(e^{nz_{,}}-e^{-nz_{,}})\}f(m,n,t)\,dm\,dn=0.$$

This equation is satisfied, if we can assign such relations between m and n that

$$m\,(e^{m\psi z_{,}}-e^{-m\psi z_{,}})\,(e^{nz_{,}}+e^{-nz_{,}})-n\psi'z_{,}\,(e^{m\psi z_{,}}+e^{-m\psi z_{,}})\,(e^{nz_{,}}-e^{-nz_{,}})=0,$$

which we evidently can do, since one term is positive, and the other negative.

Also, from the value of $f(m,n,t)$ it is evident that

$$c^2=\frac{\mathrm{N}}{p^2}=\frac{gn}{m^2+n^2}\cdot\frac{e^{nz_{,}}-e^{-nz_{,}}}{e^{nz_{,}}+e^{-nz_{,}}}.$$

We can approximate to the value of this expression just in the same way as in the previous process; thus the first equation gives $m^2\psi z_{,}-n^2z_{,}\psi'z_{,}=0$;

and the second $\qquad c^2=\dfrac{gn}{m^2+n^2}nz_{,}.$

Combining them, $c^2=\dfrac{gz_{,}}{\dfrac{z_{,}\psi'z_{,}}{\psi z_{,}}+1}=\dfrac{gz_{,}\psi z_{,}}{\psi z_{,}+z_{,}\psi'z_{,}}$,

the same result as that given by our approximation in the previous solution.

It is necessary to remark that m and n may in this case admit of an infinity of different values: but since for them all the above equations hold, this circumstance has no effect whatever on the value of c. The most important consequence which results from this general process is, the evidence which it affords in favour of the truth of our previous conclusion relative to the form which the wave as-

sumes, viz. that it does not lag in the neighbourhood of the shallower part of the canal.

SECTION VII.

66. The problem of determining the motion due to a slight disturbance, such as an impact on the surface of the fluid, or the elevation or depression of a portion, so as to leave it to regain its original position by disturbing the rest of the fluid;—this problem has occupied the attention of philosophers much, and it would appear that little remains to be done on the subject. We shall, in what follows, adopt the process employed by M. CAUCHY and M. POISSON, viz. that of solving the general equation which results from the hypothesis, that the pressure is a complete differential of the co-ordinates. We shall also adopt their solution in its utmost generality. In so doing we must, however, express a doubt whether it is a *complete* solution of the problem. That modified form of it which M. CAUCHY has adopted as the ground-work of his Memoir, we have no hesitation in pronouncing far from complete. But to what state of motion the integral applies, if not complete, we can hardly venture to guess. It is probably to a rippling motion or slight, almost vertical, oscillation of the surface, which is very inconsiderable compared with the depth. We make this remark from an examination of the results which M. POISSON has arrived at. Yet though the equation be imperfect, it will undoubtedly serve as an approximate representation of the *form* of the function on which the motion depends. A discussion of it, therefore, will probably lead us to some important conclusions relative to the arrangement of the particles at the beginning of the motion, though it fail to give a satisfactory value to the length of the wave.

We adopt the usual notation, and suppose, as is commonly done, the disturbance to be small.

The equations of motion are,

$$\frac{d^2\phi}{dx^2}+\frac{d^2\phi}{dy^2}=0 \quad . \quad . \quad . \quad . \quad (1)$$

$$\frac{d\phi}{dx}=u,\ \frac{d\phi}{dy}=v$$

$$\frac{du}{dy}=\frac{dv}{dx} \quad . \quad . \quad . \quad . \quad . \quad (2)$$

$$gv_{\prime}+\frac{d^2\phi_{\prime}}{dt^2}=0 \quad . \quad . \quad . \quad . \quad (3)$$

where $v_{\prime}$, $\frac{d^2\phi_{\prime}}{dt^2}$ correspond to the surface of the fluid.

The integral of equation (1), to which we alluded in the preceding paragraph, is

$$\phi=\Sigma\int_0^\infty \cos m x f(m,t)\,(e^{my}+e^{-m\overline{y+2h}})\,dm \quad . \quad . \quad . \quad (4)$$

the origin being placed at the quiescent surface of the fluid, so that z shall be a very small quantity.

The symbol Σ is such that it expresses the sum of the two functions $f(m,t)$, $f_{\prime}(m,t)$, the latter being multiplied by $\sin m x$; and it evidently requires that, when it *stands* before $\sin m x$ the quantity not expressed to which it applies, it is to have the negative sign.

Also equation (3) is

$$g\,\Sigma\int_0^\infty \cos m x f(m,t)\,m\,(e^{mz}-e^{-m\overline{z+2h}})\,dm$$

$$+\Sigma\int_0^\infty \cos m x\,\frac{d^2 f(m,t)}{dt^2}\,(e^{mz}+e^{-m\overline{z+2h}})\,dm=0.$$

We can satisfy this equation by making

$$\frac{d^2 f(m,t)}{dt^2}+g m\,\frac{e^{mz}-e^{-m\overline{z+2h}}}{e^{mz}+e^{-m\overline{z+2h}}}\,f(m,t)=0$$

$$\frac{d^2 f_{\prime}(m,t)}{dt^2}+g m\,\frac{e^{mz}-e^{-m\overline{z+2h}}}{e^{mz}+e^{-m(z+2h)}}\,f_{\prime}(m,t)=0\,.$$

Denoting $\qquad g m\,\dfrac{e^{mz}-e^{-m\overline{z+2h}}}{e^{mz}+e^{-m\overline{z+2h}}}$ by c_m^2, $\quad . \quad . \quad . \quad (5)$

The solution of these equations evidently is

$$f(m,t)=\phi(m)\cos c_m t+\psi(m)\sin c_m t$$

$$f_{\prime}(m,t)=\phi_{\prime}(m)\cos c_m t+\psi_{\prime}(m)\sin c_m t,$$

which being substituted in the value of ϕ reduces it to

$$\phi=\Sigma\int_0^\infty \cos m x\cos c_m t\,(e^{my}+e^{-m\overline{y+2h}})\,\phi(m)\,dm$$

$$+\Sigma\int_0^\infty \cos m x\sin c_m t\,(e^{my}+e^{-m\overline{y+2h}})\,\psi(m)\,dm.$$

Now one condition is, that when $x=k$, $u=0$ for all values of y, this gives

$$-\sin m k\,\phi(m)+\cos m k\,\phi_{\prime}(m)=0$$

$$-\sin m k\,\psi(m)+\cos m k\,\psi_{\prime}(m)=0$$

$$\therefore \quad \phi=\int_0^\infty \cos m x\cos c_m t\,(e^{my}+e^{-m\overline{y+2h}})\,\phi(m)\,dm$$

$$+\int_0^\infty \sin m x\cos c_m t\,(e^{my}+e^{-m\overline{y+2h}})\tan m k\,\phi(m)\,dm+\&c.$$

$$=\int_0^\infty \cos c_m t\,(e^{my}+e^{-m\overline{y+2h}})\,\frac{\phi(m)}{\cos m k}\cos m(x-k),$$

+ &c.

By altering $\frac{\phi(m)}{\cos m k}$ into $\phi'(m)$ to make the notation similar to that usually adopted, we get

$$\phi = \int_0^\infty \cos c_m t \, (e^{m y} + e^{-m\overline{y+2h}}) \, \phi'(m) \cos m(x-k) \, dm$$

$$+ \int_0^\infty \sin c_m t \, (e^{m y} + e^{-m\overline{y+2h}}) \, \psi'(m) \cos m(x-k) \, dm \quad . \quad . \quad (6)$$

This value of ϕ, it must be observed, has been deduced from the general form by the aid of equation (3) which belongs to the surface, and as that equation is only an approximation, so is this value of ϕ itself only approximate. The object of the following process is the determination of the functions ϕ' and ψ' by means of a knowledge of the initial state of the fluid.

67. Let us suppose that the motion has been produced by the sudden loosing of a disk which kept a small portion of the fluid at a higher level than the rest.

The integral of the equation for the pressure is $p = -g y - \frac{d\phi}{dt} - \&c. + C$,

therefore at the surface $$0 = -g z - \frac{d\phi_{,}}{dt} + C,$$

or $$\frac{d\phi_{,}}{dt} = -g z$$

the density being 1, and $u^2 + v^2$ very small.

Also $\phi_{,}$ is the impulse on the surface, therefore when $t=0$, $\phi_{,}=0$, $\frac{d\phi_{,}}{dt} = f(a)$,

$x=a$, $y=b$, when $b = -\frac{f(a)}{g}$ is the original form of the surface,

$$\therefore \quad f(a) = \int_0^\infty c'_m \, (e^{m b} + e^{-m\overline{b+2h}}) \, \psi'(m) \cos m(a-k) \, dm \quad . \quad . \quad . \quad (7)$$

and $$\phi'(m) = 0.$$

Thus the functions ϕ', $\phi'_{,}$, ψ', $\psi'_{,}$ are all determined. It remains that we *express* the function ψ' in terms of the given function f or its equivalent.

Denote $a-k$ by a', $f(a)$ by $f(a')$; $x-k$ by x';

then $$f(a') = \int_0^\infty c'_m \, (e^{m b} + e^{-m\overline{b+2h}}) \, \psi'(m) \cos m a' \, dm$$

$$\phi = \int_0^\infty \sin c_m t \, (e^{m y} + e^{-m\overline{y+2h}}) \, \psi'(m) \cos m x' \, dm \quad . \quad . \quad (8)$$

and our object is to obtain ϕ in terms of f without the intervention of ψ'; or to eliminate ψ' between these two equations.

68. We shall make use of the following theorem in order to effect this purpose.

THEOREM (See CAUCHY'S Memoir, Note xi.)

If $\Sigma \int_0^\infty \cos a' m \, \phi(m) \, dm = f(a')$, then will

$$\Sigma\int_0^\infty \cos a' m\,\gamma(m)\,\phi(m)\,dm$$

$$=\frac{1}{\pi}\int_0^\infty\int_0^\infty \gamma(\mu)\,f(\varpi)\,(\cos\mu\,\overline{\varpi-a'}+\cos\mu\,\overline{\varpi+a'})\,d\mu\,d\varpi$$

For $\int_0^\infty d\varpi\cos\mu\varpi\,(f\,\overline{a'+\varpi}+f\,\overline{a'-\varpi})=\int_{-\infty}^\infty d\varpi\cos\mu\varpi\,f(a'+\varpi)$

$$=\int_{-\infty}^\infty d\varpi\cos\mu(\varpi-a')\,f(\varpi)$$

$$\therefore\quad \Sigma\int_0^\infty \cos a' m\,\gamma(m)\,\phi(m)\,dm=\frac{1}{\pi}\int_{-\infty}^\infty\int_0^\infty \cos\mu(\varpi-a')\,\gamma(\mu)\,f(\varpi)\,d\varpi\,d\mu$$

69. In applying this theorem to the question before us, we take equations (8), which will coincide with our theorem if Σ be omitted, and

$$\phi(m)=c'_m\,(e^{mb}+e^{-mb+2h})\,\psi(m)$$

$$\gamma(m)=\frac{\sin c_m t\,(e^{my}+e^{-m\overline{y+2h}})}{c'_m\,(e^{mb}+e^{-m\overline{b+2h}})}$$

It should have been observed, that $c'^2_m=g\,m\,\dfrac{e^{mb}-e^{-m(b+2h)}}{e^{mb}+e^{-m\overline{b+2h}}}$.

Now in the case before us, $f(a')$ is a function which is constant $=-bg$, between the limits $a'=0$, $a'=a$, and zero for every other value of a'.

We obtain, therefore, $\int_{-\infty}^\infty f(\varpi)\cos\mu(\varpi-a')\,d\varpi$

$$=\int_0^a f(\varpi)\cos\mu(\varpi-a')\,d\varpi=-\frac{bg}{\mu}\{\sin\mu(\varpi-a')+\sin\mu a'\}$$

Hence $\phi=\frac{1}{\pi}\int_{-\infty}^\infty\int_0^\infty \cos\mu(\varpi-a')\,\gamma(\mu)\,f(\varpi)\,d\varpi\,d\mu$

$$=-\frac{bg}{\pi}\int_0^\infty \frac{\sin\mu(a-a')+\sin\mu a'}{\mu}\cdot\frac{\sin c_\mu t}{c'_\mu}\cdot\frac{e^{\mu y}+e^{-\mu(y+2h)}}{e^{\mu b}+e^{-\mu(b+2h)}}\cdot d\mu$$

$$=-\frac{b\sqrt{g}}{\pi}\int_0^\infty\frac{\sin\mu(a-a')+\sin\mu a'}{\mu\sqrt{\mu}}\cdot\frac{e^{\mu y}+e^{-\mu(y+2h)}}{\sqrt{e^{2\mu b}-e^{-2\mu(b+2h)}}}\cdot\sin c_\mu t\,d\mu\ :\ (9)$$

when we have integrated the expression, we must write x' instead of a'.

This is the complete value of ϕ corresponding to our hypothesis.

Our next process is to obtain the values of u and v, corresponding to a given canal.

70. CASE I. Let us suppose that y, b, and h, are small quantities. Neglecting their powers,

$$\phi=-\frac{b\sqrt{g}}{\pi}\int_0^\infty\frac{\sin\mu(a-a')+\sin\mu a'}{\mu\sqrt{\mu}}\cdot\frac{\sin c_\mu t}{\sqrt{\mu(b+h)}}\,d\mu$$

and $\quad c_\mu = g(z+h)\mu^2$ nearly.

Also $\quad u = \frac{d\phi}{dx} = \frac{b\sqrt{g}}{\pi}\int_0^\infty \frac{\cos\mu(a-a') - \cos\mu a'}{\sqrt{\mu}\ \sqrt{\mu(b+h)}} \sin c_\mu\, t\, d\mu$

$$= \frac{b}{2\pi}\sqrt{\frac{g}{b+h}}\int_0^\infty \Big\{ \sin\mu\left(a-a'+\sqrt{g(z+h)}\,t\right)$$

$$-\sin\mu\left(a-a'-\sqrt{g(z+h)}\,.\,t\right) - \sin\mu\left(a'+\sqrt{g(z+h)}\,t\right) + \sin\mu\left(a'-\sqrt{g(z+h)}\,t\right)\Big\}\frac{d\mu}{\mu}$$

Now the integral $\int_0^\infty \frac{\sin q\mu}{\mu} d\mu$ is $\frac{\pi}{2}$ whatever be q, provided it be positive.

Hence, of the four integrals which make up the value of u, it is necessary, in order that u may be equal zero, that all the quantities under the circular sign should not have the same sign.

When $t = 0$ and $x' > a$

$$u = \frac{b}{2\pi}\sqrt{\frac{g}{b+h}}\left\{-\frac{\pi}{2} + \frac{\pi}{2} - \frac{\pi}{2} + \frac{\pi}{2}\right\} = 0.$$

If $t < \frac{x'-a}{\sqrt{g(z+h)}}$, we obtain as the value

$$u = \frac{b}{2\pi}\sqrt{\frac{g}{b+h}}\left\{-\frac{\pi}{2} + \frac{\pi}{2} - \frac{\pi}{2} + \frac{\pi}{2}\right\} = 0.$$

If $t > \frac{x'-a}{\sqrt{g(z+h)}} < \frac{x'}{\sqrt{g(z+h)}}$,

$$u = \frac{b}{2\pi}\sqrt{\frac{g}{b+h}}\left\{\frac{\pi}{2} + \frac{\pi}{2} - \frac{\pi}{2} + \frac{\pi}{2}\right\}$$

$$= \frac{b}{2}\sqrt{\frac{g}{b+h}}.$$

If $t > \frac{x'}{\sqrt{g(z+h)}}$, $\quad u = \frac{b}{2\pi}\sqrt{\frac{g}{b+h}}\left\{\frac{\pi}{2} + \frac{\pi}{2} - \frac{\pi}{2} - \frac{\pi}{2}\right\} = 0.$

We must bear in mind that our results are mere approximations, and cannot be expected to give any thing more than the *nature* of the disturbance.

It appears, therefore, that the different particles of the fluid are not put into motion (at least horizontally) until such time, from the moment at which the raised fluid is set free, as would be occupied by a body moving with a velocity $=\sqrt{g(z+h)}$ in travelling from the nearest portion of the elevated fluid to the point in question. This is a very important conclusion, and differs altogether from the result obtained by Mr CAUCHY. It must be observed, that the demonstration supposes the fluid free in both directions, a supposition absolutely requisite to the form of the function which has been adopted.

Again, it is evident that not only is the velocity of transmission of the first

motion $=\sqrt{g(z+h)}$, but that every successive wave is transmitted with the same velocity.

Also the coefficient of the velocity $\frac{d\phi}{dx}$ varies inversely as $\sqrt{b+h}$, that is the $(\text{velocity})^2 \propto$ inversely as depth, therefore mass moved $\times$ velocity^2 or *vis viva* is constant.

Another, and, for our present purpose, a still more important result appears from the form of the function; viz. that the initial conditions which we have assumed to exist *must* of necessity give rise to a wave transmitted in the negative as well as in the positive direction. Thus the hypothesis belongs only to a canal open in both directions. And farther, since the other hypothesis, that when $t=0$, $\frac{d\phi}{dt}=0$, and $\phi=a$ *constant*, will give the sum of *sines* as the function corresponding to the sum of *cosines* in the present case, it is clear that no hypothesis of this nature can apply to the case of motion in a closed canal.

We must therefore look for some other process when we come to that case, and proceed at present with the problem before us, admitting it to be restricted to an open canal.

71. Let us now expand the different quantities which enter into the expression for ϕ.

We get $c_\mu{}^2 = g\mu \dfrac{1-e^{-2\mu(z+h)}}{1+e^{-2\mu(z+h)}}$

$$= g\mu \frac{1-1+2\mu(z+h)-2\mu^2(z+h)^2+\frac{4}{3}\mu^3(z+h)^3}{1+1-2\mu(z+h)+2\mu^2(z+h)^2}$$

$$= g\mu^2(z+h)\frac{1-(z+h)\mu+\frac{2}{3}(z+h)^2\mu^2}{1-\mu(z+h)+\mu^2(z+h)^2}$$

$$= g\mu^2(z+h)\{1-\mu(z+h)+\tfrac{2}{3}\mu^2(z+h)^2\}\{1+\mu(z+h)-\mu^2(z+h)^2+\mu^2(z+h)^2\}$$

$$= g\mu^2(z+h)\{1-(1-\tfrac{2}{3})\mu^2(z+h^2)\}$$

$$= g\mu^2(z+h)(1-\tfrac{1}{3}\mu^2(z+h)^2)$$

$$\therefore \quad c_\mu = \mu\sqrt{g(z+h)}\,(1-\frac{\mu^2}{6}(z+h)^2)$$

$$\sin c_\mu t = \sin \mu\sqrt{g(z+h)}\,t - \cos\mu\sqrt{g(z+h)}\,t \;.\; \frac{\mu^3}{6}(z+h)^2\sqrt{g(z+h)}$$

$$\frac{e^{\mu y}+e^{-\mu\overline{y+2h}}}{\sqrt{e^{2\mu b}-e^{-2\mu(b+2h)}}} = \frac{1+\mu y+\frac{\mu^2y^2}{2}+\frac{\mu^3y^3}{6}+1-\mu(y+2h)+\frac{\mu^2\overline{b+2h}^2}{2}-\&c.}{\sqrt{(1+2\mu b+\frac{4\mu^2b^2}{2}+\frac{8\mu^3b^3}{6}-1+2\mu(b+2h)-\frac{4\mu^2(b+2h)^2}{2}+\&c.)}}$$

$$=\frac{2-2\mu h+\mu^2(y^2+2hy+2h^2)}{\sqrt{\{4\mu(b+h)-8\mu^2 h(b+h)+\frac{4\mu^3}{3}(\overline{b^3+b+2h^3})\}}}$$

$$=\frac{1-\mu h+\mu^2(y^2+2hy+2h^2)}{\sqrt{\mu(b+h)}\sqrt{\left(1-2\mu h+\frac{\mu^2}{3}\frac{\overline{b^3+b+2h^3}}{b+h}\right)}}$$

$$=\frac{1}{\sqrt{\mu(b+h)}}\left\{1-\mu h+\mu^2\,\overline{y^2+2hy+2h^2}\right\}\left\{1+\mu h-\frac{\mu^2}{6}\frac{\overline{b^3+b+2h^3}}{b+h}+\frac{3}{2}\mu^2 h^2\right\}$$

$$=\frac{1}{\sqrt{\mu(b+h)}}\left\{1+\mu^2\,\overline{y^2+2hy+2h^2}-\mu^2 h^2-\frac{\mu^2}{6}\frac{\overline{b^3+b+2h^3}}{b+h}+\frac{3}{2}\mu^2 h^2\right\}$$

$$=\frac{1}{\sqrt{\mu(b+h)}}\left\{1+\mu^2(y^2+2hy+2h^2+\frac{1}{2}h^2-\frac{1}{3}\overline{b+h}^2-3h^2)\right\}$$

$$=\frac{1}{\sqrt{\mu(b+h)}}\left\{1+\mu^2(y^2+2hy+2h^2-\frac{1}{2}h^2-\frac{1}{3}\overline{b+h}^2)\right\}$$

$$=\frac{1}{\sqrt{\mu(b+h)}}\left\{1+\mu^2(y^2+2hy-\frac{1}{3}b^2-\frac{2}{3}bh+\frac{7}{6}h^2)\right\}.$$

The simple inspection of these formulæ will shew that they will afford no new terms in ϕ except those which are introduced by $\cos\mu\sqrt{g(z+h)}\,.\,t$.

But for $\frac{d\phi}{dy}$ instead of $e^{\mu y}+e^{-\mu\overline{y+2h}}$, we have

$$\mu(e^{\mu y}-e^{-\mu\overline{y+2h}})=2\mu^2(1-\mu h+\frac{\mu^2}{6}\overline{y^2+2yh+4h^2})(y+h),$$

therefore the coefficient which involves y and b is

$$\frac{\mu^2(y+h)}{\sqrt{\mu(b+h)}}\left\{1+\mu^2\left(\frac{1}{6}\overline{y^2+2hy+4h^2}-\frac{1}{2}h^2-\frac{1}{3}\overline{b+h}^2\right)\right\}$$

$$=\frac{\mu^2(y+h)}{\sqrt{\mu(b+h)}}\left\{1+\mu^2\frac{1}{6}(\overline{y+h}^2-2\overline{b+h}^2)\right\},$$

and $\frac{d\phi}{dy}$ is reduced to

$$\frac{d\phi}{dy}=-\frac{b\sqrt{g}}{\pi}\int_o^\infty\frac{\sin\mu\overline{a-a'}+\sin\mu a'}{\mu\sqrt{\mu}}\cdot\frac{\mu^2(y+h)}{\sqrt{\mu(b+h)}}d\mu\times$$

$$\left\{1-\frac{\mu^2}{6}(\overline{y+h}^2-2\overline{b+h}^2)\right\}\left\{\sin\rho\mu-\frac{\mu^3}{6}(z+h)^2\sqrt{g(z+h)}\cos\rho\mu\right\}$$

denoting $\sqrt{g(z+h)}\,.\,t$ by ρ.

$$=-\frac{b\sqrt{g}}{\pi}\int_o^\infty(\sin\mu\overline{a-a'}+\sin\mu a')\frac{y+h}{\sqrt{b+h}}d\mu\times$$

$$\left\{\sin\rho\mu-\frac{\mu^2}{6}\sin\rho\mu(\overline{y+h}^2-2\overline{b+h}^2)-\frac{\mu^3}{6}(z+h)^2\sqrt{g(z+h)}\cos\rho\mu\right\}$$

$$= -\frac{b\sqrt{g}\,(y+h)}{2\pi\sqrt{b+h}}\int_0^\infty \left\{\left[\cos\mu\,\overline{\varrho+a'-a} - \cos\,\overline{a-a+\varrho}\,\mu\right.\right.$$

$$\left. + \cos\mu\,\overline{a'-\varrho} - \cos\mu\,\overline{a'+\varrho}\right]\left[1-\frac{\mu^2}{6}(\overline{y+h}^2 - 2\,\overline{b+h}^2)\right]$$

$$-\frac{\mu^3}{6}(z+h)^2\sqrt{g\,(z+h)}\left[\sin\mu\,\overline{\varrho+a-a'} - \sin\mu\,\overline{\varrho-a+a'}\right.$$

$$\left.\left. + \sin\mu\,\overline{\varrho+a'} + \sin\mu\,\overline{a'-\varrho}\right]\right\} d\mu$$

= an integrated function

$$-\frac{b\sqrt{g}\,(y+h)}{2\pi\sqrt{b+h}}\int_0^{\frac{\infty}{z}} \cos\mu z\left\{1+\frac{1}{3x^2}\left((\overline{y+h}^2 - 2\,\overline{b+h}^2) \pm \frac{(z+h)^2\sqrt{g\,(z+h)}}{x^3}\right\} d\mu.\right.$$

If we confine our attention to waves transmitted in the positive direction, this gives

$$\text{function } -\frac{b\sqrt{g}\,(y+h)}{2\pi\sqrt{b+h}}\int_0^\infty d\mu \times$$

$$-\cos\overline{a-a'+\varrho}\,\mu\left\{1+\frac{1}{3\,(a-a'+\varrho)^2}(\overline{y+h}^2 - 2\,\overline{b+h}^2) - \frac{(z+h)^2\sqrt{g\,(z+h)}}{(a-a'+\varrho)^3}\right\}$$

$$+\cos(a+a'-\varrho)\mu\left\{1+\frac{1}{3\,(a+a'-\varrho)^2}(\overline{y+h}^2 - 2\,\overline{b+h}^2) + \frac{(z+h)^2\sqrt{g\,(z+h)}}{(a+a'-\varrho)^3}\right\}$$

or if ϱ be greater than $a+a'$, the factor is

$$\left\{-\cos\overline{a-a'+\varrho}\,\mu\left[1+\frac{1}{3\,(a-a'+\varrho)^2}(\overline{y+h}^2 - 2\,\overline{b+h})^2 - \frac{\overline{z+h}^2\sqrt{g\,(z+h)}}{(a-a'+\varrho)^3}\right.\right\}$$

$$+\cos\overline{\varrho-a-a'}\,\mu\left[1+\frac{1}{3\,(\varrho-a-a')^2}(\overline{y+h}^2 - 2\,\overline{b+h}^2) - \frac{\overline{z+h}^2\sqrt{g\,(z+h)}}{(\varrho-a-a')^3}\right.$$

$$\text{or}\quad v \propto \int_0^\infty \sin\left(\sqrt{g\,(z+h)}\;t - x'\right)\mu\,\sin a\,\mu\left\{1+\frac{1}{3\,(\varrho-a')^2}+\&\text{c.}\right\}$$

omitting a as a very small factor.

Hence it appears that the *recurring* function is independent of a, and consequently that a small disturbance in a shallow canal will be transmitted in precisely the same manner, whether the quantity originally disturbed be small or not.

We cannot extend this memoir to other cases of our present hypothesis, but must pass on to the more general problem.

72. Case II. Suppose the expansion to be carried on in terms of $e^{-\mu z}$, &c. This approximation is applicable to all conceivable cases of motion, and will consequently deserve our careful consideration.

From equation (9) we have

$$\frac{d\phi}{dy} = -\frac{b\sqrt{g}}{\pi}\int_0^\infty \frac{\sin\mu\,(a-a')+\sin\mu\,a'}{\sqrt{\mu}}\cdot\frac{e^{\mu y} - e^{-\mu(y+2h)}}{\sqrt{e^{2\mu b} - e^{-2\mu(b+2h)}}}\sin c_\mu\,t\,d\mu.$$

Now $c_\mu^2 = g\mu \dfrac{1-e^{-2\mu(z+h)}}{1+e^{-2\mu(z+h)}} = g\mu$ as a first approximation.

Hence, approximately

$$\frac{d\phi}{dy} = -\frac{b\sqrt{g}}{\pi}\int_0^\infty \frac{\sin\mu(a-a') + \sin\mu a'}{\sqrt{\mu}} \sin\sqrt{g\mu}\, t\, e^{-\mu(b-y)}\, d\mu.$$

We can obtain a very important result from this equation when t is a very small quantity. In that case,

$$\frac{d\phi}{dy} = -\frac{b\sqrt{g}\, t\sqrt{g}}{\pi}\int_0^\infty \left\{\sin\mu(a-a') + \sin\mu a'\right\} e^{-\mu(b-y)}\, d\mu$$

$$= -\frac{bgt}{\pi}\left\{\frac{a-a'}{(a-a')^2+(b-y)^2} + \frac{a'}{a'^2+(b-y)^2}\right\}$$

or by writing, as we ought, x' for a'

$$= \frac{bgt}{\pi}\left\{\frac{x'-a}{(x'-a)^2+(b-y)^2} - \frac{x'}{x'^2+(b-y)^2}\right\}.$$

Let us transfer the origin to the middle point of the base of the parallelopiped originally elevated, and put $x' = x + \dfrac{a}{2}$;

$$\therefore \quad \frac{d\phi}{dy} = \frac{bgt}{\pi}\left\{\frac{x-\frac{a}{2}}{\left(x-\frac{a}{2}\right)^2+(b-y)^2} - \frac{x+\frac{a}{2}}{\left(x+\frac{a}{2}\right)^2+(b-y)^2}\right\}.$$

1. If x be $< \dfrac{a}{2}$; $\dfrac{d\phi}{dy}$ is negative, or the particles move downwards, as, from the nature of the case, they must evidently do.

2. If x be very large, and $b-y$ small, the latter may be neglected, and

$$\frac{d\phi}{dy} = \frac{bgt}{\pi}\left(\frac{1}{x-\frac{a}{2}} - \frac{1}{x+\frac{a}{2}}\right) = \frac{bgta}{\pi\left(x^2-\frac{a^2}{4}\right)}.$$

Hence $\sin c_\mu t = \sin\sqrt{g\mu}\, t - \sqrt{g\mu}\, e^{-2\mu(z+h)} \cos\sqrt{g\mu}\, t$

and $$\frac{e^{\mu y} - e^{-\mu(y+2h)}}{\sqrt{(e^{2\mu b} - e^{-2\mu(b+2h)}}} = e^{-\mu(b-y)}(1 - e^{-2\mu\overline{y+h}} + \tfrac{1}{2}e^{-4\mu\overline{b+h}})$$

$$\therefore \quad \frac{d\phi}{dy} = -\frac{b\sqrt{g}}{\pi}\int_0^\infty \frac{\sin\mu(a-a') + \sin\mu a'}{\sqrt{\mu}} \cdot e^{-\mu(b-y)}\left\{(1-e^{-2\mu\overline{y+h}}\sin\sqrt{g}\,u\,t\right.$$
$$\left. - \sqrt{g\mu}\, e^{-2\mu\overline{z+h}}\cos\sqrt{g\mu}\, t\right\}.$$

3. $\dfrac{d\phi}{dy}$ will equal zero if

$$\frac{x-\frac{a}{2}}{\left(x-\frac{a}{2}\right)^2+(b-y)^2} - \frac{x+\frac{a}{2}}{\left(x+\frac{a}{2}\right)^2+(b-y)^2} = 0;\ i.\ e.$$

if
$$\left(x-\frac{a}{2}\right)\left(x+\frac{a}{2}\right)\left(x+\frac{a}{2}-\overline{x-\frac{a}{2}}\right)-(b-y)^2\left(x+\frac{a}{2}-\overline{x-\frac{a}{2}}\right)=0;$$

or if
$$x^2-\frac{a^2}{4}-(b-y)^2=0.$$

Consequently the particles of the fluid which lie *above* the equilateral hyperbola defined by this equation will commence to move upwards: those which lie *below* it will commence to move downwards; and those which lie along it will at the first instant be at rest.

4. $\dfrac{d\phi}{dy}=\dfrac{b\,g\,t\,a}{\pi}\;\dfrac{\left(x^2-\frac{a^2}{4}\right)-(b-y)^2}{r^2\,.\,r'^2}$ where r, r' are the distances of the point whose motion is determined from the two extremities of the summit of the originally elevated fluid.

Combining these remarks with the conclusions previously arrived at, Art. 70, we conclude that no part of the fluid, except that which is in the immediate vicinity of the disturbed particles, commences to move with a wave motion; but that it gradually swells so as to diminish the quantity of fluid which actually constitutes the volume of a wave.

These conclusions are of sufficient importance to warrant us in bestowing a little more attention on this part of the subject; especially as M. Poisson's result, although in some points the same as our own, in others differs materially from it. For instance, M. Poisson's line of no vertical velocity is a *straight line.*

73. Let us then determine the value of ϕ corresponding to very small values of t:

We have
$$\phi=-\frac{b\,g\,t}{\pi}\int_0^\infty\frac{\sin\mu\,\overline{\frac{a}{2}-a}+\sin\mu\,\overline{\frac{a}{2}+a}}{\mu}\left(1-\frac{g\,\mu\,t^2}{6}\right)e^{-\mu(b-y)}\,d\mu$$

But
$$\int_0^\infty d\mu\,e^{-\mu(b-y)}\frac{\sin r\,\mu}{\mu}=\frac{\pi}{2}-\tan^{-1}\frac{b-y}{r}$$

$$\therefore\quad \phi=-\frac{b\,g\,t}{\pi}\left\{\pi-\tan^{-1}\frac{b-y}{\frac{a}{2}-x}-\tan^{-1}\frac{b-y}{\frac{a}{2}+x}\right.$$

$$\left.-\frac{g\,t^2}{6}\left(\frac{\frac{a}{2}-x}{(\frac{a}{2}-x)^2+(b-y)^2}+\frac{\frac{a}{2}+x}{(\frac{a}{2}+x)^2+(b-y)^2}\right)\right\}$$

$$=-\frac{b\,g\,t}{\pi}\left\{\pi+\tan^{-1}\frac{a\,(b-y)}{(b-y)^2+x^2-\frac{a^2}{4}}\right.$$

$$\left.+\frac{g\,t^2}{6}\left(\frac{x-\frac{a}{2}}{(x-\frac{a}{2})^2+(b-y)^2}-\frac{x+\frac{a}{2}}{(x+\frac{a}{2})^2+(b-y)^2}\right)\right\}$$

Hence
$$\frac{d\phi}{dy}=\frac{bgt}{\pi}\left\{\frac{x-\frac{a}{2}}{(x-\frac{a}{2})^2+(b-y)^2}-\frac{x+\frac{a}{2}}{(x+\frac{a}{2})^2+(b-y)^2}\right.$$
$$\left.-\frac{gt^2}{3}\left(\frac{(x-\frac{a}{2})(b-y)}{\{(x-\frac{a}{2})^2+(b-y)^2\}^2}-\frac{(x+\frac{a}{2})(b-y)}{\{(x+\frac{a}{2})^2+(b-y)^2\}^2}\right)\right\}$$

$$\frac{d\phi}{dx}=\frac{bgt}{\pi}\left\{\frac{2ax(b-y)}{\{(b-y)^2+x^2-\frac{a^2}{4}\}^2+a^2(b-y)^2}\right.$$
$$\left.+\frac{gt^2}{6}\left(\frac{(x-\frac{a}{2})^2-(b-y)^2}{\{(x-\frac{a}{2})^2+(b-y)^2\}^2}-\frac{(x+\frac{a}{2})^2-(b-y)^2}{\{(x+\frac{a}{2})^2+(b-y)^2\}^2}\right)\right\}$$

$$\frac{d\phi}{dt}=-\frac{bg}{\pi}\left\{\left(\pi-\tan^{-1}\frac{b-y}{\frac{a}{2}-x}-\tan^{-1}\frac{b-y}{\frac{a}{2}+x}\right)\right.$$
$$\left.+\tfrac{1}{2}gt^2\left(\frac{x-\frac{a}{2}}{(x-\frac{a}{2})^2+(b-y)^2}-\frac{x+\frac{a}{2}}{(x+\frac{a}{2})^2+(b-y)^2}\right)\right\}$$

or if
$$x-\frac{a}{2}=r\cos\theta$$
$$x+\frac{a}{2}=r'\cos\theta'$$
$$b-y=r\sin\theta=r'\sin\theta'$$

we get
$$\phi=-\frac{bgt}{\pi}\left\{\pi+\theta-\theta'+\frac{gt^2}{6}\left(\frac{\cos\theta}{r}-\frac{\cos\theta'}{r'}\right)\right\}$$
$$\frac{d\phi}{dy}=\frac{bgt}{\pi}\left\{\frac{\cos\theta}{r}-\frac{\cos\theta'}{r'}-\frac{gt^2}{6}\left(\frac{\sin 2\theta}{r^2}-\frac{\sin 2\theta'}{r'^2}\right)\right\}$$
$$\frac{d\phi}{dx}=\frac{bgt}{\pi}\left\{\frac{\sin\theta}{r}-\frac{\sin\theta'}{r'}+\frac{gt^2}{6}\left(\frac{\cos 2\theta}{r^2}-\frac{\cos 2\theta'}{r'^2}\right)\right\}$$
$$\frac{d\phi}{dt}=-\frac{bg}{\pi}\left\{\pi+\theta-\theta'+\frac{gt^2}{2}\left(\frac{\cos\theta}{r}-\frac{\cos\theta'}{r'}\right)\right\}$$

These formulæ are very elegant and symmetrical.

COR I. $\frac{d^2\phi}{dt^2}+g\frac{d\phi}{dy}=0.$

We know that this equation holds true approximately for the surface: it appears from the results above, that at the very beginning of the motion the equa-

tion holds true at all points. It is, however, accurately true when t has a value only in the case in which $\frac{\sin^2\theta}{r} - \frac{\sin^2\theta'}{r'} = 0$, that is, for the surface, or at least near it.

Cor. 2. When x lies between $\frac{a}{2}$ and $-\frac{a}{2}$, $b-y$ is zero at the commencement of the motion;

$\therefore$ when $t=0$, and $b-y=0$

$\frac{d\phi}{dt} = -bg$, which we know to be correct.

Still supposing $b-y=0$, we get

$$\frac{d\phi}{dt} \propto \pi - \tfrac{1}{2}\frac{g t^2 a}{\frac{a^2}{4}-x^2}$$

$$\therefore \quad \frac{d\phi}{dt}=0 \text{ when } t^2=\frac{2\pi}{ga}\left(\frac{a^2}{4}-x^2\right)$$

$$=\frac{\pi a}{2g} \text{ at the origin.}$$

This gives the time at which the surface has attained the statical level.

Cor. 3. If $b-y$ be neglected in comparison with x', we have

$$\frac{d\phi}{dy}=0 \text{ when } \frac{1}{x-\frac{a}{2}} - \frac{1}{x+\frac{a}{2}} = \frac{g t^2}{3}(b-y)\left\{\left(\frac{1}{x-\frac{a}{2}}\right)^3 - \left(\frac{1}{x+\frac{a}{2}}\right)^3\right\}$$

$$\therefore \quad \frac{g t^2}{3}(b-y) = \frac{\left(x^2-\frac{a^2}{4}\right)^2}{3x^2+\frac{a^2}{4}}$$

or if x be large, $\quad g t^2(b-y)=x^2$

$$t^2=\frac{x^2}{g(b)} \text{ at the surface.}$$

This expression gives the time at which the swell has attained its maximum.

Cor. 4. The height to which it attains is approximately

$$\tfrac{1}{2}\frac{d\phi}{dz}t = \frac{bgt^2}{2\pi}\;\frac{a}{x^2-\frac{a^2}{4}}$$

$$=\frac{ba}{2\pi(b)}\;\frac{x^2}{x^2-\frac{a^2}{4}} = \frac{ba}{2\pi(b)}$$

$$=\frac{a}{2\pi}$$

This result is curious, inasmuch as it shews that the swell depends on the extent of surface disturbed, and not on the magnitude of the disturbance.

COR. 5. $$\frac{d\phi^2}{dx^2}+\frac{d\phi^2}{dy^2}=\frac{b^2g^2t^2}{\pi^2}\cdot\frac{r'^2+r^2-2rr'\cos\overline{\theta'-\theta}}{r'^2r^2}$$

$$=\frac{b^2g^2t^2\,.\,a^2}{\pi^2r'^2r^2}$$

$$\therefore\ \text{velocity}\ =\frac{bgta}{\pi rr'},$$

which shews that the actual velocity of a particle after a small time t varies inversely as the product of its distances from the two extremities of the displaced mass.

COR. 6. If a be small, velocity $\propto$ inversely as square of distance from centre of displacement, which is M. POISSON'S result.

74. Let us return to the value of $\frac{d\phi}{dy}$:

It is $$\frac{d\phi}{dy}=-\frac{b\sqrt{g}}{\pi}\int_0^\infty\frac{\sin\mu(a-a')+\sin\mu a'}{\sqrt{\mu}}\sin\sqrt{g\mu}\,t\,e^{-\mu(b-z)}\,d\mu\ \text{ nearly}$$

$$=-\frac{b\sqrt{g}}{2\pi}\int_0^\infty\frac{\cos(\mu\overline{a-a'}-\sqrt{g\mu}\,t)-\cos(\mu\overline{a-a'}+\sqrt{g\mu}\,t)+\&c.\,e^{-\mu(b-z)}}{\sqrt{\mu}}\,du \qquad (a).$$

To find the value of this integral, we will first determine that of the expression

$$\int_0^\infty(\cos(\sqrt{g\mu}\,t+r\mu)\,\frac{e^{-\mu(b-z)}}{\sqrt{\mu}}\,d\mu;$$ which let us designate by V.

Assume $\sqrt{g\mu}\,t+r\mu=\theta$; then $\sqrt{\mu}=-\frac{\sqrt{g}\,.\,t}{2r}+\sqrt{\frac{g\,t^2}{4r^2}+\frac{\theta}{r}}$

$$\therefore\quad\frac{d\mu}{\sqrt{\mu}}=\frac{2\,d\theta}{\sqrt{g\,t^2+4r\theta}},\ \text{and}$$

$$V=\int_0^\infty\cos\theta\,e^{-\mu(b-z)}\frac{2\,d\theta}{\sqrt{g\,t^2+4r\theta}}=\int_0^\infty\frac{2}{\sqrt{g}\,.\,t}\cos\theta\,e^{-\mu(b-z)}\,d\theta\left(1-\frac{2r\theta}{g\,t^2}\right).$$

Now $$\mu=\frac{1}{4r^2}\left(\sqrt{g}\,t-\sqrt{g\,t^2+4r\theta}\right)^2$$

$$=\frac{1}{4r^2}(2g\,t^2-2g\,t^2\sqrt{1+\frac{4r\theta}{g\,t^2}}+4r\theta)$$

$$=\frac{1}{4r^2}2g\,t^2\,.\,\frac{2r^2\theta^2}{(g\,t^2)^2}=\frac{\theta^2}{g\,t^2}\ \text{nearly.}$$

Hence $$V=\frac{2}{\sqrt{g}\,t}\int_0^\infty\cos\theta\,e^{-\frac{b-z}{g\,t^2}\theta^2}\left(1-\frac{2r\theta}{g\,t^2}\right)d\theta$$

$$=\frac{2n}{\sqrt{g}\,.\,t}\int_0^\infty\cos n\phi\,e^{-\frac{\phi^2}{2}}\left(1-\frac{2rn\theta}{g\,t^2}\right)d\phi$$

if $\theta=n\phi$; and $n=\sqrt{\frac{g\,t^2}{2(b-z)}}$.

Now $\int_0^\infty \cos n\phi e^{-\frac{\phi^2}{2}} d\phi = \int_0^\infty \frac{1}{2}\left(e^{-\frac{\phi^2}{2}+n\phi\sqrt{-1}} + e^{-\frac{\phi^2}{2}-n\phi\sqrt{-1}}\right) d\phi$

Let $$A = \int_0^\infty e^{-\frac{\phi^2}{2}+a\phi} d\phi.$$

$$\therefore \quad \frac{dA}{da} = \int_0^\infty e^{-\frac{\phi^2}{2}+a\phi} \phi\, d\phi$$

$$= -\int_0^\infty e^{-\frac{\phi^2}{2}+a\phi} (-\phi+a-a)\, d\phi$$

$$= -e^{-\frac{\phi^2}{2}+a\phi} + aA + \text{const.}$$

$$= 1 + aA$$

$\therefore$ by integration $$A \cdot e^{-\frac{a^2}{2}} = \int da\, e^{-\frac{a^2}{2}} + C.$$

Let A_0 be the value of A when $a=0$.

$$\therefore \quad A_0 = \int^0 da\, e^{-\frac{a^2}{2}} + C = \int_0^b db\, e^{-b^2} \sqrt{2} = \sqrt{\frac{\pi}{2}}$$

and $$Ae^{-\frac{a^2}{2}} - A_0 = \int_0^a da\, e^{-\frac{a^2}{2}}$$

$$A = A_0 e^{\frac{a^2}{2}} + e^{\frac{a^2}{2}} \int_0^a da\, e^{-\frac{a^2}{2}}$$

By putting $n\sqrt{-1}$ and $-n\sqrt{-1}$ successively for a, and adding, we get

$$\int_0^\infty \cos n\phi\, e^{-\frac{\phi^2}{2}} d\phi = \sqrt{\frac{\pi}{2}} e^{\frac{a^2}{2}} + \frac{e^{\frac{a^2}{2}}}{2} \int_0^a da\, e^{-\frac{a^2}{2}} + \frac{e^{\frac{a^2}{2}}}{2} \int_0^{-a} da\, e^{-\frac{a^2}{2}} = \sqrt{\frac{\pi}{2}} e^{-\frac{n^2}{2}},$$

since the last terms destroy each other.

Also $$\int_0^\infty \phi \cos n\phi\, e^{-\frac{\phi^2}{2}} d\phi = \frac{1}{2}\int_0^\infty \phi\, d\phi \left(e^{-\frac{\phi^2}{2}+a\phi} + e^{-\frac{\phi^2}{2}-a\phi}\right).$$

Now $$\int_0^\infty \phi\, d\phi\, e^{-\frac{\phi^2}{2}+a\phi} = \int_0^\infty (a-\overline{a-\phi})\, d\phi\, e^{-\frac{\phi^2}{2}+a\phi}$$

$$= aA - e^{-\frac{\phi^2}{2}+a\phi} \text{ between limits;}$$

$$= aA + 1 = 1 + ae^{\frac{a^2}{2}} \left(\sqrt{\frac{\pi}{2}} + \int_0^a d e^{-\frac{a^2}{2}} da\right)$$

$$\therefore \quad \int_0^\infty \phi\, d\phi\, e^{-\frac{\phi^2}{2}-a\phi} = 1 - ae^{\frac{a^2}{2}} \left(\sqrt{\frac{\pi}{2}} + \int_0^{-a} e^{-\frac{a^2}{2}} da\right).$$

Hence $\int_0^\infty \phi \cos n\phi\, e^{-\frac{\phi^2}{2}} d\phi = 1 + \frac{a e^{\frac{a^2}{2}}}{2} \int_{-a}^{a} e^{-\frac{a^2}{2}} da$

$$= 1 + \frac{n\sqrt{-1}}{2} e^{-\frac{n^2}{2}} \int_{-n\sqrt{-1}}^{n\sqrt{-1}} e^{-\frac{a^2}{2}} da$$

$$\therefore \quad V = \frac{2n}{\sqrt{g}\,.\,t}\left\{\sqrt{\frac{\pi}{2}}\, e^{-\frac{n^2}{2}} - \frac{2nr}{gt^2}\left(1 + \frac{n\sqrt{-1}}{2} e^{-\frac{n^2}{2}} \int_{-n\sqrt{-1}}^{n\sqrt{-1}} e^{-\frac{a^2}{2}} da\right)\right\}.$$

By putting $-(a-a')$ and $a-a'$ successively for r, and subtracting the results, we shall obtain the terms corresponding to $a-a'$ in $\frac{d\phi}{dy}$, and by adding to this result a similar expression with a' instead of $a-a'$, we shall obtain the whole of the expression under the integral sign in equation (a).

Now the first difference is

$$\frac{2n}{\sqrt{g}\,.\,t} \cdot \frac{4n(a-a')}{gt^2}\left(1 + \frac{n\sqrt{-1}}{2} e^{-\frac{n^2}{2}} \int_{-n\sqrt{-1}}^{n\sqrt{-1}} e^{-\frac{a^2}{2}} da\right)$$

and the second

$$\frac{2n}{\sqrt{g}\,.\,t} \quad \frac{4na'}{gt^2}\quad\left(1 + \frac{n\sqrt{-1}}{2} e^{-\frac{n^2}{2}} \int_{-n\sqrt{-1}}^{n\sqrt{-1}} e^{-\frac{a^2}{2}} da\right)$$

$$\therefore \quad \frac{d\phi}{dy} = -\frac{b\sqrt{g}}{2\pi} \cdot \frac{2n}{\sqrt{g}\,.\,t} \cdot \frac{4na}{gt^2}\left(1 + \frac{n\sqrt{-1}}{2} e^{\frac{n^2}{2}} \int_{-n\sqrt{-1}}^{n\sqrt{-1}} e^{-\frac{a^2}{2}} da\right)$$

Now $\int e^{-\frac{a^2}{2}} da = -\frac{e^{-\frac{a^2}{2}}}{a} - \int \frac{e^{-\frac{a^2}{2}} da}{a^2}$

$$= -\frac{e^{-\frac{a^2}{2}}}{a} + \frac{e^{-\frac{a^2}{2}}}{a^3} - \frac{3e^{-\frac{a^2}{2}}}{a^5} + \frac{5e^{-\frac{a^2}{2}}}{a^7} - \text{\&c.}$$

$$\therefore \quad \int_{-n\sqrt{-1}}^{n\sqrt{-1}} e^{-\frac{a^2}{2}} da = -\frac{2}{n\sqrt{-1}} e^{\frac{n^2}{2}} \left(1 + \frac{1}{n^2} + \frac{1\,.\,3}{n^4} + \frac{1\,.\,3\,.\,5}{n^6} + \text{\&c.}\right)$$

$$\therefore \quad \frac{d\phi}{dy} = -\frac{4bn^2a}{\pi g t^3}\left(1 - 1 - \frac{1}{n^2} - \frac{1\,.\,3}{n^4} - \text{\&c.}\right)$$

$$= \frac{4ba}{\pi g t^3}\left(1 + 1\,.\,3\,\frac{2(b-z)}{gt^2} + 1\,.\,3\,.\,5\,\frac{4(b-z)^2}{g^2t^4} + \text{\&c.}\right)$$

Hence, as the time increases, the velocity tends to vary inversely as the cube of the time; a result obtained by very different processes as well by M. Poisson as by M. Cauchy.

II. Let us next suppose that the motion has been produced by an impulse at the surface of the fluid, but without any elevation or depression of the surface.

Hence, at the surface, when $t=0$, $\phi=f(a)$, and $\frac{d\phi}{dt}=0$

between the limits $x=k$ and $x=k+a$: This gives

$$f(a')=\int_0^\infty \cos m\,(a-k)\,(1+e^{-2mh})\,\phi'(m)\,dm \text{ by equation (6);}$$

and $\psi'(m)=0$, since $\frac{d\phi}{dt}=0$ when $t=0$.

$$\therefore \quad \phi=\int_0^\infty \cos m\,x'\,(e^{my}+e^{-m(y+2h)})\,\phi'(m)\cos c_m\,t\,dm$$

therefore in this case, writing a' for $a-k$, and therefore x' for $x-k$, we get

$$\phi(m)=(1+e^{-2mh})\,\phi'(m)$$

$$\text{and } \gamma(m)=\frac{e^{my}+e^{-m(y+2h)}}{1+e^{-2mh}}\cos c_m t$$

$$\therefore \quad \phi=\frac{1}{\pi}\int_{-\infty}^{\infty}\int_0^\infty \cos\mu\,(\varpi-x')\cos c_m\,t\,\frac{e^{\mu y}+e^{-\mu(y+2h)}}{1+e^{-2\mu h}}\,F(\varpi)\,d\varpi\,d\mu$$

where $F(\varpi)=f(\varpi-k)$.

If $f(a')$ be constant between the limits k and $k+a$, and equal to C, we have

$$\int_{-\infty}^{\infty} F(\varpi)\,d\varpi=Ca$$

$$\therefore \quad \int_{-\infty}^{\infty} F(\varpi)\cos\mu\,(\varpi-x)=\frac{C}{\mu}(\sin\mu\,\overline{a-x+k}+\sin\mu\,\overline{x-k})$$

$$\therefore \quad \phi=\frac{C}{\pi}\int_0^\infty \frac{\sin\mu\,\overline{a-x+k}+\sin\mu\,\overline{x-k}}{\mu}\,.\,\frac{e^{\mu y}+e^{-\mu\,\overline{y+2h}}}{1+e^{-2\mu h}}\cos c_\mu\,t\,.\,d\mu.$$

From the circumstance that the present formula contains only $\cos c_\mu t$, whereas the formula deduced for the preceding case contains only $\sin c_\mu t$, it follows that if, at the same time, there be an elevation of the fluid and an impulsion at the surface, the total effect will be the *sum* of the two effects estimated independently of each other. This conclusion relieves us from the necessity of specifically considering the case in which both are united.

With two or three conclusions from the above formula, we shall conclude the present memoir.

1. If $t=0$, we may for the surface omit y, which is very small, and we shall obtain

$$\phi=\frac{C}{\pi}\int_0^\infty \frac{\sin\mu\,(a-x+k)+\sin\mu\,(x-k)}{\mu}\,\frac{1+e^{-2\mu h}}{1+e^{-2\mu h}}\,d\mu$$

if $\qquad a>x-k$;

and

$$\phi=\frac{C}{\pi}\int_0^\infty \frac{\sin\mu\,(x-k)-\sin\mu\,(x-k-a)}{\mu}\,d\mu$$

if $\qquad a<x-k$.

But generally

$$\int_0^\infty \frac{\sin q\mu}{\mu}\,d\mu=\frac{\pi}{2},$$

therefore in the latter case

$$\phi=\frac{C}{\pi}\left(\frac{\pi}{2}-\frac{\pi}{2}\right)=0;$$

in the former

$\phi=\frac{C}{\pi}\left(\frac{\pi}{2}+\frac{\pi}{2}\right)=C$ as it evidently ought to be. Hence it appears that the instantaneous impulse conveyed to any point in the *surface* of the fluid is exceedingly small.

2. If

$$y=-h$$

$$\phi=\frac{2C}{\pi}\int_0^\infty \frac{\sin\mu(a-x+k)+\sin\mu(x-k)}{\mu}\frac{e^{-\mu h}}{1+e^{-2\mu h}}$$

$$=\frac{2C}{\pi}\int_0^\infty \frac{\sin\mu(a-x+k)+\sin\mu(x-k)}{\mu}e^{-\mu h}(1-e^{-2\mu h}+e^{-4\mu h}-\&c.)$$

But

$$\int_0^\infty \frac{\sin\mu r e^{-\mu s}}{\mu}d\mu=\tan^{-1}\frac{r}{s}$$

$$\therefore \quad \phi=\frac{2C}{\pi}\left\{\tan^{-1}\frac{x-k}{h}+\tan^{-1}\frac{a-x+k}{h}-\tan^{-1}\frac{x-k}{3h}-\tan^{-1}\frac{a-x+k}{3h}\right.$$
$$\left.+\tan^{-1}\frac{x-k}{5h}+\tan^{-1}\frac{a-x+k}{5h}-\&c.\right\}$$

$$=\frac{2C}{\pi}\left\{\frac{x-k}{h}-\frac{1}{3}\left(\frac{x-k}{h}\right)^3+\frac{1}{5}\left(\frac{x-k}{h}\right)^5+\&c.\right.$$
$$+\frac{a-x+k}{h}-\frac{1}{3}\left(\frac{a-x+k}{h}\right)^3+\frac{1}{5}\left(\frac{a-x+k}{h}\right)-\&c.$$
$$-\frac{x-k}{3h}+\frac{1}{3}\left(\frac{x-k}{3h}\right)^3-\frac{1}{5}\left(\frac{x-k}{3h}\right)^5+\&c.$$
$$-\left(\frac{a-x+k}{3h}\right)+\frac{1}{3}\left(\frac{a-x+k}{3h}\right)^3-\frac{1}{5}\left(\frac{a-x+k}{3h}\right)^5+\&c.$$
$$\left.+\quad \&c. \quad \&c.\right\}$$

$$=\frac{2C}{\pi}\left\{\frac{a}{h}-\frac{1}{3}\frac{a}{h}+\frac{1}{5}\frac{a}{h}-\&c.\right.$$
$$-\frac{1}{3}\left(\frac{a}{h}\right)^3+\frac{1}{3}\frac{1}{3^3}\left(\frac{a}{h}\right)^3-\frac{1}{3}\frac{1}{5^3}\left(\frac{a}{h}\right)^3+$$
$$\left.+\quad \&c. \quad \&c.\right\}$$

$=\frac{2C}{\pi}\frac{a}{h}\frac{\pi}{4}$ nearly; if $x-k$ be small and h considerable. Hence we learn that the impulse instantaneously communicated to the bottom of the canal varies inversely as the depth.

The few cases we have exhibited above, must not be supposed to include all that our analysis is capable of developing. We have given these cases rather

as examples than as attempts to produce the complete solution of the problem. Hitherto our endeavours to deduce a relation between the depth of the fluid and the length of the wave have been unavailing, principally (as I think) from the difficulty attendant on summing slowly converging series. I trust what has been done will serve to introduce the subject to mathematicians of this country, who do not appear as yet to have taken a very lively interest in the theory, zealous as they are in prosecuting the experimental study of fluid motion.

VIII.—*Examination and Analysis of the Berg-Meal, or Mineral Flour, found in the Parish of Degersfors, in the Province of West Bothnia, on the confines of Swedish Lapland.* By Thomas Stewart Traill, M.D., Professor of Medical Jurisprudence in the University of Edinburgh.

(Read 18th January 1841.)

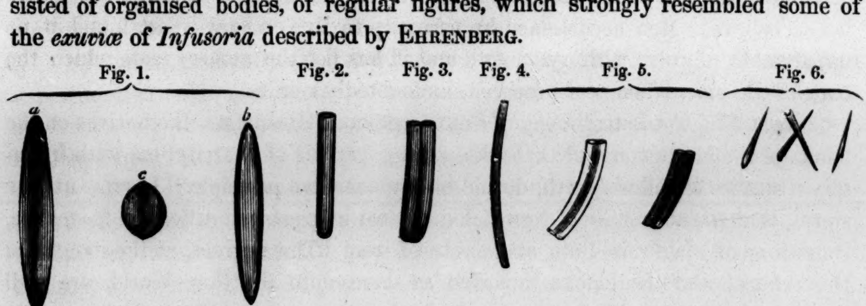
In 1832 or 1833, a peasant, in felling a tree in the forest about forty miles above Degersfors, laid bare a substance strongly resembling meal, which, tempted by its appearance, he baked with a mixture of rye-flour, and used as bread. "All the world," says Mr Laing, "of this and the next parish, flocked to the spot to take their part of this extraordinary blessing of meal, produced in the earth at a time when they were reduced to bark bread. The functionaries of the district at last heard of it, and gave orders that it should not be used until they had ascertained that it was safe. Some of it was sent to Stockholm to be analyzed." Mr Laing has stated, in his Tour, that it was said to consist chiefly of finely pulverised flint and felspar, with a residuum of organised matter; but the proportions, or the regular analysis, he could not learn.

Mr Laing procured specimens of this curious *Berg-Meal;* and, on the return of my friend and relative from his northern tour, he was so kind as put them into my possession for examination, in the end of 1838.

I soon ascertained that this substance really contained organic matter; for, when heated, it first became black at a red heat, exhaling a smell like a mixture of vegetable and animal matter, and when the heat was increased, it burnt to snowy whiteness. By exposing a portion of it to a red heat in a glass tube, I found that it gave out ammonia in sufficient quantity to restore the colour of litmus paper, reddened by weak acetic acid.

Thus satisfied of the presence of animal matter in the *Meal,* I proceeded to examine it with the microscope, and was surprised to find that it chiefly consisted of organised bodies, of regular figures, which strongly resembled some of the *exuviæ* of *Infusoria* described by Ehrenberg.

Fig. 1. Fig. 2. Fig. 3. Fig. 4. Fig. 5. Fig. 6.



The most conspicuous figures in the powder, when highly magnified, had the form of thin, translucent, elongated, elliptical bodies, longitudinally divided by a less translucent septum or canal, as in fig. 1. The largest of this form measured 0.006 of an inch; but their general size was 0.002 in length, by 0.0005 in greatest breadth. The elliptical body fig. 1, *b.*, with darker margins, measured 0.006 by 0.0005. The other *c* was equal to 0.0006 by 0.0005. Mixed with these, I observed many tubular bodies, some of which are straight, others gently curved. Fig. 2 measures 0.002 by 0.0001 inch in diameter. Fig. 3 is a double tube of the same length, but twice the breadth. Fig. 4 is a slender tube, the length of which I forgot to measure; but it seemed to me about one-third, at least, longer than fig. 2. Fig. 5 is of various sizes; but one of the most perfect measured 0.002 in length. The spicula, fig. 6, vary greatly in size,—from 0.0001 to 0.002. The principal mass of the powder consists of fragments of the forms now described, and of minute granules, which seemed to have a regular oval form, and could not be above one thirty-thousandth of an inch in their longest diameter.

Anxious to ascertain whether, besides these bodies—which EHRENBERG would consider as animal—the powder does not contain vegetable remains, I submitted a part of it to Dr GREVILLE, who has favoured me with the following remarks.

"I have carefully examined the Berg-Meal, and find it full of siliceous remains of minute animals. There appear to be a few forms, also, of those minute *Algæ* which have a siliceous structure; especially the curved tubular bodies, fig. 5, and possibly also the oval bodies, fig. 1. But if you consider EHRENBERG as decisive authority, I can detect no form that, according to him, is not animal." Dr GREVILLE is of opinion that the *diatomacea* are really vegetable productions, and that the forms in question belong to that class of beings. Be this as it may, it is sufficient to state that they are indisputably the remains of organised beings; and as the Berg-Meal contains organic matter destructible by fire, and, as I shall presently shew, partially soluble in water, the peasantry of Swedish Lapland were not so irrational in obstinately adhering to the secret use of their Berg-Meal, in defiance of the local authorities, as we might, at the first glance of the subject, be led to believe. Men accustomed by necessity to live on *bark bread*, found it no unpalatable mixture with rye-flour; and it has not the austere taste which the bark of the pine, when best prepared, undoubtedly retains.

According to the testimony of CONDAMINE and HUMBOLDT, the natives on the banks of the Maragnon and Orinooko, during periods of scarcity, eat with impunity a species of fuller's earth, devoid of any nutritive principle. LABILLARDIERE states, that the aborigines of New Caledonia eat in great quantities a soft steatite, consisting of magnesia, lime, and oxide of iron. The negroes, at the mouths of the Senegal, and the blacks, imported as slaves into the New World, are well known to mix clay with farinaceous food; but the presence of organic matter in

the Swedish Berg-Meal seems to give it a marked superiority, as a substitute for food, over all the earthy substances which are said to enter into the repasts of of the Ottomacks, the Papuas, and the Negroes.

ANALYSIS.

A.

This substance, which appears as a friable powder, is rather soft when rubbed between the fingers; of a colour between greyish-white and wood-brown; blackens at a red heat, giving out a faint, empyreumatic, ammoniacal odour; and finally, when the heat is increased, becomes of a pure white.

1. It does not dissolve in water; but, by long digestion in distilled water, it loses about seven per cent. of its weight, and imparts a yellowish tint to the fluid. This water is quite transparent, even after concentration; yet, on standing for between two and three weeks, a few gelatinous flakes appeared in it, but I found the quantity inappreciable. It also afforded a perceptible, but inappreciable quantity of some muriate.

2. When digested with strong sulphuric acid, it blackens, and a small portion of earthy matter is dissolved.

3. Digested with hydrochloric or with nitric acid, a part of it is also dissolved.

4. The dissolved portion gave no trace of baryta, nor of strontia, nor of magnesia, but a very slight one of lime, by the addition of oxalate of ammonia. The presence of alumina was shewn by the addition of carbonate of ammonia to its solution in nitric acid. Benzoate of ammonia, and ferro-prussiate of potassa, in different portions of the neutralized solutions, indicated the presence of iron.

After these preliminary experiments, I made the following experiments in order to find the relative proportions of its ingredients.

B.

From the difficulty of depriving a powder containing organic matter, wholly of water, without partial decomposition, I was under the necessity of repeatedly performing the process of desiccation and incineration.

100 grains, gradually dried at a heat a little above 212°, were introduced into a platinum-crucible and heated to redness. The mass first blackened and gave out the smell already noticed—it was rather pungent; and when litmus paper, reddened by diluted acetic acid, was exposed to the vapour, its colour was immediately restored; and a rod dipt in hydrochloric acid, exposed to it, instantly produced white fumes; shewing the evolution of ammonia. A full red heat, continued for half an hour, converted it into a snow-white powder, which when weighed before it was quite cold = 78 grains; or the Berg-Meal by incineration had lost 22 per cent. The incineration was five times repeated on different quantities of the

substance, with results so nearly similar, that I consider 22 per cent. as the real proportion of this mineral destructible by a red heat: and as it had first been carefully dried, that number may probably be fairly considered as an approximation to the quantity of organic matter which it contains.

C.

1. 78 grains of the incinerated Berg-Meal (B. 1.) were digested with sulphuric acid, to which distilled water was added, and the digestion continued for 48 hours. The clear liquid was drawn off by the pipette, and the residue, largely diluted with distilled water, was thrown on the filter, when it was well washed, dried, heated to redness in a platinum-crucible, and weighed while still warm. It =71.13 grains, or 8.87 grains had been taken up by the acid.

2. Similar results were obtained by digestion with undiluted hydrochloric acid, which became of a pale straw-colour. The residue very nearly agreed with the above carefully performed experiment (C. 1.)

D.

1. 78 grains of the incinerated mineral were digested with hydrochloric acid (in C. 2.) diluted with distilled water for 48 hours. To the solution neutral benzoate of ammonia was added until all precipitation had ceased. The liquid became turbid and yellowish-brown. Next day the precipitate, in bulky flocks, lay at the bottom of the vessel, leaving the supernatant liquid colourless and transparent. The addition of more of the test produced no further change in the liquid; and therefore I conclude that all the iron had been thrown down. The clear liquor was withdrawn by the pipette; the rest was thrown on the filter and well washed.

2. I found it impossible to separate all the precipitate from the paper, but having dried the part of the filter stained with the iron, I burnt it on a platinum dish, moistened the ash with nitric acid, and exposed the whole to a strong red heat. The residue was a reddish-brown oxide of iron which weighed 0.15 grain.

D.

1. To separate the alumine, 78 grains of the incinerated Berg-Meal were digested in hydrochloric acid as before; and when a diluted clear solution was obtained, it was precipitated by carbonate of ammonia: a gelatinous greyish precipitate resulted, which adhered to the filtering paper. The greatest part of it was removed before it was quite dry. This, when exposed to a red heat, had a reddish-yellow colour; and it was found that the iron was precipitated with the alumine. The filter was burnt and treated as before (D. 2.), and the whole together weighed 5.46 grains. The former experiment (D. 1.) had shewn that the quantity of oxide of iron = 0.15 grain, and, therefore, the real quantity of alumine in the Berg-Meal = 5.31 grains.

2. The examination of the residual liquid, after the separation of the iron,

almost coincided to a few hundredths of a grain with this, which I considered as the most accurate experiment.

3. In one small parcel of the Berg-Meal, differing a little in colour from that of which the analysis is now given, I found a larger quantity of iron, and about 0.02 of a grain of lime: but as the iron in the Berg-Meal, by several experiments, was only from 0.15 to 0.156, and the quantity of lime wholly inappreciable, I am disposed to consider these discrepancies as arising from accidental causes; and I think we shall not greatly err in considering the following as the composition of the Berg-Meal when dried.

22.00	Organic matter destructible by heat.
71.13	Silica.
5.31	Alumina.
0.15	Oxide of iron.
98.59	
1.41	Loss.
100.00	

In this loss must be included the trace of lime in the solutions, and still slighter trace of muriates which the aqueous decoction exhibited. But I am inclined to suppose, that the principal part of the apparent loss is to be attributed to the difficulty of obtaining, in a state of uniform dryness, a powder which contains matter destructible by heat.

It is quite obvious also, that in a powder exposed, as the Berg-Meal appears to be in its native repository, to accidental mixtures from the influence of decaying vegetables, and from the percolation of water charged with earthy matter, in different situations, and even in different parts of the same bed, its accidental ingredients may vary; but this analysis sufficiently proves that its principal and essential chemical ingredients are *organic matter* and *silica*, both derived from the decomposition of beings once endowed with the principle of life.

Perhaps no geological speculation is more calculated to excite surprise and admiration than a reflection on the countless myriads of animated beings, whose remains, even to the naked eye, now appear to fill our calcareous formations and our coal-beds: but how much is this sentiment increased, since Ehrenberg's discoveries have taught us to consider the chalk (which forms whole districts in Europe), polishing slate, and some other minerals, as aggregations of the *exuviæ* of animals so inconceivably minute, that a single cubic inch of chalk contains 1,382,400 individuals!! The *Berg-Meal* of Swedish Lapmark adds another link to the infinite chain of organized existences, whose delicate structure, symmetry, and astonishing minuteness, are not among the least wonderful of the works of the supreme Creator.

IX.—*Farther Researches on the Voltaic Decomposition of Aqueous and Alcoholic Solutions.* By Arthur Connell, Esq., F. R. S. Ed.

(Read 15th February 1841.)

Since my last communication to the Society on this subject, I have continued my experimental investigation of the proposed law which limited the direct action of the voltaic current to the solvent, in solutions of primary combinations of elementary bodies in the more important solvents. All my farther researches have confirmed the rule in regard to aqueous solutions; and I feel now fully convinced of its truth, although, in the mean time, I have had occasion to see a different view advocated by some other experimenters, to whose opinions I shall afterwards advert. Neither have I seen any grounds for altering my views in regard to alcoholic solutions. In regard to ether, some experiments which I shall afterwards mention, have satisfied me that it would be improper at present to include that solvent in any general rule.

I.—*Aqueous Solutions.*

I need not revert to the proof adduced in my former papers* of the secondary decomposition of the hydracids in their aqueous solutions; nor to that of the secondary origin of iodine in a solution of bromide of iodine.†

With respect to the oxyacids, in addition to the experiments on the sulphuric, boracic, and iodic acids, formerly detailed, which led to the inference, from the quantity of gases evolved, that such acids are not directly decomposed in their aqueous solutions;‡ I have now to mention a still more direct method of arriving at the same conclusion in regard to iodic acid, and, by analogy, in regard to other oxyacids. A moderately strong solution of iodic acid, mixed with a starch solution, was placed in the tube B,§ and was made positive by a power of 50 pairs of two-inch plates; whilst a starch solution placed in the tube A was made negative, the connection being by asbestos moistened with starch solution. Effervescence soon ensued from both poles; but in half an hour there was no trace of any formation of blue matter at the negative pole or in any part of either tube. The battery was then reversed, when blue matter appeared in two minutes on the negative foil, without effervescence, or scarcely any. Thus, in the first position

* Edin. Trans. vol. xiii. p. 339, and xiv. p. 116.
† Ibid. xiv. p. 119.
‡ Ibid. xiii. 338.
§ Fig. 1, Pl. II. vol. xiv. Edin. Trans., bottom of Plate.

of the battery, the iodine did not pass towards the negative pole, and the acid, consequently, did not suffer voltaic decomposition; whilst on reversal iodine very soon appeared at that pole, by the reducing action of hydrogen.

In regard to oxides, it is not easy to obtain such direct experimental evidence, because the metals of such as are soluble in water react on the solvent: but we may take the decomposition of metallic oxides, as contained in soluble salts, during which the metal frequently appears at the negative pole, as illustrating the action on solutions of such oxides as are dissolved by pure water. It is now pretty generally admitted, that when metal appears in solutions of metallic salts at the negative pole, it is due to the reducing action of nascent hydrogen; and this opinion I have verified directly by finding that, when a solution of sulphate of copper was made positive, by 50 pairs of 2-inch plates, and connected by asbestos with distilled water which was made negative, neither metallic copper nor oxide was carried towards the negative pole during half an hour's action; whilst, on reversal of the battery, the now negative foil was found in a quarter of an hour to be coated with metallic copper without any elastic fluid from that pole. It was also found, by a similarly arranged experiment with an aqueous solution of chloride of zinc, that neither zinc nor oxide was carried in a similar time to the negative pole, when that pole was placed in distilled water; but that, after reversal, metallic zinc soon appeared at that pole, from the reducing action of hydrogen; and holding, as I do, from the experiments formerly detailed,* that chloride of zinc is dissolved as a muriate, the above result affords the same illustration as that with sulphate of copper. A similar result was obtained when distilled water was carefully poured over a concentrated solution of muriate of zinc in a bent tube, instead of connecting the liquids by asbestos, and the poles plunged separately into the two liquids; the only difference being, that, previous to reversal, a little oxide of zinc appeared to be formed at the boundary of the water and the metallic solution.

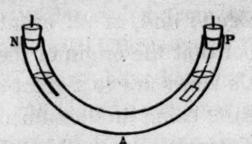
I thought it likely that the employment of a negative pole of metallic tellurium might have thrown farther light on this matter; since, if the tellurium in the solution of a common metal combines with hydrogen, as it does under ordinary circumstances of voltaic action, no metal ought to be reduced from the solution. But I found that, when so employed, copper and zinc still appear at the negative pole. This will doubtless be thought by some a proof in favour of the direct decomposition of the metallic oxide; but, on due consideration, it appears to me to be quite insufficient to establish it. If the oxide really suffers direct decomposition, it seems evident that in the experiments above detailed, in which metal appeared, on reversal, at the negative pole, that either it, or at least oxide, ought also to have appeared proceeding towards that pole previous to reversal. It is impossible to perceive what should constitute the differ-

* Edin. Trans. vol. xiv. p. 127.

ence between the two cases. The experiment with tellurium is therefore, I conceive, to be explained simply by supposing that, in the presence of a readily reducible metallic oxide, the nascent hydrogen rather combines with the oxygen of that oxide than with tellurium.

In DAVY's Bakerian Lecture of 1807, he states, that in one experiment, where nitrate of silver was made positive, and distilled water negative, the asbestos which formed the connection was found to be coated with metallic silver. I do not doubt the result; but I conceive that, under the powerful voltaic action employed (apparently 100 pairs of 4 or 6 inch plates), the oxide of silver carried over to the negative side had suffered reduction either by the simple action of light, or more probably by hydrogen evolved in the negative tube. I made the same experiment, using only fifty pairs of 2-inch plates, and found that neither silver nor oxide appeared on the asbestos in forty minutes, but, on reversal, the negative foil was found fringed with metallic silver in half an hour. A strong solution of nitrate of silver was also placed in a bent tube, and distilled water poured above it. The negative foil was then plunged in the distilled water, and the positive in the solution, but no oxide nor silver appeared anywhere in half an hour, whilst, on reversal, metallic silver began to be formed on the negative foil in ten minutes. What, then, is the difference between the two positions? Simply this, that, in the second, nascent hydrogen is evolved in the solution itself.

In addition to the proof formerly adduced* of the secondary origin of the electro-negative constituent of the haloid salts in their aqueous solutions, I may mention a very simple experiment which leads to the same conclusion, and from being capable of extension, as shall be afterwards shewn, to alcoholic solutions of those salts, throws light on their nature also. If an aqueous solution of iodide of potassium is acted on, using platinum poles, iodine immediately separates at the positive pole, and is dissolved by the liquid giving it a deep red colour. But if, instead of platinum, a positive pole of zinc is employed, the liquid being in the bent tube A, and the power fifty pairs of 2-inch plates, there is not the slightest



discoloration of the liquid; but a speedy and copious deposition of oxide of zinc takes place from the *positive pole*, with only a bubble or two of elastic fluid, whilst at the negative pole there is brisk effervescence. These appearances are inexplicable, unless on the idea of the secondary origin of the iodine; and they are best explained by assuming that the oxygen from decomposed water combines with

* Edin. Trans. xiii. 344, and xiv. 118.

the zinc, and causes the precipitation of oxide of zinc in an apparently anomalous manner at the positive pole; and when platinum is used, it unites with the hydrogen of the acid of the salt, and liberates iodine. When chlorides, as those of potassium or calcium, are employed, there is in like manner a separation of oxide of zinc at the positive pole; and a portion of the oxide which is taken up by the acid is sometimes carried to the negative pole, where it separates along with reduced zinc.

I conceive that a sufficient number of cases has now been investigated to warrant the general conclusion, that, "When aqueous solutions of primary combinations of elementary bodies are submitted to voltaic agency, the dissolved substance is not directly decomposed by the current, but only the solvent." The rule of course does not embrace combinations of the second order, such as oxisalts, in the solutions of which acid and base, as is well known, go to their proper poles, under the direct influence of the current.

It does not appear to me to be necessary to dwell long on the views of MATTEUCCI and Professor DANIELL, both of whom have recently advocated the primary origin of metal in aqueous solution, whether of haloid or oxisalts. MATTEUCCI* argues, because a weak pile, incapable of decomposing distilled water, effects the decomposition of haloid salt solutions, the haloid salts must be more readily decomposed than water, and must be the subject of direct action in their solutions. It is evident, however, that the affinity of the elements of water for the constituents of the salts which they find at the poles, and the inferior conducting power of the solutions, afford the true solution of this observation. He also founds on some experiments with basic acetate of lead, which, he says, both in its fused state and in solution, yields less lead than the equivalent proportion; but no account is given of the exact quantity got in the former situation, nor is any accurate correspondence shewn to exist between the quantities in the two cases, and the whole seems explicable on the idea of such a mode of union between acid and base as to make the salt less susceptible of voltaic action, and of the reducing agency of hydrogen; and, at all events, I conceive that the above experiments afford direct proof that the origin of the metal is not due to primary decomposition. Mr DANIELL's views are to a great extent theoretical; the direct decomposition of the haloid salts being first assumed, and an attempt then made to extend the analogy to the oxisalts, on the hypothesis that the latter, in solution, consist of metal united to acid, plus an atom of oxygen, and to embrace, on a similar analogy, the ammoniacal salts on the idea of the existence of ammonium in them. All these views must assume, as their basis, that metal passes by the direct action of the current to the negative pole, an assumption which is, I conceive, directly negatived by the experiments above detailed. These views are

* Bib. Univ.

equally inconsistent with the experiments so often detailed, shewing the secondary origin of the electro-negative constituents of the haloid salts, and with the separation of oxide of zinc from a positive zinc pole, as above detailed.

In the whole circumstances, there appears to be no reasonable doubt of the general rule which has been above laid down. No binary combination of elements gives way by direct action, in its aqueous solution; in other words, of all simple substances, oxygen and hydrogen are the most directly opposed in their electric nature, and their combination yields the most readily to voltaic action. When we rise to the next order of combinations, those of binary compounds themselves, a different rule comes into operation; for then the combination is decomposed with the same facility as the solvent itself, acid and alkali going to their respective poles at the same time as the elements of water, and, according to the experiments of Mr Daniell, in the same atomic proportion.

II.—*Alcoholic Solutions.*

Since my former communication, I have made a similar experiment on an alcoholic solution of hydriodic acid, as those formerly detailed on alcoholic solutions of haloid salts, and obtained a similar result.

Alcohol of 0.793 at 62° F. was charged in a little Wolfe's apparatus with hydriodic acid gas, which had been passed over fused chloride of calcium, and was then placed in a tube A* and connected by asbestos with two others B and C, containing distilled water; A being made negative and C positive by a power of 72 pairs of 4-inch plates. Gas soon arose from both poles, but during the first twenty minutes no formation of iodine was any where observed, nor any acid reaction except in A. Afterwards a brown discoloration was observed at the positive pole in C, and at the same time an acid reaction at the same place, and on the asbestos between A and B, and a trace on that between B and C. In half an hour the appearances were the same, but more decided. No iodine was any where observed but in C. The battery was then reversed, when a brown stream from liberated iodine instantly descended from the positive pole, without any elastic fluid from that pole, and with effervescence from the negative pole.

This experiment would of course have had more analogy to those with aqueous solutions, if the tubes B and C had contained alcohol instead of water; but the very feeble conducting power of the former liquid prevented its employment, and the dissolving of any substance to favour the conduction would have interfered with the delicacy of the reactions. The phenomena are, however, best explained on the view that the water of the alcohol only suffers direct decomposition.

The appearances, when a positive pole of zinc is used in alcoholic solutions of the haloid salts, are instructive, because they tend to identify the circumstances

* Fig. 2. Plate II., Ed. Trans. xiv.

both of solution and of voltaic action, with those in aqueous solutions of the same substances.

When a saturated solution of well-dried iodide of potassium in alcohol of 0.7918, at 66° F., was thus acted on by 50 pairs of 2-inch plates in the bent tube, p. 153, oxide of zinc, after a short time, separated at both poles, without any appearance of iodine,* and without effervescence at the positive. These appearances can only be explained on the idea of the secondary origin of the iodine, which appears when, instead of zinc, platinum is employed; and are conformable to the view of the direct decomposition of the water of the absolute alcohol; and the appearance of oxide of zinc at the negative pole, whether it could only have come by solution and voltaic transference, leads, I conceive, to the view that, even in alcoholic solutions of haloid salts, at least those of moderate strength, the salt decomposes the water of the absolute alcohol, and exists in solution as a hydracid salt. But to this latter matter I shall afterwards recur.

When a solution of chloride of lithium, the salt having been heated until it began to rise in vapour, and then dissolved by heat in alcohol of the above strength, was acted on in the same manner, oxide of zinc separated after a little at the negative pole, with effervescence from that pole, but none from the positive; and it was somewhat uncertain whether any separation of the oxide took place on the positive side; but little doubt could exist that the oxide originated, as in the case of iodide of potassium, by the action of the oxygen of water on the zinc, and subsequent solution and transference of the oxide formed. The principal condition of the deposition of oxide of zinc at the positive pole, whether in aqueous or alcoholic solutions, appears to be a pretty rapid formation from brisk action; and the less powerful the acid, and the less its quantity drawn to the positive side, the more of the oxide separates previous to solution and transference.

When an alcoholic solution of fused chloride of calcium was employed, there was a slight appearance of deposition on the negative side, but none on the positive, and no effervescence at the latter pole; the whole action being retarded by the coating of lime which the negative foil soon acquired. With an aqueous solution of chloride of calcium, oxide of zinc separated at both poles.

In regard to alcoholic solutions, therefore, I have seen no reason to depart from the general rule formerly proposed respecting them.

With respect to pyroxylic solutions, I have made few experiments; because, if the general rule holds good in regard to alcohol, there can be little doubt that it will embrace pyroxylic spirit, since, as I formerly shewed, the decomposition of its water is much more readily effected than that of alcohol. I found, experi-

* The zinc pole usually gets slightly blackened both in alcoholic and aqueous solutions, but this darkish matter was carefully examined and found to be merely oxidated zinc.

mentally, that when a solution of dry iodide of potassium in rectified pyroxylic spirit was placed in a tube A,* and water in a tube B, the two being connected by asbestos, and A made negative and B positive by 50 pairs of 2-inch plates, although iodine soon appeared in the neighbourhood of the positive pole in B, yet it was accompanied by acid passing into the water of B; and, after forty minutes' action, these appearances continued the same, only more decided, and without any appearance of iodine elsewhere. There is little doubt that the nature of the action was just the same as in aqueous and alcoholic solutions.

In the whole circumstances, although the evidence may not be of quite so decided a character in some of the cases of alcoholic solutions as in regard to those in water, still I think there need not be much hesitation in laying down, as a still more general proposition than that above stated, that "When solutions of primary combinations of elementary substances in water, and in those liquids, such as alcohol and pyroxylic spirit, which contain water as such as an essential constituent, are submitted to voltaic agency, the dissolved substance is not directly decomposed by the current, but only the water of the solvent."

III.—*Ethereal Solutions.*

Previous to my former communications, I had been unable to find any substance which, by solution in ether, led to any decided symptoms of decomposition under voltaic agency, whether of the dissolved body or of the solvent; and I was thus led to expect that a general rule would be found to exist, by which both that liquid and all bodies dissolved in it resist such agency. This led to the particular form which I formerly proposed provisionally for the general law on the subject; but I have since seen cause to strike ether out of the rule entirely, until farther light can be thrown on the appearances which some of its solutions present under electric action.

I have found that, when highly rectified ether was saturated with dry muriatic acid gas, and the solution submitted to the action of a moderate voltaic power, elastic fluid was given off from the negative pole, and none from the positive, and that the solution acquired a yellow colour, and, on subsequent evaporation, yielded a little of a less volatile liquid, smelling of chlorine. The gas, when examined in the voltaic eudiometer, appeared to be hydrogen, retaining some ethereal vapour even after being washed with water, as a little carbonic acid resulted from the detonation with oxygen.

When dry hydriodic acid gas was conducted into ether, in a little WOLFE's apparatus, the liquid immediately separated into two layers,—a lower, dense, and deep red, and an upper, slightly coloured, which, under voltaic agency, yielded gas from the negative pole.

* Fig. 1, Pl. II., Edin. Trans. vol. xiv.

In regard to the latter of these experiments, there can be no doubt that, in saturating the ether with hydriodic acid gas, decomposition took place; and although the nature of this decomposition was not fully investigated, it seems probable that the lower liquid consisted principally of iodised hydriodic acid, resulting from the combination of oxygen, derived from the ether, with hydrogen, of a portion of the hydriodic acid; on which view, water, of course, would be formed, and become the subject of the subsequent voltaic action. During the saturation of ether with muriatic acid, no signs of decomposition were visible; but still it is not improbable that some internal changes may have taken place, and water resulted from the action, united to muriatic acid. Unless such a view be adopted, I should be inclined to hold that there really was direct decomposition of the muriatic acid by the voltaic current; for when I recollect that pure ether resisted very powerful voltaic currents, and that even potash, which has so wonderful an effect in promoting the galvanic decomposition of the water in absolute alcohol, did not make ether more susceptible of electric agency, I cannot allow myself to suppose that the decomposition, in the case of ethereal solutions of the hydracids, is that of water entering into the constitution of ether; but adhere to the original view, that ether contains no water, and that alcohol consists of ether and of water.

Under the circumstances, the advisable course evidently is, to omit ether altogether from any general rule as to the voltaic decomposition of solutions, and to limit such rule, as above done, to water, and such solvents as have been ascertained to contain water as an essential constituent.

IV.—*On the state in which the Haloid Salts are dissolved by Water and Alcohol.*

I formerly endeavoured to shew, by acting on aqueous solutions of haloid salts,—both poles being placed beyond the solutions in distilled water,—and comparing the results with those obtained by using alcoholic solutions, that the salts exist in the aqueous solutions as hydracid combinations.* The evidence on the subject is, however, quite sufficient, even without any illustration from alcoholic solutions, as the following observations will, I hope, shew.

It is frequently difficult, particularly in the case of iodides, to observe any acid reaction at the positive pole, when both poles are plunged directly into the solution, on account of the reducing action of oxygen on the acid formed. And even in those cases where acid is observed, that circumstance will not of itself illustrate the state of solution; because it might be held that acid is formed by secondary action at the negative pole, from whence it is drawn to the positive. In this way only—on the hypothesis of solution as haloids and direct voltaic decomposition of water only—could the separation of reduced metal at the negative be accounted for. A doubt might also exist whether the acid reaction at the pole, in

* Edin. Trans. xiv. 127.

so far as observed, might not arise from an oxyacid formed by secondary action at the positive. All such objections are, however, obviated, by placing the poles beyond the solution, so as to get quit of secondary actions; and we thus readily observe the transference of acid in all cases in which it takes place, by saving it from secondary decomposition. In this way I shewed, in numerous instances, that acid and alkali went to their respective poles, and that the acid, passing, was the hydracid. These facts, wherever they are observed, are, I apprehend, quite sufficient to prove the haloid to be dissolved as a hydracid salt; for, even laying aside for a moment the experiments by which I have endeavoured to shew that the haloids, if existing as such in water, are not directly decomposed, let us take the different views of the nature of the galvanic action which suggest themselves when both poles are plunged into the solution in the ordinary manner, and consider them on the supposition that haloids are dissolved as such.

First, let us suppose that only one of the two substances—water or haloid, it matters not which—is decomposed, it is evident that we cannot account for the production of acid, where secondary action is excluded. Next, let us suppose that both substances are decomposed, and that either the elements, going to the same pole, unite on their journey, or, by an interchange of elements, the oxygen of water unites with the metal of the haloid, and the hydrogen of the water with the electro-negative constituent of the haloid. The former of these alternatives is contradicted by the fact that the acid formed is a hydracid; and the latter, although it might account for the formation of acid and alkali, would not account for the liberation of the electro-negative constituent of the haloid at the positive pole, and of hydrogen in fixed and definite proportion at the negative, whatever be the strength of the solution. It appears to me, then, to be sufficient, in order to prove the aqueous solution of a haloid as a hydracid salt, to shew the separation of the hydracid by voltaic action, under circumstances which exclude secondary action.

It must, however, always be remembered, that although such production can be readily shewn in many cases of haloids, it does not necessarily follow that this should hold in all cases. We are, of course, best prepared to expect it in the case of haloids, of which the constituents have the strongest affinities for oxygen and hydrogen,—such as the ordinary haloids of the bases of the alkalies and alkaline earths. And, accordingly, I shewed that it applied to chlorides and iodides of potassium and calcium. But farther, it was found to hold good in regard to the ordinary haloids of the common metals of strong affinities, such as zinc. I was prepared, however, to consider it as doubtful what might be the result in regard to those of the noble metals. Accordingly, when a moderately strong solution of chloride of gold was placed in the tube B*, and connected by asbestos with the

* Fig. 3, Pl. II. Edin. Trans. xiv.

tubes A and C, which were filled with distilled water, A being made negative and C positive by a power of 50 pairs of 2-inch plates, no decided indications of the formation of acid were obtained during an hour's action; for although, latterly, there was a slight acid reaction at the positive pole, it was not greater than distilled water itself might have yielded, and there was a trace of alkali at the negative. Before, however, deciding conclusively that chloride of gold in solution does not yield to voltaic action, it would be necessary to repeat the experiment with a more powerful current, because it may possibly only be a case of more difficult electric resolution. In such cases, also, atomic constitution may have a considerable, if not the principal, influence on the result.

Whenever we have obtained an instance of the decided formation of acid in the above circumstances, we may conclude, with every probability, that all haloids of the same nature and atomic constitution, of metals of equal or more powerful affinities, are in the same situation. Thus, having verified the rule for chloride of zinc, we may conclude that all protochlorides of more electro-positive metals, such as manganese, cerium, magnesium, barium, potassium, &c. are dissolved as muriates. On the other hand, for the whole series of metals, of less powerful affinities, as well as for all haloids of more complex atomic constitution, the matter will still require to be investigated, and I propose to make some further researches on the subject.

In regard to sal ammoniac, I found that it was resolved into acid and alkali in the above circumstances; a result shewing that, in solution at least, it is simply muriate of ammonia, and cannot be justly regarded as chloride of ammonium.

The same reasoning above applied to the results with ordinary haloids, can be readily extended to the hypothetical chloride of ammonium; and to complete the evidence on this point, I found that a solution of muriate of ammonia yielded the definite quantity of hydrogen from the negative pole.

The experiments with a positive zinc pole lead to the same result, at least when taken in conjunction with those, shewing that the haloids, if viewed as existing as such in solution, are not directly decomposed. The oxide of zinc, which is dissolved and transferred, must have been taken up by acid which had been previously drawn to the positive side.

The analogy of the action with a positive zinc pole in alcoholic solutions of haloid salts, as formerly described, leads, by similar reasoning, to the view that, in moderately strong solutions of that description also, such as those of chloride of lithium, iodide of potassium, and moderately saturated alcoholic solutions of chloride of calcium, the haloid decomposes the water of the alcohol, and exists in solution as an oxisalt. Many of the phenomena of the voltaic action on such solutions will thus receive a more ready explanation than on the idea of these salts being dissolved as haloids; such as the appearance of alkalies and earths at the negative pole, which will thus result directly from the decomposition of a

hydracid salt, instead of supposing the secondary action of hydrogen on the haloid, and reaction of the metal on water.

We cannot easily get the same direct evidence on this subject by the method applied to aqueous solutions, of placing the poles in water beyond the solution, because, from the inferior conducting power of the alcoholic solution, less acid will be separated, if it truly exists, in the liquid; and we cannot distinguish whether it may not come from the point of junction of the alcoholic solution with the water in which the poles are placed. Hence, I believe that, in formerly contrasting the results with alcoholic and with aqueous solutions, the conclusion that no acid was formed in the former solution was incorrect; and I think it more likely that the acid formed was confounded with that from the point of union. It is fortunate that there is no real need for this illustration in proof of the decomposition of water by haloid salts.

If alcohol dissolves haloid salts as hydracid salts, there can be little doubt that pyroxylic spirit does the same: I incline to think that the greater solvent powers of the latter fluid than the former in regard to some substances, such as barytes, is due to its greater absolute quantity of water, although not greater atomic proportion.

V.—*On the Conducting Power of Solutions.*

Without going the length of holding that the additional conducting power bestowed on water by dissolved substances, is exactly proportional to the degree of chemical change, under voltaic action, resulting from the dissolved body, there seems in every instance in which increased conducting power is bestowed, some chemical change, or at least voltaic transference, attending the increase of conduction. This chemical change may result either from the direct action of the current or from secondary agencies; and both circumstances lend their aid, where they occur, in augmenting conducting power.

In the case of salts, the voltaic separation of acid and alkali at once explains the result, and in many of such cases we have an additional effect from secondary actions at one or both poles.

Acids alone in solution, as is now generally known, and as I have myself verified experimentally, for sulphuric acid and the hydracids, undergo transference to the proper pole; which circumstance appears to be the primary cause of their promoting conduction. In some instances secondary actions at the poles also contribute to the result.

To ascertain whether alkalies have a similar action by suffering transference, a moderately strong solution of caustic potash was placed in a tube B connected as in fig. 3* by asbestos, moistened with distilled water, with two tubes, A and

* Edin. Trans. xiv. Plate II.

C, containing distilled water, A being made negative, and C positive, by 72 pairs of 4-inch plates. The whole tubes were covered with a close glass covering, a piece of turmeric paper having been introduced into the liquids A and C, between the asbestos and the poles. In a few minutes alkali was indicated at the negative pole, and went on increasing during half an hour, whilst the test-paper in C was not discoloured, shewing that the effect in A was not due to capillary action. The experiment was then stopped, when the water in A, although not alkaline to test-paper throughout, became decidedly so by concentration, whilst that in C shewed no alkali even after concentration.

In the experiments also, already detailed, in which acid and alkali were separately drawn to the poles in distilled water, from saline solutions, the alkali usually reached the pole as soon as the acid.

There can thus be no doubt that by voltaic action the alkali in an aqueous solution is transferred to the negative pole.

Water coloured by bromine gives sensibly more effervescence under galvanic action than distilled water, shewing a superior conducting power of the solution.

The manner in which such simple substances increase the conducting power of water requires a little investigation. Chlorine, bromine, and iodine, are generally admitted not to be conductors themselves; and, even if a little doubt may exist as to iodine in a state of fusion, it is scarce possible that the minute quantity of it in an aqueous solution can operate in that way.

To ascertain whether such substances are capable of transference in solution, an aqueous solution of bromine, with a little undissolved bromine at the bottom to maintain a state of saturation, was placed in the tube B, the arrangement being in all other respects the same as in the last described experiment, with a solution of potash; and after nearly an hour's action of 72 pairs of 4-inch plates, no discoloration from transference of bromine could be observed in the water either of A or of C, and the latter had only a scarce perceptible smell of bromine, which I believe was due to the secondary decomposition of a trace of hydro-bromic acid, drawn into C, as both the liquids B and C shewed some degree of acid reaction.

An aqueous solution of iodine was then substituted in B for that of bromine, a little iodine being also left at the bottom, and all other circumstances the same, and the battery recharged. After an hour's action there was no appearance of iodine either in A or C.

From these experiments it is obvious that neither of these substances are transferred in solution under voltaic agency. We must, therefore, look for some other explanation of the increased conducting power; and that which readily occurs is a secondary action at the negative pole, by the union of hydrogen with the dissolved substance. To determine the accuracy of this view, the current from 50 pairs of 2-inch plates was passed at the same time through a solution

of bromine and diluted sulphuric acid, and the hydrogen evolved from the two negative poles collected; when, after half an hour's action, 0.13 C I were collected from the sulphuric solution, and only a bubble the size of a pea from the brome solution. The difference had evidently combined with bromine.

When an aqueous solution of iodine, which had been previously purified by sublimation, solution in alcohol, and precipitation by water, was substituted for that of bromine, the action was much more feeble. In a quarter of an hour, only a small bubble of gas was collected from each negative pole; and in two and a quarter hours 0.1 C I from the sulphuric solution, and 0.077 C I from the iodine.

It is thus evident that, both in the case of bromine and iodine, the action is increased by the combination of the dissolved substance with hydrogen of the decomposed water, but that, as was to be expected, this circumstance occurs to a much larger extent in the case of bromine than of iodine.*

On connecting the rules regulating the voltaic decomposition of solutions and the transference of substances held dissolved, we observe that no substance, whilst in a state of transference, suffers direct voltaic decomposition. Acids and alkalies suffer transference, but not direct decomposition. On the other hand, salts, whether oxyacid or hydracid, are not transferred, but are resolved into their constituent acid and alkali.

We cannot, however, say, that every substance which is not transferred is directly decomposed. Thus we can hardly doubt that such combinations as bromide of iodine do not suffer voltaic transference, seeing that their constituent elements, when separate, are not transferred; and we have farther seen that this combination is not directly decomposed in solution. Probably, also, some cases of chlorides exist, in which, from peculiarity of atomic constitution, or other circumstances, there is neither transference nor direct decomposition.

* Long after these experiments were made, and conclusions drawn, I observed that M. Becquerel had also found that bromine and iodine, in solution, unite with hydrogen under Galvanic Agency. L'Institut. Juin, 1840.

Erratum in former Memoir, vol. xiv. p. 133, line 2, for *latter* read *former*.

X.—*On the Preparation of Paracyanogen in large quantities, and on the Isomerism of Cyanogen and Paracyanogen.* By Samuel M. Brown, M.D. Communicated by Robert Christison, M.D., F.R.S.E., &c.

(Read 15th February 1841.)

The design of the processes described in this memoir was to decompose the bicyanuret of mercury at such a temperature, and under such a degree of pressure, as to secure the simultaneous extrication of the two equivalents of cyanogen, or their elements, in the expectation that they should come off united, and produce the interesting compound of nitrogen and carbon, isomeric with cyanogen, *Paracyanogen:* And that result was sought in the belief that it would illustrate the chemical theorem of the existence of bodies which, though composed of the same elements in the same proportions, yet differ as widely from each other in chemical properties and mechanical conditions, as one element differs from another.

I. *History of Paracyanogen.*—M. Gay-Lussac* observed, in the course of his admirable researches on the prussic acid, that it is spontaneously decomposed on exposure to light, ammonia being liberated and a brown solid matter deposited. From experiments made *in vacuo* it appeared that these were the sole products of the reaction of the elements of the acid on each other, so that the deposit necessarily contained nitrogen, and, without analysis, it was inferred to be "un azoture de carbone."

When M. Gay-Lussac proceeded to the discovery of cyanogen and the composition of the cyanurets, he procured cyanogen from the prussiate of mercury, which he represented as a true bicyanuret. Among other processes, he had recourse to the decomposition of the mercurial cyanuret by the oxide of copper, in order to determine the quantitative composition of cyanogen. The results of this analysis coincided with those of his other methods, and confirmed his view of the nature of the so-called prussiate; but there was one circumstance which, at first sight, seemed to throw suspicion on these conclusions. "Cependant, s'il en est ainsi, pourquoi reste-t-il une matière charbonneuse lorsqu' on décompose le cyanure par la chaleur? Cette difficulté m'embarrassé pendant quelque tems; mais je crois être parvenu à la résoudre." He then states that he found, that, when the carbonaceous matter, now known by the name of paracyanogen, is left in the retort after the reduction of the cyanuret by heat, nitrogen appears in the gaseous product in such a proportion as very nearly to make up the equivalent weight of

* Ann. de Ch. 1815.

cyanogen with the nitrogen and carbon of the solid residue; and from this observation it was concluded that the residue in question was a carburet of nitrogen, containing less nitrogen than cyanogen.

In A.D. 1829, Professor JOHNSTONE published some interesting analyses of this substance, from which it appeared to contain nitrogen and carbon in the very same ratio as cyanogen itself. His results were received with some distrust; but only on account of the singularity of the inference to which they conducted, isomeric bodies being then comparatively unknown. Some analyses of LIEBIG's did not at first bring out exactly the same proportions as those of Mr JOHNSTONE, but in the summer of A.D. 1835, they examined the subject together in the presence of Dr GREGORY, and their results accorded with the former observations of the British chemist, who subsequently published an elaborate memoir on paracyanogen in the Transactions of the Royal Society of Edinburgh for 1838. That memoir contains satisfactory and numerous analyses of the carbonaceous matter under consideration, all of which tend to establish the proposition that it is isomeric with cyanogen, and that, consequently, the volume of cyanogen produced from cyanuret of mercury by heat is less, exactly in proportion as the quantity of paracyanogen left in the retort is greater. The analyses were made by decomposing paracyanogen by means of oxide of copper and bichromate of potash, collecting the gaseous products, removing the carbonic acid from different volumes of the mixture, and finding the proportional volumes of nitrogen; for example, with the former reagent a mixed product was obtained, of which

90.0	vols. left	32.6	v. nitrogen,
92.9	...	30.1	...
293.0	...	98.0	...

and with the latter,

94.5	vols. left	33.5	v. nitrogen,
129.0	...	43.0	...
108.0	...	36.0	...
175.0	...	58.5	...

These ratios, taken in connection with the ascertained composition of cyanuret of mercury, prove that the subject of analysis is composed of nitrogen and carbon in the proportion of 1 : 2. The composition of gaseous cyanogen is $N+C^2$.

II. *Properties of Paracyanogen.*—We are indebted to Professor JOHNSTONE for all that has been published about the properties of paracyanogen. Prepared by heat from the cyanuret of mercury, it is a brown solid, more or less dark-coloured and dense according as it is procured at higher or lower temperatures, varying in these respects from the condition of a loose, nut-brown, hygrometric powder, to that of a compact, black scoria. It cannot, however, be made to assume the latter form without the loss of some of its nitrogen; the more suddenly it is raised

to a white heat, and the shorter the time it is kept at that temperature, the less nitrogen is lost. The change which it suffers from elevation of temperature resembles that which the same process produces on carbon, boron, and silicon; after ignition it is very difficult of decomposition, and indisposed to enter into combination. It is soluble in cold concentrated sulphuric acid, and yields a deep-brown, semi-transparent solution, from which I have found that it gradually falls, unchanged and anhydrous, on prolonged exposure to the moisture of the atmosphere. This is the only way in which pure paracyanogen can be prepared, for, taken as it occurs in the retorts, it is saturated with cyanogen, which adheres to it with great obstinacy; and I have never seen a specimen which did not leave an evident, though inappreciable, residue after the action of sulphuric acid, as might have been expected from GAY-LUSSAC's observation that, when paracyanogen remained in his retorts, he always found traces of nitrogen in the cyanogen collected. It likewise dissolves in nitric and hydrochloric acids, but to a much smaller extent. It is insoluble in water, alcohol, ether, oils. When the sulphuric acid solution is poured into water, there falls a bulky hydrate of paracyanogen. This hydrate is produced in several humid reactions, such as that of cyanogen dissolved in alcohol and exposed to the influence of light; but it is unnecessary to refer to them, as it is with the production of anhydrous paracyanogen by fire that the present inquiry has to do. These proximate characters are exceedingly well marked, and are quite sufficient for the discrimination of the substance to which they belong.

In the memoir of A. D. 1838, Mr JOHNSTONE states that paracyanogen is slowly resolved by heat into cyanogen, but without alluding to the experiments which led to this conclusion. As I had not only never succeeded in producing such an effect upon the pure anhydrous substance, prepared by the process which has been indicated above, but had invariably obtained results directly negative of his proposition, viz. the extrication of unmixed nitrogen, I requested Mr JOHNSTONE to inform me of the manner in which his experiments had been performed. That chemist kindly gave me to understand that he had never effected the total resolution of the solid into the gaseous isomeric, and that the process by which he had produced the partial resolution, consisted in keeping paracyanogen (procured as the residue of the preparation of cyanogen) at a low red, followed by a higher heat, for some time, and collecting the gaseous product, which came away less and less rapidly as the operation was continued. Now, I had separated cyanogen in this way from common paracyanogen, *but always mixed with more or less nitrogen*, less at the beginning and more towards the end of the process, till there was extricated nitrogen alone; and had inferred that the cyanogen had been retained in the paracyanogen, which yielded it, by its absorptive power. This inference was suggested and corroborated by the considerations that paracyanogen is peculiarly fitted, by its mechanical form, for retaining gases; that its chemical relation to cyanogen renders it particularly adapted to the absorption of that gas, and that

cyanogen is presented to it in the most favourable condition for retention in a process of which they are simultaneous products; and the following selection of simple experiments ratifies the suggestion.

A quantity of paracyanogen was heated, the first product was rejected till the air of the little apparatus, and any gaseous matters which the subject of experiment might have absorbed from the atmosphere, were expelled, and then 32, 18, and 28.5 volumes were successively collected and examined; the first was found, after the removal of cyanogen by potassa, to contain 13.3 vol. or 41.5 per cent., the second 14 vol. or 77.7 per cent., and the third 26 vol. or 92.8 per cent. of nitrogen; the volume of nitrogen being greater, and of cyanogen less, as the operation was prolonged. The paracyanogen, which yielded these mixed volumes, was then found to give off nothing but nitrogen.

Again, a parcel of the same paracyanogen was kept a week in a vessel full of chlorine, by the copious absorption of which it was changed in appearance, having assumed a very light brown colour. One part of this product, treated as in the former case, first gave away chlorocyanic acid alone at a temperature somewhat lower than that of red heat, and then, at a more exalted temperature, unmingled nitrogen. Another portion was boiled several hours in water with a little carbonate of potass, filtered, dried at 212°, and decomposed by heat; the first 30.1 volumes of the product suffered a diminution of only 0.1 vol. by the action of potassa; and the subsequent volumes suffered none.

These observations confirm the rationale which I have given of the appearance of cyanogen at the beginning of the decomposition of paracyanogen procured in the ordinary way; especially when they are viewed in connection with GAY-LUSSAC's discovery of traces of nitrogen in cyanogen produced from bicyanuret of mercury at high temperatures. And the fact that *pure* paracyanogen (precipitated by the atmospheric moisture from the sulphuric acid solution of the common product) does not afford the slightest appearance of cyanogen, warrants the conclusion that paracyanogen, once formed from cyanogen (or its elements), cannot be rechanged into cyanogen by heat. Indeed, it would have been anomalous if it had been otherwise; for it will be found in the sequel, that the higher the temperature at which bicyanuret of mercury is decomposed, the greater is the quantity of paracyanogen produced; and how should the same cause which converts the cyanogen of the mercurial salt into paracyanogen transform the latter into its gaseous isomeric again?

III. *Experiments upon the Preparation of Paracyanogen.*—The following experiments were made with bicyanuret of mercury, formed by the reaction of hydrocyanic acid, prepared by GEIGER's process, on the peroxide of mercury, and subsequently ascertained to be pure by a humid analysis.

They are presented in the form of a list selected from many others, so as both to exhibit the process of observation which led to the last of them, and illustrate the conclusion deducible from the whole series, viz. that the greater the pressure, up to a certain degree, under which the haloid is decomposed, the greater the quantity of paracyanogen produced. Accordingly, although the two last may appear to supersede the rest, they would in reality be inconclusive as to the influence of gradually increased pressure without them. They are all described with what may seem to be unnecessary minuteness, partly because they are the first recorded experiments of their kind, and partly because it will be necessary to make particular references to them in future communications.

1. A quantity of bicyanuret was thrown on an iron plate, previously heated to a temperature much higher than the point of decomposition of the salt, yet considerably short of that at which paracyanogen enters into combustion. Decomposition instantly ensued, and there remained a residue of nearly half the bulk of the cyanuret employed, which was found to be paracyanogen by the appropriate tests. This was repeated several times at different temperatures, within the same range, and with the evident result, that the speedier the decomposition, *i. e.* the higher the temperature at which it was effected, the greater the bulk of the solid nitrocarbon product. This rude experiment led to the next, in which the pressure of cyanogen passing through a capillary tube was *partly* substituted for quick decomposition.

2. Some cyanuret was introduced into a tube of German glass, sealed at one end, which was then drawn out from two inches above the surface of the contents into a very fine capillary, a foot and a half in length. The containing extremity was suspended in a large spirit-lamp flame till the decomposition was completed. During the operation, the opening of the capillary was carefully watched for cyanogen by a lighted taper, but no combustion or other evidence of gaseous escape ever appeared, the great pressure given by the long capillary having prevented freedom of passage. An apparently full proportion of mercury had sublimed into the upper two inches of the wide part of the tube; and there remained below a quantity of brown matter, occupying nearly the same space as the original cyanuret. The residue in this case consisted of paracyanogen, cyanogen mechanically retained, and a few little globules of mercury, but not a trace of unreduced cyanuret. This trial was also repeated several times, and always gave similar results: when the drawing of the tube was less capillary there took place escape of cyanogen, and less paracyanogen was left. These two tentative experiments conducted to the following attempts to obtain numerical results.

3. A strong test-tube, weighing 117.1 grs., was charged with 14.9 grs. of cyanuret, equal to 132 grs. It was drawn out at two inches from the bottom to about five inches, and an inch above this to a foot in length, neither of the drawings being quite so fine as in the second experiment. Heat was now applied,

quickly raising the cyanuret to a full red; mercury sublimed, some cyanogen was observed to escape, and after the operation the apparatus was sealed down by the blowpipe into small pieces, which, together, weighed 130.85 grs., indicating a loss of 1.15 gr.; whereas, if the products had been mercury and cyanogen, the loss would have been 3.1 grs. The paracyanogen remaining in the tube weighed 2.3 grs., and, with the exception of retained cyanogen, was pure. Thus, 2.3 grs. of the cyanogen of 14.9 grs. of cyanuret, appear to have been transformed into the solid isomeric; but 2.3 grs. + 1.15 gr. is a slightly greater weight than that of all the cyanogen contained in the weight of salt decomposed; an error which is to be accounted for by the hygrometric property of paracyanogen, and the small but appreciable loss sustained by glass-tubes when much drawn out.

16.4 grs. were treated exactly as in this experiment, the drawing having been made more capillary; and there was *no loss*, but the product contained slight traces of undecomposed cyanuret, so that the value of the result was diminished.

4. A quantity of cyanuret, weighing 10.81 grs., was put into a strong test-tube, three inches long, which was then bent to an obtuse angle in the middle, and six inches of thermometer tube, of so fine a bore that quicksilver neither rose in it spontaneously, nor could be forced up by suction, was tightly sealed into the open extremity. The containing part was held in a large spirit-flame, so as to keep the rest of the apparatus out of the heated current; and at the end of the process there was found to be a loss of 1.5 gr. The mercury had sublimed past the bend, at which the tube was then broken across. The upper part, with the mercury, weighed 124.8 grs., and lost 7.8 grs. by the removal of its contents; while the lower, containing paracyanogen, weighed 43.4 grs., and, on being emptied and cleaned out by means of black oxide of copper, lost 1.5 gr. Now, 2.2 grs. = cyanogen of 10.81 grs. of bicyanuret of mercury; so that all the cyanogen but 0.7 gr. was, in this case, produced as paracyanogen. But 7.8 grs. of mercury implies a loss of 0.8 gr., to be accounted for partly by experimental error, and partly by the first loss of 1.5 gr. during the process; for 1.5 gr. first loss, + 1.5 gr. paracyanogen, + 7.8 grs. mercury, = 10.8,—a weight short by only 0.01 gr. of that of the salt submitted to decomposition.

5. Eight inches of the strongest green glass-tube, one-third of an inch in diameter, sealed at one end, and bent to an angle of about 130° between the third and fourth inches from the bottom, having been tared, some well-dried crystals of bicyanuret of mercury were introduced, and found to weigh 10.6 grs. The open end was hermetically sealed. The containing limb of this little shut retort was put into a jacket-tube, and surrounded by fine sand within the jacket. It was suspended half an hour in a very large spirit-flame, at the end of which time complete decomposition had been effected, except that a *very* small quantity had been carried up with the mercurial vapour. After the reduction of the traces in the bend, the apparatus was found to have gained 0.05 gr. from the sand, or otherwise. It was filed across at the knee over a sheet of clean paper; a slight explosion

took place, and the odour of cyanogen was evident. Cyanogen having been diffused away, and replaced by air, the two pieces, with their contents, weighed 390.5 grs., 1.7 gr. having been lost by explosion and diffusion; mercury was sublimed away from both, and they weighed 383.1 grs., giving 7.4 grs. for mercury. Paracyanogen was now removed, and the tubes cleaned with oxide of copper, when they weighed 381.7 grs., giving 1.4 gr. for paracyanogen. These three weights of cyanogen, mercury, and paracyanogen, make up 10.55 grs., less by only 0.05 gr. than that of the original compound; and the first (1.7 gr.) may be so divided between cyanogen and mercury as exactly to complete their ratios. In this experiment, 1.4 gr. of cyanogen was changed into its solid form.

6. 16.25 grs. of the salt were put into a green glass-tube, three inches long, and weighing 116.55 grs., the gross being 132.8 grs. The mouth of the tube was plugged half an inch down with stucco-paste; the whole was imbedded in a stucco-mould, and immersed in a sand-bath, which was kept near the low red heat for more than an hour. Freed from the mould and carefully cleaned, it was examined: 0.9 gr. of mercury was procured from the plug; 13.1 grs. of brown product mixed with mercury taken from the tube, and 0.4 gr. of incrusted paracyanogen removed by oxide of copper, so that there had taken place a loss of 1.85 gr.; and this experiment shews that, even under the pressure of a stucco-plug, more than half an equivalent of cyanogen is given off in the form of paracyanogen. This modification was tried thrice with analogous results; at least half of the cyanogen having been transformed in every case.

7. 50 grs. were closely packed in a small iron bottle fitted with a screw-stopper, which, though tight, had been observed, in a previous experiment, to admit of the passage of mercurial vapour under high pressure. Having been weighed, it was enclosed in a shut crucible, and kept more than an hour at a low red heat, after which it was found that the screw had allowed the expected exit, and that the total loss was only 1.0 gr. greater than the weight of the mercury. The fixed residue was good paracyanogen, containing no traces of either mercury or undecomposed salt, but having lost some of its nitrogen. The loss of nitrogen was discovered by the action of concentrated sulphuric acid. When it is taken into consideration that the 1 gr. of total loss must be at least *partly* attributed to the extrication of nitrogen from produced paracyanogen, the transformation effected by this experiment appears to be very complete.

8. 32.15 grs. were crushed into a strong test-tube, which was sealed close to the surface of the cyanuret, and immersed three hours in a boiling linseed-oil bath. It seemed from without to be thoroughly decomposed, the characteristic brown powder, with globules of mercury interspersed, having replaced the white needles. Having been first weighed, it was filed across over a sheet of paper; a trifling explosion took place; the scattered product and the pieces of the tube, weighed together, indicated a loss by explosion of 0.5 gr. Thus the pressure of contained air and 0.5 gr. of cyanogen, at the boiling heat of

oil (600°), had hindered the further extrication of cyanogen during three hours. However, all the remainder of the cyanogen had not been separated from the mercury as paracyanogen; the solid product was a mixture of that principle, mercury and bicyanuret, only two-thirds of the last having been decomposed. This oil-bath experiment was repeated several times with a view to finishing the reduction by prolonging the operation, but in eight trials the packed tubes burst in about nine hours; and when removed before that time the cyanuret was never found to have been completely decomposed. In one case, for example, 130 grs. gave two-thirds of its nitro-carbon product as paracyanogen, while the cyanogen set free, as indicated by the loss after explosion, was only 0.9 gr. In this form of the process the mercury is so intimately mixed up with the paracyanogen as not to be visible till it be ground under water; they may be separated either by levigation, which is a very tedious process, or by rubbing the mixture gently in a leaden mortar, which absorbs the mercury. It is rather difficult to separate paracyanogen entirely from the surface of the lead, on account of its adhesiveness; and for the purposes of analysis it is necessary to weigh the mixture, to weigh the little mortar before and after the trituration, and to subtract the first from the second weight of the mortar for the mercury, and the weight of mercury from that of the mixed product for the paracyanogen.

9. In order to discover the degree of pressure under which the gaseous product of these experiments is separated from the mercury in the solid form, I constructed a sort of differential barometer on the principle of the law of aërial elas-

Fig. 1.

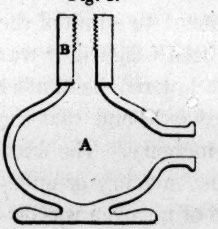


Fig. 2.

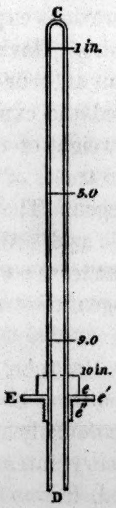


Fig. 4.

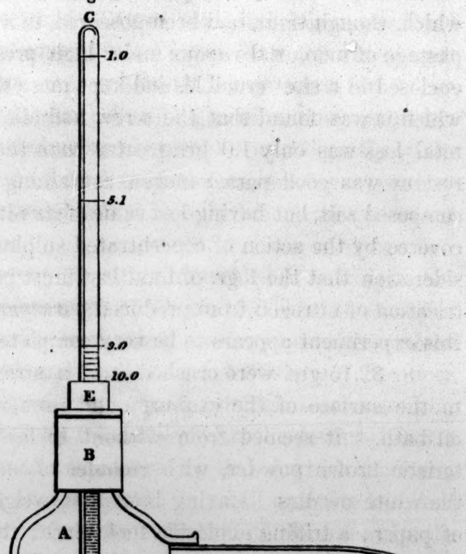
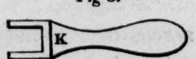


Fig 3.



ticity, commonly known as the law of BOYLE and MARIOTTE. A (fig. 1) is a very thick and strong glass-vessel, somewhat of the shape and size of a common spirit-lamp, with a strong tubular and well ground opening in one side. B is a brass collar, an inch and a half in length, fitted closely round the neck of A with brazier's cement; the upper half of the interior of B is a female screw. C D (fig. 2) is a barometer tube of the greatest strength which is made, thirteen inches long; the upper ten inches are graduated from above downwards, every inch being divided into tenths; at the tenth inch is fitted on the immoveable brass collar E, the part *e* being cubic, and fitted with a detached crane-lever (fig. 3), *e'* being a round disc fitting tightly down on the top of B, and *e''* being a male screw fitting that of B. F (fig. 4) represents a thick glass tube-retort, the beak of which is nicely ground into the tubular opening of A, fig. 1.

The method of using this instrument is exemplified in the details of the experiment. C D was filled with quicksilver, inverted and screwed at *e''* into the collar B, a well cut and greased leather disc having been interposed between the top of B and the under surface of *e'*; it was fastened with the aid of the crane-lever K. Quicksilver had been previously introduced into A to the height of the tubular opening, so that the open end of C D was now submerged. The apparatus was thrown on its side and shaken gently till the quicksilver fell in C D to the ninth inch. The tube-retort was charged with bicyanuret and adapted to the tubular opening with an air-tight cement, the junction being likewise thickly luted with a paste of gypsum and gum-arabic, which sets very hard. Having remained two days in this condition, the retort was put in a jacket-tube, and surrounded by a large spirit-flame till the reduction appeared to be completed. In the mean time the quicksilver in the meter-tube was watched; it steadily ascended with a velocity manifestly diminishing with the increase of the height, till in twenty minutes it reached 5.1 in. where it stopped. The flame was now withdrawn, equilibrium of temperature restored, and the quicksilver fell to 8.1 in.; from which it was inferred that the quantity of cyanogen which had been liberated was measured by the ascent of 0.9 in.; while the maximum pressure under which the decomposition had been effected was measured by the ascent of 3.9 in. The apparatus was taken down, and it was found that the reduction had been finished, so that cyanogen is given off from bicyanuret of mercury as paracyanogen, when decomposed under a pressure bearing the same proportion to that of one atmosphere, as 9 in. of the meter-tube to 5.1 in., *i. e.* according to the law of elasticity, a pressure of 1.76 atmospheres. The phenomenon may, for all that this and similar experiments can determine, take place at lower degrees of tension, but I do not know how the minimum may be estimated.

IV. *Practical Observations.*—1. Such are the experiments which have been made on the transformation of cyanogen by fire. As processes, they contain

two elements of action, high temperature and great pressure; the former being the sole principle of experiment 1, the latter of experiment 8, and both being concerned in the other seven. It is impossible to change a whole equivalent of cyanogen by this kind of procedure, for no apparatus can be so filled with salt as to leave no space for extricated gas, and, as has been several times observed already, paracyanogen absorbs it largely. Whatever may be the value of the foregoing results as the first-fruits of a new field of chemical investigation, viz. the decomposition of compound bodies under greater than atmospheric pressure, and at temperatures higher than their natural points of reduction, they at least supply the desideratum of a simple process for the large preparation of a curious substance, which eminently deserves to be studied, but has hitherto been procured only in small residual quantities. *Process.*—Let any quantity of cyanuret be tightly packed into a tube of cast copper, eight inches long and two-thirds of an inch in diameter, shut at one end and open at the other; let the mouth be plugged an inch down with stucco-paste, and a little fire-clay put over the stucco; and, after the tube has been luted and dried, let it be kept half an hour at an obscure red heat. The greater part of the nitro-carbon product remains in the form of paracyanogen. If it contain unreduced cyanuret it must be returned, if it contain mercury it may be cleaned in a leaden mortar, and it may be rendered perfectly pure by solution in concentrated sulphuric acid. By this and similar processes I have made paracyanogen by the drachm instead of the grain, having always procured about two-thirds of the whole weight of nitrogen and carbon. The mercury may be collected by adapting a conducting tube and receiver to the copper-tube and plug. This process may be variously modified. I employ two copper-tubes, one of which terminates at its open end in a male screw, and the other in a female. The latter, thrice as long as the former, is charged, and the two are firmly screwed together so as to give very great, but not fixed, pressure, for the screws always permit gaseous passage in such circumstances.

V. *The Constitution of Paracyanogen.*—It is isomeric with cyanogen. Analysis thus suggests two possible arrangements of its components: It may be regarded as a compound of two combined atoms of nitrogen and four combined atoms of carbon, and represented by the symbol $N_2 + C_4$; or, it may be viewed as a combination of two atoms of cyanogen, with the symbol Cy + Cy, or Cy_2. These are the only schemes of constitution which can be formed, without supposing radicals which are not known to exist, and modes of combination of which there is no known example. Both of them assume the combination of "equal and similar"*

* I have borrowed the phrase "equal and similar" from geometry, because several atoms are equal which are not similar. It implies identity both of atomic weight and of true isomorphism, both of force and of form.

atoms; the former of two nitrogens with one another, and four carbons; the latter of two cyanogens. The former, however, supposes a combination of two nitrogens, which is decomposed by the decomposition of its compound with the carbon; for, by whatever process paracyanogen be decomposed, nitrogen is liberated. Now, it is not easy to conceive of a compound of two atoms of nitrogen, *once combined*, being resolved into its constituents by (for example) elevation of temperature; and this reflection, simple as it is, applies to every case in which chemists have hitherto represented two equal and similar atoms as combined. If there be any analogy between the union of equal and similar atoms, and that of two unequal and dissimilar atoms, it should be broken up only by analogous forces. Now, heat decomposes a compound of the latter kind by driving away the more volatile, or liquefying the more fusible element; and if the true symbol of paracyanogen were $N_2 + C_4$, heat should extract from it neither nitrogen nor carbon, the elements of a compound of two nitrogens being equally volatile, and those of a compound of four carbons equally infusible. For these reasons I set aside the ordinary doctrine of the constitution of paracyanogen, and adopt the alternative, which is both simpler and more consistent with the results which have been described above. Two atoms of cyanogen are, by an artifice, separated simultaneously from one of mercury; they come away as paracyanogen—a new form; and the most direct inference is, that the product is a compound of one cyanogen, as such, with another. It has been shewn that the solid isomeric cannot be resolved by heat into two equivalents of cyanogen, which is the best and only possible confirmation of the conclusion that it is a true cyanide of cyanogen, decomposed neither by heat, because its constituents are equally volatile, nor by electrolysis and reagents, because it is a perfectly neutralized combination. Heat and reagents decompose, not paracyanogen, but its *constituents*, producing from each one of nitrogen and two of carbon, and making up, for an equivalent of the whole, two of nitrogen and four of carbon. According to the same principle, the symbol of cyanogen is not $N + C_2$, but $NC + C$. But it is unnecessary to generalize the proposition which has been laid down, as the method of doing so is very simple;* suffice it, that if it be admitted, it affects the whole doctrine of chemical constitution, and strikes at the root of a hypothetical inference regarding affinity, which has been promulgated by syste-

* For example, the carbo-hydrogenous series of isomeric bodies would be represented thus:—

Methylene, . .	$(C_2 H_2)$ = Me ; .	=	1
Olefiant gas, . .	$(C_4 H_4)$ = Me + Me ;	=	2
Oil gas, . .	$(C_8 H_8)$ = Olft. + Olft. ;	=	4
X, an unknown form,	$(C_{16} H_{16})$ = Olg. + Olg. ;	=	8
Cetene, . .	$(C_{32} H_{32})$ = X + X ;	=	16

Naphthaline, . . . = Na. ; eq. 1.
Paranaphthaline, . . . = Na. + Na. ; eq. 2. ; and so on.

matic writers ever since the term was defined by BOERHAAVE, and is now calculated to impede the progress of discovery, viz. that the attraction of affinity between dissimilar atoms is identical with, or analogous to, the attraction of cohesion between similar atoms.

In conclusion, this view of isomerism, and the relation of cyanogen to paracyanogen, is further recommended by the consideration, that it affords a practical foundation for a likely hypothesis of the constitution of the so-called chemical elements, and points out the way in which such a hypothesis may be either established, or overthrown by experimental observation. Let it be supposed that several of the elemental groups are so many series of isomeric forms, and it is at once to be inferred that heat, electrolysis, and reagents, shall all be incapable of decomposing them, as has been found in the actual practice of the laboratories of modern Europe by innumerable trials. If, to take one instance, sulphur (16 or 2) be an isomeric form of oxygen (8 or 1) which it as much resembles in chemical properties as it is conformably contrasted with it in mechanical condition, it must be impossible to extract oxygen from it by any analytical force which has yet been discovered; and the only method in which it shall be possible to prove that such is the mutual relation of these two elements shall be to have recourse to synthesis, and convert oxygen into sulphur. It is within the scope of this hypothesis that the various elements may all be isomeric forms of one truly elementary substance; but it would be out of place to indulge in such speculations at present. I should not, indeed, have alluded to the general conclusions regarding the constitution of paracyanogen, and the isomerism of elementary bodies, deducible from the preceding inquiries, if I had not succeeded, by the farther application of the same method of experimental investigation, in obtaining results which, if there be no fallacy in the facts brought under my notice,—and I have not hitherto been able to detect any,—seem to establish the substantial identity or isomerism of two familiar bodies hitherto supposed to be elementary.

XI.—*On the supposed Progress of Human Society from Savage to Civilized Life, as connected with the Domestication of Animals and the Cultivation of the Cerealia.* By JOHN STARK, Esq. F.R.S.E. &c.

(Read 1st March 1841.)

I.—SUPPOSED PROGRESS OF HUMAN SOCIETY.

IT is a general belief that Man, in his supposed progress from Savage to Civilized Life, has passed through three distinct stages or periods, each one leading a step forward in the road to social improvement. These stages are asserted to be, 1. *The Hunter State;* 2. *The Pastoral State;* and, 3. *The Agricultural State.* Allusions to these different stages crowd the pages of the historian, the philosopher, and the poet; and arguments are founded on, and deductions drawn from, these states of existence, as if they were ultimate truths, neither to be discussed nor dissented from. It is the object of this paper to question the existence of these separate states, their necessary connection with one another, and the end to which ultimately they are supposed to lead.

Among the earliest writers who treat of the first ages of the world is HESIOD, the Grecian poet, a contemporary of HOMER, who lived about a thousand years before the present era. His story of PROMETHEUS and PANDORA, as well as his detail of five different periods which had preceded the time when he wrote, are evidently taken from some of the floating traditions of the early history of the world met with in all countries. The first period is termed the *Golden Age*, referrible, it is supposed, to the state of man in Paradise; the second is entitled the *Silver Age*, which may allude to the antediluvian period; the third is the *Brazen Age*, which, from some of its characteristics, would seem to refer to the state subsequently termed the *Hunter's State;* the fourth is the *Age of Heroes;* and the fifth the *Iron Age*, or Agricultural period.*

LUCRETIUS, the Roman poet and philosopher, follows in describing the early stage of man's existence as little better than that of the beasts. But afterwards, according to the same authority, men built huts, clothed themselves with skins, and learned the use of fire; the power of forming sounds and language followed; and, finally, towns were erected and governments established.†

* Works and Days, Book I. translated from the Greek by THOMAS COOKE.

† T. LUCRETIUS CARUS, De Natura Rerum, lib. v.

OVID follows HESIOD in his division of the Ages, but makes them in number only four;* and HORACE, in a well known passage, records the general ideas prevalent as to man's degraded origin.†

Such were the opinions of the ancients as to the early state of human society,—opinions which have influenced the details of almost all subsequent writers.

Dr FABER, it may be remarked, considers that the mythology of the ancients recognises two golden ages; the first coinciding with the Creation, the second coinciding with the period which immediately succeeded the Deluge. The modern theories, founded on these statements, are such as I shall now shortly notice.

"In temperate climates," says Lord KAMES, "the original food of man was fruits that grow without culture, and the flesh of land animals procured by *hunting*. A fawn, a kid, or a lamb, taken alive, and tamed for amusement, suggested probably flocks and herds, and introduced the *shepherd state*."—"Necessity, the mother of invention, suggested *agriculture*. When corn, growing spontaneously, was rendered scarce by consumption, it was an obvious thought to propagate it by art."‡

Many other writers of celebrity, among whom may be mentioned Principal ROBERTSON, Baron CUVIER, and Sir HUMPHRY DAVY,§ have adopted this theoretical opinion in regard to the progression of the human race from savage habits to civilized life.|| "In the earliest ages," says M. VIREY, "human societies were scattered over the surface of the globe, living on the fruits of the chase, fishing, and on the wild herbs which a beneficent Nature made grow under their feet. The increase of the numbers of individuals upon a soil which the plough had not yet fertilized, the scarcity of game, the difficulty of subsisting in severe seasons, forced men to rear cattle to feed upon in these necessities, and they became shepherds. Supported, then, on the milk of their cattle, and clothed with their fleeces, their manners became polished, and their minds accustomed to contemplate nature. But even the resources of the pastoral life, in spite of the dispersion of families and nations to new lands, became too limited for the increase of the human race, and the earth began to be appropriated, and the labour of the ox to be applied to the cultivation of the soil. To this succeeded the settlement of men in cities and towns, the rise of the arts, and the division of employments which characterize civilized life."**

The author of the "Wealth of Nations," and his latest illustrator Mr M'CUL-

* Metamorph. lib. i.

† Sat. iii. v. 199, &c.

‡ Sketches of the History of Man, i. 46, 47.

§ Consolations in Travel, p. 76.

|| The traditions of the Chinese, separated as they are, in many respects, from every other people, correspond, according to Dr THOMAS YOUNG, with the classical theory of man's savage original and progressive civilization. Dr YOUNG himself, like most other philosophers, takes the truth of the theory for granted. Supp. Encyclop. Brit. Art. CHINA.

** Nouv. Dict. de l'Histoire Naturelle, tome xv. Art. L'HOMME.

LOCH, Mr MALTHUS, Mr SADLER, and every other writer on the progress of society, adopt the same theory; and even Mr ALISON, in his work on Population, published within these few months (1840), argues as if these were fixed and marked periods in the progressive civilization of man. In fact, though founded in fable, or the dreams of ancient poets, and though such a gradation has never yet been pointed out as existing by those who take its existence for granted, yet such is the influence of classical associations on the spread of knowledge, that, till lately, this series of advancing links in the scale of human improvement has never been challenged.

In drawing up a few popular lectures for a local scientific association, my attention was first called to the improbability of such a series of progressive changes; and the result of my inquiries was, that such a progress is not supported by the evidence of recorded observation, and is opposed to the statements of the oldest written records of the human race.

On further investigating the subject, I found that the theory of human society, in its earliest stage, being originally savage, had been called in question by several writers. Soon after the publication of Lord KAMES's Sketches of the History of Man, the doctrine which his Lordship maintained, in common with many others, as to man's savage original, was animadverted on in a Letter addressed to him by Dr DAVID DOIG of Stirling, which, with a second Letter on the same subject, which personal communication rendered unnecessary to be transmitted, was afterwards published in 1792. Dr DOIG supports the propositions, that the more populous and extensive kingdoms and societies were civilized at a period prior to the records of history; that degenerated races or savage tribes can never recover their pristine condition without foreign aid; that there seems to be in human nature an innate propensity to degeneracy; and that if all mankind had been once in the savage state, they not only never could have arrived at any considerable degree of civilization, but would have sunk lower and lower, till degraded to the level of the beasts that perish.*

The next writer I have met with who questions man's savage original, is Mr JOHN BIRD SUMNER, in a work entitled "A Treatise on the Records of Creation," which was published in 1816 in two volumes 8vo. "The barbarous state of the inhabitants of countries newly discovered," says he, "their general ignorance of arts and deficiency of morals, has naturally introduced a vague idea that man was originally, at his birth or creation, a savage. But according to the Mosaic account,

* For the knowledge and perusal of this scarce and, I believe, little known book, I am indebted to the kindness of JOHN GORDON, Esq. of Cairnbulg. The first Letter is dated 20th December 1774, the second March 12. 1775; but they were not published till 1792. The title of the volume is, "Two Letters on the Savage State, addressed to the late Lord KAIMS." The advertisement, detailing the author's purpose in writing and afterwards publishing them, is written by the Rev. Mr GLEIG, afterwards a bishop of the Scottish Episcopal Church.

which agrees, too, with the suggestions of reason, the savage state was not the primitive state of man."*

In 1830, Dr ROBERT HAMILTON of Aberdeen, in a posthumous "Essay on the Progress of Society," followed Mr SUMNER in asserting that man was not, in his earliest state, an ignorant savage. "Revelation does not favour this opinion," says he; "history does not prove it; the fables of the poets are unworthy of credit; and the reasonings which have been adduced in support of it are extremely conjectural."† But Dr HAMILTON, except in this passage, does not allude to the subject further, and the remainder of his Essay is occupied with other topics.

The theory of the progress of human society from savage to civilized life was afterwards questioned by the present Archbishop of Dublin, who states "the impossibility of men's emerging unaided from a completely savage state; and, consequently, the descent of such as are in that state (supposing mankind to have sprung from a single pair) from ancestors less barbarous, and from whom they have degenerated. The first race of mankind seem to have been placed merely in such a state as might enable and incite them to commence and continue a course of advancement."‡

The theory of savage original was also attacked in 1834 by Mr CHARLES TILSTONE BEKE, in a work entitled "Origines Biblicæ, or Researches in Primeval History," of which only one volume has been published.§ Mr BEKE, however, does not go so far back as the original creation of man for proofs of the earliest state of civilization, but takes his stand on the fact, that "the present human race has sprung, not from a common ancestor in a primitive state of society, but from one who was himself a member of a previous social state, which had already existed for many ages; that whatever may have been the natural state of the first man ADAM, the progenitor of the antediluvian world, the contemplation of that state cannot aid us in the consideration of the primary condition of the postdiluvian world, which takes its origin from NOAH and the seven other persons saved in the Ark, who were members of an artificial, and most probably a highly advanced state of society." ||

Within these few months (October 1840), another work has appeared, entitled "The Natural History of Society in the Barbarous and Civilized State; an Essay towards discovering the Origin and Course of Human Improvement. By W. COOKE TAYLOR, Esq. LL.D. of Trinity College, Dublin."** In this work, Dr

* Records of Creation, i. 47.

† The Progress of Society. By the late Rev. ROBERT HAMILTON, LL.D. F.R.S. Lond. 1830.

‡ Introductory Lectures on Political Economy, p. 129. Lond. 1831.

§ Origines Biblicæ, or Researches in Primeval History. By CHARLES TILSTONE BEKE. Lond. 1834, 8vo.

|| Ibid. i. 49.

** In two volumes 8vo. London, 1840.

TAYLOR maintains that civilization is natural to man; that barbarism is not a state of nature; and that there is no *prima facie* evidence for assuming it to be the original condition of man.* Far, however, from regarding with mortification the circumstance that I have been anticipated in some of my views on this subject by the appearance of this work, I consider such a coincidence as a proof of the general truth of the positions assumed, that more than one mind has been verging to the same conclusions. It were to have been wished, at the same time, that Dr TAYLOR had noticed Dr DOIG's Letters, Mr SUMNER's work, Mr BEKE's work, or the prior one of Professor HAMILTON; for, though he may have carried out the illustration of the proposition to a greater length than these gentlemen, the merit of challenging the received theory is unquestionably due to them.

In such circumstances, I should not have thought of laying my particular views on this subject before the Society, had the opinion advanced, and so well illustrated in some of its details, by Dr TAYLOR, been in all points the same as my own. But, besides other considerations, there are two elements which enter into my theory of man's early civilization, which are totally omitted or opposed in the statements of the writers to whom I have alluded, and which it is impossible to separate from the consideration of his social condition,—I mean the *domestication of animals* and the *cultivation of the Cerealia:* And while I attempt to shew that the supposed progress of man from a savage to a civilized state is an unfounded assumption, flowing from the dreams of poets or the theories of philosophers, I shall also endeavour to make it evident, that *races of domestic cattle* and the *cultivation of the Cerealia* must have been contemporary with the earliest existence of the human race.

According to some French writers, man has arisen to what he is at present from very humble beginnings indeed. LAMARCK more than hints that some species of the Quadrumanous animals, or Apes, may, from the exigencies of their situation, have given up their natural propensities, and learned to walk, and speak, and think, by some fancied necessity of a progressive development of faculties. A similar opinion was entertained by Lord MONBODDO.† M. BORY DE ST VINCENT, following in the same train, thinks that a progress may be traced from the apes and orang to the Hottentot, and from the Hottentot to the most civilized races. The sexual propensity, according to this author, brings the male and female together; the long helplessness of infancy secures the family alliance; and hence follow tribes or connected families. Thunder has struck down trees in the ancient forests, or a crater has poured out ignited lava on the vegetation, and thus the use of fire was learned by man.‡

BUFFON supposes that the first Man, though fashioned in his body and organs

* TAYLOR's Natural History of Society, i. 19.

† Origin and Progress of Language, i. 272.

‡ Dict. Classique de l'Hist. Nat. Art. L'HOMME.

in the most perfect state, could know nothing till experience had taught him the use of his senses; and supposing such a being awakened, in the plenitude of his powers and with the faculty of speech, to the objects of the world around him, he has given a very interesting and, I believe, philosophical account of his first sensations.*

Sharon Turner in one place represents the first pair as utterly ignorant at first of every thing, and having to acquire the knowledge of whatever there was to know by gradual sensations as they should occur, and totally incapable of foreseeing any result or of distinguishing good from evil, until, by slow and progressive experience, they should learn what was either, or what would become such. —And again, " Adam could not, at his creation, be perfect in knowledge, because he would have it all to acquire, and must begin his earthly existence without any."†

Dr Adam Ferguson, forgetting for a moment that the state of the first pair bore no analogy to their future offspring, observes, that " the individual in every age has the same race to run, from infancy to manhood; and every infant or ignorant person now, is a model of what man was in his original state."‡

And Sir Humphry Davy, supposing that the first created man had certain powers and instincts, such as now belong to the rudest savages of the southern hemisphere, observes,—" Their progress from this early state of society to that of the highest state of civilization and refinement may, I think, be easily deduced from the exertions of reason, assisted by the influence of the moral powers and of physical circumstances."§

Such are some of the representations of man's early state given by writers of no mean celebrity; and such are some of the degrading theories which would bring down man to the level of the beasts around him. Several of these opinions have apparently their origin in the fables of ancient poets; but even those more modern and plausible theories, which suppose savage man capable, in the

* Buffon, par Sonnini, xx. 51. † Sacred History of the World, ii. 253, 293.

‡ Essay on the History of Civil Society, p. 7. By Adam Ferguson, LL.D. Lond. 1793.

§ Consolations in Travel, or the Last Days of a Philosopher, p. 76. In this little work, though Sir Humphry makes one of the speakers in his Second Dialogue question the classical theory of man's savage origin, yet this is so feebly done as to imply the author's want of confidence in the position his opponent is made to assume; and even the appeal to the first book of Moses is modified by passages, in which the author shews his leaning to the doctrine of savage original and progressive advancement. In the Vision, which is the base of the first two dialogues, the human race are described as advancing through all the classical periods, from the mute savage of Horace up to the pastoral and agricultural life. But Sir Humphry fails to shew how his supposed instincts could ever lead man to sow or reap, or tame animals, and none of the speakers in the dialogue explain how man, created savage, could ever have risen above that condition. Afterwards, however, he makes another speaker concede that man was created, not a savage (as formerly represented), but perfect in his faculties, and with a variety of instinctive powers and knowledge, and that he transmitted these powers and knowledge to his offspring. (P. 100.)

course of long ages of experiment, of acquiring a knowledge of himself and the external world around him by his own unaided exertions, are founded on hypotheses equally without foundation. No authority is referred to—no evidence is produced—not a single recorded instance is pointed out of the circumstances they allege, beyond the suppositions of those poets and philosophers,—for assuming the facts to be as they have stated them. That man, in certain states of civilization, and with the transmitted knowledge which is the appanage of his race, is capable of increasing his dominion over nature to an incalculable extent, and even of discovering his progressive improvement here to be connected with a future state of existence, is apparent from many considerations. But it is equally apparent, even on the recognised principles of philosophic observation, that if man had been created a degraded being, procuring his scanty subsistence from the spontaneous produce of nature, he never could, by his unaided exertions, have risen above that state,—he never could have arrived even at the first period of the philosophical gradation, or the hunter's state; for the hunter's state necessarily presupposes knowledge which an acorn-eating savage could not possibly acquire. Man has none of the instinctive propensities which guide the lower animals to their food with unerring certainty. He must be trained to be what he is; and the transmitted knowledge which he inherits as the descendant of a civilized progenitor, though it may be so far lost or deteriorated, cannot be acquired by the untaught exertions of the savage. Though the habit of flying from predaceous animals did, according to LAMARCK, lengthen the limbs and quicken the pace of the gazelles and antelopes,* so as to produce, through ages of practice, the present handsome and light forms which these animals now bear; yet, extravagant as this theory is, it would not be more so than that which would suppose a naked and fruit-eating savage, with no instinctive propensities for blood and animal fibre, no means of pursuit, and no implements of chase, to discover that the animals which fled from him would serve him for food.

But not only was man, according to the opinion of some authors, created a savage, scarcely raised above the animals around him, but, in the opinion of others, he was created a *mute savage*; and hence it has been the object of philosophers, taking this also for granted, to trace the steps which led him, from natural signs, to acquire articulate speech.† Dr ADAM SMITH, in his "Dissertation on the Origin of Languages," endeavours to trace these fancied steps. "Two savages," says he,

* Philosophie Zoologique, tome i. p. 255.

† According to LAMARCK, the dominant race, or man, having multiplied their wants as society became more numerous, felt the necessity of communicating their ideas to their companions. The result was, to augment and vary the *signs* proper for the communication of these ideas; and pantomimic signs, and all possible inflections of the voice, having failed to keep pace with the multitude of acquired ideas, they came at last, by redoubled efforts, to form *articulate sounds*. From thence the origin of the admirable faculty of speech. (Philosophie Zoologique, tom. i. 356, 357.)

"who had never been taught to speak, but had been bred up remote from the societies of men, would naturally begin to form that language by which they would endeavour to make their mutual wants intelligible to each other, by uttering certain sounds whenever they meant to denote certain objects. Those objects only which were most familiar to them, and which they had most frequently occasion to mention, would have particular names assigned to them."* Dr SMITH does not say, in direct terms, that such indeed was the state of our first parents; but if speech was not the gift to our race of the Great CREATOR, the situation he supposes must have been theirs.

According to Dr REID† and DUGALD STEWART,‡ *natural language* preceded the formation of *artificial language*, and artificial signs must have been the effect of convention. The natural signs consist "in certain expressions of the countenance, certain gestures of the body, and certain tones of the voice," of which the pantomimes of the Roman stage furnished an example. This natural language declined, according to the philosophical theory, in consequence of the use of artificial signs. "As ideas multiply, the imperfections of natural language are felt, and men find it necessary to invent artificial signs, of which the meaning is fixed by mutual agreement." But this opinion as to the origin of language, adopted or assented to rather in compliance with classical associations than his own convictions, is modified by Mr STEWART in another passage, where he says, that—"When we consider what a vast and complicated fabric language is, it is difficult for us to persuade ourselves that the unassisted faculties of the human mind were equal to the invention."§ And it is remarked by Dr ADAM FERGUSON, "that the speculative mind, in comparing the first and last steps of the progress, feels the same sort of amazement with a traveller who, after rising insensibly to the slope of a hill, comes to look from a precipice of an almost unfathomable depth, to the summit of which he scarcely believes himself to have ascended without supernatural aid."‖ I have further to remark, that although Mr STEWART, in referring to the early periods of society, when, according to the universally adopted theory of man's savage original, everything was to be learned; and considering, in accordance with this theory, "by what steps our rude forefathers must have proceeded in their attempts towards the formation of a language, and how the parts of speech arose,"—prefaces his remarks with the observation, that he does not mean to prejudge the question, "Whether language be, or be not, the result of immediate revelation?"¶ and though he gives it as his opinion that the

* A Dissertation on the Origin of Languages, appended to the second volume of the Theory of Moral Sentiments, seventh edition, Lond. 1792, vol. ii. pp. 402, 404.

† Inquiry into the Human Mind, chap. iv. sect 2.

‡ Elements of the Philosophy of the Human Mind, iii. 2, 3.

§ Ibid. vol. iii. p. 25.

‖ An Essay on the History of Civil Society. By ADAM FERGUSON, LL.D.

¶ Elements, iii. 26.

human faculties *are* competent to the formation of a language, yet his purpose is only to trace the steps which men, left entirely to themselves, would be likely to follow, in their first attempts to communicate their ideas to each other.*

Now, if it has been ascertained beyond doubt that speech is purely imitative; if the cases of individuals who have been born deaf, and, in consequence of never hearing articulate sounds, are themselves dumb, be taken into consideration, it does not appear how mute savages could, in any length of time, learn the use of articulate signs. And the cases which have occurred of human beings, left at an early age to their own resources, afford further evidence that the acquisition of speech by individuals in that situation was hopeless.†

It is certain, therefore, that the use of speech must have been an attribute of the first pair; and that, whatever was the future progress of language, its origin cannot be referred to the untaught ingenuity of dumb savages, but must start at a point when articulate speech was equal to all the physical and intellectual wants and enjoyments of man. It has been observed by more than one author of eminence, that the general analogy which runs through all the forms of human speech is in favour of the idea of its being an original attribute of our first parents. "Whence has arisen that analogy," says Mr STEWART, "which runs through the mixture of languages spoken by the most remote and unconnected nations, and those peculiarities by which they are all distinguished from each other?"‡—"From the country of the Eskimoes to the banks of the Oroonoko," says M. HUMBOLDT, "and again, from these torrid banks to the frozen climate of the Straits of Magellan, mother tongues, entirely different with regard to roots, have (if we may use the expression) the same physiognomy."§—"With man," observes Dr FERGUSON, who differs from most of the writers I have named on this

* Lord MONBODDO sets out with the proposition, that articulation is altogether the work of art or habit, and that ages must have elapsed before language was invented. Origin and Progress of Language, i. 71.

The result of an experiment made by one of our kings (JAMES IV.), and recorded by LINDSAY of Pitscottie, of sending two children under the care of a dumb woman, to be reared on the island of Inchkeith till they came of age, is not mentioned. But there can be no doubt of its termination. They were expected to speak Hebrew. (Chronicles of Scotland, p. 250. By ROBERT LINDSAY of Pitscottie. Edinburgh, 1814.)

"Were it possible," says Dr SMITH, "that a human creature could grow up to manhood in some solitary place, without any communication with his own species, he could no more think of his own character, of the propriety or demerit of his own sentiments and conduct, of the beauty or deformity of his own mind, than of the beauty or deformity of his own face." (Theory of Moral Sentiments, i. 277, 278.)

† See account of Peter the Wild Boy, and other instances, in MONBODDO's work on the Origin and Progress of Language. Edinburgh, 1773.

‡ Life of ADAM SMITH, LL.D. p. 47. 4to, Edinburgh, 1811.

§ Personal Narrative, iii. 245.

point, "society appears to be as old as the individual, and the use of the tongue as universal as that of the hand or foot."*

Such are some of the statements hazarded by philosophers and historians relative to the earliest ages of human society. In discussing this subject, it appears somewhat strange that the traditionary accounts of DIODORUS SICULUS and HERODOTUS, and the early poets, should alone have formed the groundwork of the philosophical theories of man's origin and progress, in opposition to the narrative of such origin and progress contained in the First Book of MOSES. That this sacred Book is not referred to as authority on the subject by a certain class of philosophers may proceed, in some, from the mistaken idea they entertain of reference to such authority superseding all further argument or inquiry. But there are others who, in the investigation of the works of nature, the structure of their own minds, or the history of their race, systematically avoid allusion to the Great FIRST CAUSE, as if their own delegated and limited powers were sufficient, in all the relations of mind and matter, to lead to final results without supposing HIS agency, or tracing the operations of HIS hand. It is gratifying to think that the present race of investigators are of a different character; and in the boundless field of the Natural Sciences,—in the world of Mind and Matter within us and around us,—it is one aim of modern philosophy to trace the indications of infinite wisdom and beneficence, unfolded at every step of its progress.

In this instance, I refer to the Scripture account of the origin of man and of his subsequent progress, so far as there detailed, as the most ancient, the most rational, and the only true account of the early history of our race. The facts there recorded are further corroborated by all that is known from other sources, of the spread of man, the traces of his progress, and by the existing monuments of the earliest times.

According to this authority, then, which is indisputable, man was not, at his first creation, a mute and rude savage, totally ignorant of everything around him till taught by experience, and not amenable to moral responsibility. On the contrary, the Scripture teaches that man was, at his creation, not only endowed with all the physical perfections belonging to our race in the highest degree, but also with all the intellectual information necessary to the happiness and enjoyment of the most perfect human being. GOD did not create man a mute savage on the banks of the Ohio or the Euphrates, with a spear in his hand, and an instinctive thirst of blood to urge him to his prey:—HE did not place him on a barren shore, to feed upon the blubber of the stranded whale:—HE did not destine him to feed upon the acorns of the forest, or scratch up edible roots from the soil to satisfy his hunger; but HE placed him in a garden, rich in all the productions of vegetable life, and

* Essay on the History of Civil Society, p. 9.

enlivened by all the forms of animal creation, that could minister to his wants or pleasures, and give him a

——— choice
Unlimited of manifold delights.

Neither was the proverbial idleness of savage life the lot of the first man; for his mental faculties were exercised in naming, according to their different natures, the various races of animals; and exercise was provided for his physical frame, in keeping and dressing the garden in which he was placed.

In accordance with the Sacred Record, MILTON asserts, in his unrivalled poem, that man was not created a mute and ignorant savage, but endowed with all the physical and intellectual powers which distinguish the human race from every other class of animated beings. He was not made, as the philosophical theory supposes, a little higher than the brutes, but "a little lower than the angels;" and though, since the introduction of moral evil, his tainted nature has indicated a downward progress, yet unfading traces remain of that Divine image in which he was originally created. In describing the inmates of Eden, MILTON says,

Two of far nobler shape, erect and tall,
Godlike erect! with native honour clad,
In naked majesty, seemed lords of all;
And worthy seemed,—for in their looks divine,
The image of their glorious Maker shone.—*Book* iv.

And afterwards, in making ADAM describe his first sensations when he awoke to life, he introduces him as saying thus—

——— To speak I tried—and forthwith spake;
My tongue obeyed, and readily could name
Whate'er I saw.—*Book* viii.

And again, in introducing him as naming the inferior creatures, these verses occur:

As thus HE spoke, each bird and beast behold
Approaching, two and two; these cowering low,
With blandishment each bird stooped on his wing.
I named them as they passed, and understood
Their nature. With such knowledge GOD endued
My sudden apprehension.—*Book* viii.

If anything required to be added to this, the fact of the Great Creator holding converse with the first of his creatures, and placing him in a station of moral responsibility, leaves no room to doubt of either ADAM's moral or intellectual endowments. Through him was to descend to his progeny the knowledge which was to guide their future progress.

In accordance with the same views, Bishop STILLINGFLEET observes, that "if man were not fully convinced, in the first moment of his creation, of the being of

Him whom he was to obey, his first work and duty would not have been actual obedience, but a search whether there was any supreme, infinite, eternal Being, or no, and whereon his duty to him was founded, and what might be sufficient declaration of his will and laws, according to which he must regulate his obedience."* And in another passage, after some important observations on man's knowledge as respects his fellow-creatures, and of the nature, being, and properties of those things which he was to use, he concludes as to the first man, that, "as he was the first in his kind, so was he to be the standard and measure of all that followed, and, therefore, could not want any thing of the due perfections of human nature."†

It thus appears, that the brutal or savage origin of man, and his gradual attainment, through ages of experiment, of the faculty of speech, and the use of his intellectual powers, is not warranted by any thing recorded of his origin or early history. Had he been created a dumb savage, a dumb savage he must for ever have remained. Had the spontaneous fruits of the forest been his instinctive food, he never could have learned to use any other; for instinct only teaches to take what nature provides, and with the instruments which she has furnished. No process of thought or reflection could be able to convey to a frugiverous animal the idea that living creatures might be converted into food. The classical theory, besides, in advancing from feeding on acorns and fruits to pursuing wild animals for their flesh, and taming them in numbers, goes much too far at one step to render it in the slightest degree probable. An acorn-eating savage might, by some possibility or chance, take to the rearing of the fruits or seeds that produced his accustomed food; and thus step from the first to the last link in the progressive chain of civilization, without altering the nature of his food. But the step from fruits to flesh—from roots to living fibre—is one which involves, even in its supposition, such a violation of all probability, as to forbid the idea that this can be the process followed by nature.

Those authors who represent the first pair to have been created mute savages, living on the spontaneous productions of the garden or forest, forget that, if such had been the case, they never could, by their unaided exertions, have risen beyond that state. If the hunter's state, according to others, was the earliest form of human society, how, it may be asked, did man discover that the objects of his pursuit would serve him as food? Is there any analogy between feeding on acorns or apples, and the raw flesh of animals procured by hunting? Had man an instinctive predilection for carnage—an irresistible appetite for living prey? It is impossible to answer these questions in the affirmative. If this were indeed Man's destination, his physical conformation would have been adapted to this

* Origines Sacræ, &c. i. 2. By EDWARD STILLINGFLEET, D.D. late Bishop of Worcester.

† Ibid. i. 4.

mode of procuring his food. Like other predaceous animals, the structure of whose members are all in strict conformity with their destined mode of life, man must have had fangs to tear, and claws to seize, his living prey, if such were to have been the principal or sole means of procuring his food. Besides, the hunter's state, as practised by tribes of savages known to Europeans, presupposes knowledge of various kinds to a considerable extent, both in regard to the habits of the animals and the implements of the chase. And it does not appear by what process of reflection or experiment, savages, ignorant of the use of dressed food, or of fire to dress it, could have acquired the knowledge of this necessary of life. M. BORY DE ST VINCENT may talk of the electric fluid kindling into flame the dry woods, or craters of volcanoes throwing out red-hot embers, and raising into temporary fires the surrounding vegetation ; but savages, to whom fire and its uses were unknown, could not, even from these appearances, have learned its use in dressing food or producing heat. A philosopher of the present day, with all the knowledge and appliances of modern science, might, from the accidental burning of a forest or the vegetation kindled by a volcano, form some idea of the uses of an agent such as this, both in regard to the preparation of food and the production of heat ; but to suppose an ignorant savage, without even the knowledge that food required dressing, or that there was any heat independent of the sun's rays,—to suppose such a being to discover that, from the friction of two pieces of wood, or by striking a flint on a piece of iron, he could produce living flames, is to take for granted the most improbable of all propositions.

But, granting for one moment that wild fruits produced spontaneously, and the animals supplied by hunting, formed the food of the earliest races, the question must recur, How did these hunters acquire the knowledge that the animals they pursued in the field or forest could be tamed and reared beside their huts? It appears far more probable, that the animals least capable of escaping the arts of the hunter would have been extirpated by the increasing population, unless they had existed in untold numbers, and over territories beyond the range of the hunting-ground.* And even from the taming of a single fawn or kid, procured alive by accident, as Lord KAMES supposes to have been the case, it could scarcely be inferred that the whole race, and other races of independent animals, could be made to contribute to the increasing wants of man. The savages of North America have not tamed the bison of their prairies nor the tapir of their marshes; and indeed there is no recorded and authentic instance of hunting-savages taming wild animals and subjecting them to their use, or rising by their own efforts from the hunting to the pastoral state.

Besides, it is a well ascertained fact, that all herbivorous animals have an instinctive dread of their natural enemies, the predaceous races, and fly at their approach. And to this instinctive distrust, which makes the deer or the antelope

* RAYNAL's Hist. of East and West Indies, Book xviii.

fly from the lion or tiger, the preservation of the species is to be attributed. If the lion and tiger are provided with fangs and claws for the purpose of procuring food suitable to their carnivorous propensities, so the more timid animals, which form their prey, are provided either with weapons to defend themselves so far when in numbers, or speed to distance their enemies in the chase. But man has, confessedly, none of that instinctive ferociousness which impels him to feed on living prey; neither the structure of his body nor the arrangement of his members is calculated for such a mode of acquiring food; and when he hunts down animals as a portion of his subsistence, he does so from knowledge previously acquired by individuals of his race, and with implements suited to the nature of the animals pursued.

And if the transition from the hunting to the pastoral state seems extremely improbable,—and, if we may judge from not one instance of this transference having been observed, we may say impossible,—by what train of circumstances could an entirely pastoral people become agriculturists? If the seeds of the agricultural plants do not grow spontaneously,—and there is no evidence of their doing so anywhere, or to any useful extent,—where did these early essayists in cultivation procure their supply of seeds? What could induce them to attempt the transformation, by cultivation, of a sterile herb (according to BUFFON) into wheat? or what could teach them that, after years or ages of experiment, a barren grass (according to other authors) would appear in their fields as oats or barley? No pressure of population—no reflection on the processes of nature which they witnessed around them,—could lead men, ignorant of seeds beyond the produce of the forests, to conceive that the small seeds of the grasses, buried in the ground, would, after a time, be replaced twenty-fold, even if they escaped destruction from their multiplied herds.* And it is not very evident how men, in that degraded situation, could first ascertain that the labour of the horse and ox might be made available for the cultivation of the ground.

But in point of fact, this fancied gradation has no existence in nature. "Throughout all America," says Dr ROBERTSON, "we scarcely meet with any nation of hunters which does not practise some species of cultivation;"† the Koords in Mesopotamia unite the pursuits of shepherds and cultivators;‡ and, according to HUMBOLDT, tribes of savages exist in South America, who, assembled in villages, cultivate the plantain tree, cassava, and cotton.§ A species of agriculture—the cultivation of maize—existed in America long before the arrival of the Europeans: and, as in mockery of the classical stage of pastoral life, the Indians of this great Continent have overleaped this intermediate step, of which, perhaps, they were not aware, and joined a species of agriculture to the pursuit of hunting. HUMBOLDT accounts for this anomaly in human progression by stat-

* MALTHUS, Essay on the Principle of Population, p. 43, 4to edition.

† ROBERTSON'S America, ii. 117.

‡ BUCKINGHAM'S Travels in Mesopotamia, i. 300.

§ Personal Narrative, iii. 211.

ing, that they had none of the animals which furnish milk in abundance; that their immense plains, more fertile than the Steppes of Asia, remained without herds; and that, in consequence, in America "the intermediate link is wanting that connects the hunting with the agricultural nations."* In Asia and Africa, the same practice prevails among all the ruder tribes. There are few or none, whatever be their more general mode of procuring food,—whether it be hunting or fishing,—that do not raise some species of root or fruit, in addition to what they otherwise procure. The really pastoral tribes of mankind, on the other hand, confined to immense plains where agriculture is impracticable, were pastoral from the beginning, and promise to be so in all time coming.†

The use of fire, and the preparation of food in some way or other, are universal among the human race; and in the rudest state in which man is now found, there are arts exercised in the procuring and dressing of his food, in the preparation of his clothing, or the erection of his habitation, which he never could have acquired but from progenitors more advanced in civilization than himself. The universality of these arts, when added to the traditions of the most barbarous tribes and the general and acknowledged filiation of their languages, point out a less base original of the race than the degrading theories of the classical writers have supposed.

If civilization were, indeed, the slow result of experience, the earliest savages must have been ages in acquiring even the necessaries of the most humble form of human society. Instinctive feelings, common to all animals, might have led them to satisfy their hunger from the acorns of the forest, and assuage their thirst at the running stream. But it does not appear how their knowledge of digging the soil for edible roots, or cultivating the most simple herbs, could originate without supernatural aid; and if man had been created a savage, without the knowledge of speech, a savage he might have for ever remained among the beasts of Eden, distinguished only by his form from the creatures around him. The theories of philosophers as to acquired information, however just when applied to man in his ordinary descent, have no analogy when considered in reference to the first man. He must, in the nature of things, have been endowed with speech,—intuitive perceptions of external nature and its relations,—the knowledge

* Personal Narrative, iv. 319.

† "The circumstances of the soil and the climate determine whether the inhabitant shall apply himself chiefly to agriculture or pasture; whether he shall fix his residence, or be moving continually about with all his possessions." (An Essay on the History of Civil Society. By Adam Ferguson, LLD. p. 162.)

"The wide extended plains inhabited by the Tartar tribes, without a shrub, which the Russians call Steppes, are covered with a luxuriant grass, admirably fitted for the pasture of numerous herds and flocks." The inhabitants are "necessarily condemned to a pastoral life." (An Essay on the Principle of Population, &c. 4to. p. 92. By T. R. Malthus.)

See also Alison on Population, vol. i. p. 21, 22; and Heeren's Manual of Ancient History, p. 16.

of the Deity and moral responsibility, else our race had never gone beyond those tribes of animals which, with much of the human form, have never advanced one step from their original condition. If the first man's knowledge was not intuitive—had not been communicated directly to him by his Maker,—he never could have availed himself of any one of the advantages which knowledge is supposed to give to rational beings. By no mental process could he have ascertained that seeds buried in the soil would be multiplied twenty-fold by an unknown process, or that the animals which fled at his approach might be tamed and increased at his will. And none of the ordinary sources of knowledge, the result of slow experiment or years of observation, open to the ingenuity of his descendants, could have been available to the first agriculturist and shepherd.

II.—Domestication of Animals.

II. I now come, in the second place, to make a few observations on the *Domestication of Animals*, which is considered by many to follow, as a necessary consequence, the civilization of man. This also, according to the classical theory, was a work of time; and the results of domestication, as now seen, the ultimate effect of ages of training. Even the opinions of the few who have questioned the theory of man's savage origin are, on this point, in accordance with the supposition which assumes that all animals were originally wild, and required to be caught and tamed, and trained to obedience, before becoming useful to man. But this assumption, like that of man's savage origin, rests upon similar baseless assertions, and is equally devoid of truth as probability.

The error, on the one hand, has originated in supposing man to have been created a savage, little better than a brute, and in supposing that all his subsequent improvement, even the power of speech, originated from the exercise of his own physical and intellectual faculties; and, on the other, in omitting to take into consideration two indispensable elements of increase and civilization, without which, indeed, man could not, confessedly, have advanced a single step,—I mean the possession of *Domestic Animals* and the *Cultivation of the Cerealia*. The animals they suppose to have existed in a wild state, till the period of society arrived when it became necessary for the demands of an increasing population that they should be tamed; and the seeds of the *Cerealia*, or grains, it is not explained how, were to be at the command of the multiplied shepherds, whenever they were forced to exchange the pastoral life for one devoted to the tillage of the ground. No even plausible grounds are stated how or where these necessary appendages to civilized life should be found in numbers or quantity when so wanted, and if such auxiliaries were within reach at these uncertain periods, why might their use not have been contemporary with man's earliest existence? why might he not have reaped all the advantages which their possession implies, from the

beginning? In fact, there is nothing in the statements of those who support the theory of gradual taming and ages of training beyond conjectural assertion or gratuitous assumption.

"A fawn, a kid, or a lamb," says Lord KAMES, "taken alive, and tamed for amusement, suggested, probably, flocks and herds, and introduced the shepherd state."*

According to BUFFON, "the most feeble species of useful animals have been first reduced to domesticity. The sheep and goat have been acquired before having tamed the horse, the ox, or the camel."† And again, "Man changes the natural state of animals in forcing them to obey him, and subjecting them to his use. A domestic animal is a slave which amuses him, which serves him, which he abuses, which he alters, and changes its country and nature."‡

In another place, the same naturalist writes thus: "It is by the power of his mind, and not by physical force, that man has subjugated animals. In the earliest times, all were equally independent. Man, become criminal and ferocious, was little calculated to tame them. Time was necessary to approach, to observe, to choose, to subjugate them. It was necessary that he himself should become civilized, to be able to instruct and command; and the empire over animals,—like all other empires,—was not founded, but with the progress of society." §

To the same effect writes the Abbé RAYNAL. "Men," says he, "while they live at large, never bring any of the animal species under their subjection. All the knowledge they have is to destroy them. The taming of animals is always posterior to the social state. The taming of animals, as well as all the other useful arts, was doubtless one of the inventions of society."‖

"A savage," says ROBERTSON, "is the enemy of the other animals, not their superior."¶

"In the domestication of animals and the cultivation of plants," says Mr LYELL, "mankind have first selected those species which have the most flexible frames and constitutions, and have then been engaged for ages in conducting a series of experiments, with much patience and at great cost, to ascertain what may be the greatest possible deviation from a common type which can be elicited in these extreme cases."**

To nearly the same purport writes Dr TAYLOR, the latest author on the subject. "The art of domesticating animals," says he, "and so completely changing their nature as to efface the original type, requires more intelligence than we are accustomed to suppose, and it is not easy to conceive how the attempt could have been originally suggested. It is also very singular, that the number of domesti-

* Sketches of the History of Man, i. 46.
† Histoire Naturelle, edit. SONNINI, xxix. 239.
‡ Ibid. xxii. 65.
§ Ibid. xxii. 70.
‖ History of East and West Indies, Book xviii.
¶ ROBERTSON's History of America, ii. 124.
** Principles of Geology, iii. 74.

cated species has not been increased by the lapse of time, though, at first sight, there are many of the untamed animals which might have been subdued and rendered serviceable."* And Baron CUVIER, when speaking of the dog as a powerful ally of man against the other animals, characterizes its domestication as the most complete, the most singular, and the most useful conquest our race has ever made.†

Such are the statements in authors regarding the domestication of animals. Like the supposed civilization of man, it was conjectured to be a work of slow degrees, brought about by the efforts of ages, and effecting such changes in the habits and structure of the animals, that their original type is unknown; *i. e.* that there are no existing races in a wild state resembling the domesticated individuals, and from which they may be said to be derived. But upon what authority do the tamers of animals rest for this supposed course of training, which fits animals naturally wild for domestication? None whatever, beyond the dreams of poets or the fanciful theories of philosophers. The very first step in the proposition is entirely conjectural. It being taken for granted that the species of cattle now domesticated existed originally in a wild state, the ingenuity of naturalists has, in consequence, been taxed in vain to find out the original types and their original country. The result of these investigations is thus stated by DESMAREST, one of the most celebrated modern zoologists, in the case of the ox, and the same may be said of all the others. "The domestic ox of Europe," says he, "of which the primitive source seems lost, has been transported into all the countries where Europeans have established colonies."‡

While the geographical limits of many families of animals can be distinctly defined,—while climate and soil confine certain races to certain localities, beyond which they cannot exist,—it is remarkable that the ox, the sheep, the goat, the horse, the dog, the most extensively useful of all the domesticated animals, have, like man, scarcely any limit to their range. Wherever civilized man is found, there are these animals, or some of them, varied in many particulars to adapt themselves to their different locations, but still the same species, with the same general capabilities.

The naturalists who have ventured to assign a peculiar country to the domesticated animals as their original one, universally point out the ancient seats of human civilization in Asia as the place of their origin. Thus, "the horse," says DESMAREST, "originally from the plains of Tartary, has been transported by man wherever he has established himself, over the vast countries of Asia, Europe, Africa, and America." Asses, it is said, are found in a wild state, in innumerable troops in the country of the Calmucks; the country of the sheep is the ancient world; and the ox is found in all the countries where Europeans have established

* The Natural History of Society in the Barbarous and Civilized State, &c. i. 195, 196.

† Règne Animal, i. 152.

‡ DESMAREST, Mammalogie, 494.

themselves. Though many places are indicated where the sheep, the goat, the horse, are apparently in a wild state, yet, for reasons to be mentioned afterwards, the probable explanation of the fact is, that these apparently wild races have taken their origin from stragglers from the herds introduced by man.* It is well known that, in some cases, this has happened, as in South America, where the horses, from domesticated sources, have increased to a great extent; and the horned cattle, sent to the Llanos by CHRISTOVAL RODRIGUEZ about the year 1548, no less so: And if, in such circumstances, and in so short time comparatively, these animals have established themselves, and become in many respects wild animals, is it not a fair conclusion, that the domesticated species now found wild along the tracks of human migration, and nowhere else, may have become so from the same cause? And this conclusion is supported by the fact, contrary to all analogies of other wild animals, that when these reputed wild races are taken, even in adult age, they are speedily reclaimed, and become again the associates of man.†

In 1825, M. F. CUVIER published two Essays, one on the Sociability of Animals,‡ the other entitled an "Essay on the Domestication of Mammiferous Animals;"§ the opinions expressed in which derive weight, not only from the character of the author, but from the opportunities he enjoyed of studying the habits of animals. This celebrated naturalist ascribes the domestication of animals as owing to what he terms *an instinct of sociability*,—a social instinct in the creatures themselves, accompanied with qualities to aid its influence. "To attain an object," says he, "it is necessary to know it; and how could the first men who associated themselves with animals have known this object? And had they conceived it hypothetically, would not their patience have been exhausted in vain efforts, from the innumerable attempts they would have had to make, and the great number of generations on which they would have to act, in order, after all, to arrive only at superficial results."‖ So far M. CUVIER writes with the caution of a philosopher. He afterwards goes on to state, as the result of all his knowledge and all the experiments that had been made in the taming of animals, that no conceivable training, without a particular disposition in the animals to attach themselves to man, could have ever effected this object.

With regard to the effects of the social instinct of gregarious animals as inducing them more easily to come under the protection of man, if the effect of this social instinct were to render all gregarious animals of equally easy acquisition, I

* "The natural state of the horse, it may be said, is not that of freedom, but of domestication." (Illustrations of the Breeds of Domestic Animals in the British Islands, No. vi. p. 6. By DAVID LOW, Esq. F.R.S.E.)

† DESMAREST, Mammalogie, 422.

‡ De la Sociabilité des Animaux, Mém. du Mus. xiii. 1. "It is difficult to conceive how they could commence and maintain the submission of animals without this disposition to sociability, if we consider, above all, at what time of human civilization the domestic animals appear to have become so." (P. 19.)

§ Mémoires du Museum, xiii. 406.

‖ Ibid.

should at once grant the principle as a chief means of their domestication. But all gregarious animals, as M. Cuvier remarks, are not found capable of domestication. The greater number of the untamed Ruminants herd together in flocks; the zebra, the wolf, the hyæna, the beaver, are found in companies, and are yet untamed by man. And even the tribes of quadrumanous animals, or apes, most nearly resembling the human form, though with hands at their extremities capable of performing all the actions of human beings, are yet the untamed denizens of the forest. Domestication is not, therefore, the necessary or sole result of the social instinct of gregarious animals, even carried to a high degree, else all gregarious animals would be equally capable of this domestication. And though it be true, as a general rule, that no solitary species, however easy it may be to tame the individuals, has ever afforded domesticated races, yet the common cat forms an instance of an exception to this rule. A particular disposition in the animals themselves—an instinctive propensity to attach themselves to the human race,—is therefore necessary, as M. F. Cuvier has stated, to their complete domestication.

This tendency in certain animals to become the associates of man, has been noticed by other observers. "It has been proved," says Buffon of the goat, "that these animals are naturally the friends of man, and that, in inhabited places, they do not become wild."*—"Compare the docility and submission of the dog, with the distrust and ferocity of the tiger; the one appears the friend of man, and the other his enemy."† The wild cattle of the island of Tinian, met with by Lord Anson in his voyage,‡ were not at all timid, and they had no difficulty in getting near them. The wild horses of the Llanos, according to Humboldt, are easily reduced to servitude, and their good qualities developed.§ The goats met with at the island of Bonavista by an early voyager, followed the negroes with a kind of obstinacy; and, according to Dr Richardson, there is no difficulty in approaching the Rocky Mountain sheep, which, in the retired parts of the mountains, exhibit the simplicity of character so remarkable in the domestic species.‖

A late writer on the "Influence of Domesticity upon Animals,"¶ M. Dureau de la Malle, after asserting that the origin and country of our domestic animals had been sought for in vain, states, in apparent opposition to this, that all the tamed animals existed in a wild state in Europe in the time of Aristotle; and that, in four hundred and fifty years from Aristotle to Pliny, the domestication of animals had but slowly extended. It is conceded at once, that the domesticated species of animals might be found in a half wild state wherever human settlements had introduced them, at the period alluded to, as at present: and the

* Buffon, Hist. Nat. xxiii. 99. † Ibid. p. 67.

‡ Anson's Voyage round the World, 4to, p. 309. Lond. 1776.

§ Personal Narrative, iv. 340. In Paraguay, they are put in harness when caught, and a day is sufficient to tame them. (Robertson's Letters on Paraguay, ii. 6.)

‖ Fauna Boreali-Americanæ, p. 279.

¶ De l'Influence de la Domesticité sur les Animaux, Ann. des Sciences Nat. xxi. 52.

subsequent remark, that no new animals had been added to the number of domesticated species then known in the space of 450 years, is likewise consonant to what I hold to be the truth. For it is a singular fact, and borne out by all observation, that no new species of domestic animal of any consequence has been added to those which have been the property of man from the first times. The animals at present domesticated have been so from the earliest period of human history; in all man's wanderings, they have accompanied his progress; and it is only in regard to America, the first settlers in which Continent may have been driven from their native shores by a thousand ways which may easily be conceived, that the migrations of the race seem to have been without the cattle of the Old Continent.

It is, besides, incumbent on those who support the poetical and philosophical theory, to point out, in the course of ages, a single instance of an important animal having been added to the stock of the domesticated races. All the animals now known as the property of man—the goat, the sheep, the ox, the dog, the horse, the ass, the hog, &c.—were the companions of man from the earliest times. The arts of Greece and Rome, the reasonings of philosophers, or the songs of poets, have not enabled them to seduce or charm one animal more from the wilds, or to add one individual to the domesticated races, though Africa and Asia were ransacked for animals to exhibit in the shows of the Roman people, and forms, never seen in Europe before, were displayed in numbers to the Roman citizens. The camel, from its limited geographical range, is only known in domesticity; and all the reputed wild animals of the domesticated species have originated from them alone. Surely if the training of animals has been progressive, as alleged, some of the reputed savages of the ancient world might have left one or two useful creatures untamed by them, for the benefit of modern philosophers, and to illustrate their theories. Let the adherents of the theory of taming, and domestication, and gradual change, make an experiment upon the fox,—said by John Hunter* to be one of the progenitors of the dog,—let us see foxes protecting in place of pillaging the poultry yard,—and this not in the case of an individual fox, but of the whole race of foxes in the country,—and then there will be some shew of reason for supposing that the domesticated animals were thus subjected to the service of Man.

M. Dureau de la Malle concludes the paper to which allusion has been made, with the announcement of the result to which, he says, he has been led by his researches, and that is, that he believes it may be affirmed that the greater portion of our domestic species of animals is originally from Asia, and have been transported to Western Europe by early wanderers from the first habitations of man.

As to what some authors say of the original types of the races of cattle being

* Phil. Trans. 1787, p. 253.

unknown, and of tamed animals, on returning to their original wild state, resuming the characters of their original condition,—this, too, is an assumption without evidence to render it even probable. The existence of embalmed animals in the tombs, and of figures on the monuments, of Egypt, shew, that at least for three thousand years there has been no essential change in form and structure.* The wild horses of the Tartarian plains, the wild horses of South America, have returned to no common type materially different from the races from which they are descended;† and the black cattle of the Llanos are of all the colours of the domestic varieties. Even the dogs introduced by Europeans to various countries, and which have become wild, have not, in the course of years, reverted to their supposed original sources, and become wolves and jackals, but obstinately remain dogs still, in defiance of all theories to the contrary.

That the domestication of cattle was not the slow result of experiments continued for ages, is farther demonstrable from the rapid increase of population in the early periods of the world. This increase could not have taken place if agriculture, including the domestication of cattle and the cultivation of the Cerealia, were then unknown, and the first tribes had roamed over the extensive hunting-grounds of a world to be peopled, or gleaned their meagre food from the spontaneous produce of the woods and fields. The American races did not advance far in agriculture, according to Dr Robertson,‡ as they had no tame animals, and knew none of the useful metals.

The writers on population have almost universally agreed as to the principle, that the numbers of mankind could not increase to any marked extent without pasturage and agriculture; and, of course, in compliance with the prevailing theory, all their disquisitions have reference to the hunting, the pastoral, and the agricultural state, as stages naturally produced in savage human nature by the pressure of numbers on the amount of food.§ "The first hordes," says Baron Cuvier, in compliance with the prevailing theory, "made little progress. Reduced to live by the chase, by fishing, or by wild fruits; obliged to give all their time to the search of subsistence, they could not multiply much, because all the game would have been destroyed. Their arts were limited to the construction of huts and canoes, to cover themselves with skins, and fabricate bows and arrows." And again, "When they had tamed the herbivorous animals, they found, in the possession of numerous flocks, a certain subsistence, and some leisure, which they

* Hasselquist's Travels in the Levant, 90, 91. † Humboldt, Personal Narrative, iv. 340.

‡ History of America, ii. 117.

§ Mr Malthus, however, is of a different opinion. "If hunger alone could have prompted the savage tribes of America to such a change in their habits, I do not conceive that there would have been a single nation of hunters and fishers remaining; but it it evident that some fortunate train of circumstances, in addition to this stimulus, is necessary for this purpose." (An Essay on the Principle of Population, &c. By T. R. Malthus, A.M. 4to, p. 43.) And in another place, he says, "It may be said, however, of the shepherd as of the hunter, that, if want alone could effect a change of habits, there would be few pastoral tribes remaining." (P. 92.)

might employ in extending their knowledge;" and he then states, as the third stage in the progress of civilization, that "man did not multiply his species to a great degree, and extend his knowledge and his arts, till the invention of agriculture, and the division of the soil into hereditary properties."*

"Accustomed, as we are," says Mr ALISON, "to the powers which ages of civilization have conferred upon mankind, and to the complete subjugation of animals which has resulted from the extension of his numbers, we can hardly imagine the difficulties with which our forefathers had to contend when society was in its infancy, and when the human race were placed in the midst of boundless forests and morasses, only to become the prey of the innumerable savage animals by whom they were peopled."† And in a passage immediately after, on considering the precarious situation of the supposed savage state, the few individuals who, in that state, survive infancy, and their want of skill in the arts which minister to the necessities of life,—"it seems," says he, "surprising how his numbers could have ever increased." And he can only account for this by supposing "the unlimited operation of the principle of increase" to be essential, both in the savage and pastoral state, to the extension and improvement of the human race.

Assenting, as I willingly do, to the proposition, that man could make little or no progress in numbers or civilization without domestic animals and the cultivated grains, the writers who adopt the progressive theory fail to shew how, in the nature of things, the hunter of wild animals could be converted into their protector; and how, even supposing one animal to have been accidentally tamed, he could from thence conclude that the species might be rendered domestic. What could induce the first man, if created a savage, and feeding on acorns and the apples of the wood, to think of killing the animals around him, and using them as food? There is no instinctive thirst of blood in the nature of man, I have already observed, to lead him to seize and devour living prey. And, even on the supposition that, by an instinctive propensity, he was led to kill and devour animals, how could he suppose that such animals could be tamed and reared in numbers around him? Every timid or herbivorous animal flies by instinct from its natural enemy; and if savage man were that enemy, it is not easy to see how they ever could have been domesticated.

On the other hand, if it can be proved, from the earliest histories of the race, that the knowledge of agriculture, including the pasturage of flocks and the cultivation of the Cerealia, were known to the first man and his immediate descendants,—then all imaginary theories of progressive improvement from the savage

* Règne Animal, i. 78.

† The Principles of Population, and their Connection with Human Happiness, i. 10. By ARCHIBALD ALISON, Esq. F.R.S.E.

Professor Low considers the ox and sheep, domesticated from the earliest records of human society, to have been instruments, under Providence, for leading man from the *savage state*. (Illustrations of the Breeds of Domestic Animals in the British Islands, No. iv. p. 11. By DAVID LOW, Esq. F.R.S.E.)

state, must be looked upon as little better than idle dreams. The first descendant of the first man, according to the sacred historian, was a "tiller of the ground;" his brother was "a keeper of sheep;" and both of these occupations are given as contemporaneous with one another, and with the existence of men. And such was the increase of population, that the son of Cain gave his name to a city. The nomade or pastoral life, to which the neighbouring country was well adapted, is not characterized as a separate mode of living till some considerable time afterwards. The use of metals, and the practice of at least one of the fine arts, is recorded in the short narrative given by Moses; and every circumstance referring to the race, entitles us to conclude, that a high state of civilization—at least a state equal to all the wants of society at this period—prevailed among the descendants of the first pair.

It is to be remarked, besides, as supporting the contemporaneous existence of domesticated cattle with man, that, in the sacred narrative, they are distinguished by a parenthetic clause, separating them from the other beasts. "And God made the beast of the earth after his kind, and *cattle after their kind.*" (Gen. i. 25.) And again, "And Adam gave names to *all cattle,*" &c. (ii. 20.) And afterwards, "Let him have dominion *over the cattle.*" (i. 26.) The same phrase is used when narrating the animals that went into the ark: "Every beast after his kind, *and all the cattle after their kind.*" And this interpretation of the passage is fully warranted on the authority of another inspired writer. In the 8th Psalm, alluding to the high rank of man in the scale of created beings, this passage occurs: "Thou madest him to have dominion over the works of thy hands—*all sheep and oxen,* yea, and the beasts of the field." The corresponding phrase for "cattle" in the former passage is here "sheep and oxen."

III.—Cultivation of the Cerealia.

I now come, in the *third* place, to make a few remarks on the *Cerealia*, or cultivated grains, as connected with the third stage of man's supposed progress from savage to civilized life. These, like man himself, and the domesticated animals, are supposed to have attained their present productive powers, through long ages of experiment; and some writers have considered it as a signal triumph of art over nature, to have improved barren grasses into the wheat, barley, and oats of the present day. Thus Buffon states, in regard to wheat, "Wheat, for example, is a plant which man has changed to that point, that it nowhere exists in a state of nature."*—"The same corn (says Sir Humphry Davy) which, four thousand years ago, was raised from an improved grass by an inventor, worshipped for two thousand years in the ancient world under the name of Ceres, still forms the principal food of mankind."† And Dr Taylor, in his lately published

* Buffon, xxiii. 177. † Consolations in Travel, or the Last Days of a Philosopher, p. 36.

work, asserts something to the same effect. "The fact that natural productions," says he, "became so altered by cultivation as to lose their original characteristics, is an incentive both to industry and ingenuity. We do not know what was the original type of wheat, oats, or barley; but we may reasonably conjecture, from this very circumstance, that the *Cerealia*, in their wild state, were not well suited to human sustenance."*

The native country of wheat and rye is unknown (says PARMENTIER), though cultivated over all Europe.† Barley (says BOSC) was brought from Upper Asia, where OLIVIER has found it in a wild state.‡ And DE CANDOLLE is of opinion, that "when the introduction of cultivated plants is of a recent date, there is no difficulty in tracing their origin; but when it is of high antiquity, we are often ignorant of the true country of the plants on which we feed."§

Lord KAMES, forgetting, or not being aware of, the fact that the *Cerealia* are never found growing spontaneously, at least to serve to any extent as human food, takes it for granted that wheat, rice, barley, &c., must have grown spontaneously from the creation; and his reason for this opinion is certainly a pretty strong one; "for (says he) surely when agriculture first commenced, seeds of these plants were not procured by a miracle." ‖

The ancient historians, connecting the traditions of their deities with the acknowledged benefit of the cultivated grains to man, have referred to Isis, the Egyptian Ceres, as having found the vine, and wheat, and barley, growing wild in the valley of Jordan, and introducing them into cultivation among the early Egyptians.¶ A late writer on the *Cerealia*, M. DUREAU DE LA MALLE, following the indications of these ancient writers, fixes upon *Nysa*, in the plain of Jordan, as probably the native country of the *Cerealia*, from whence they were spread over all the civilized world, wherever man settled in his peregrinations. The Isis of the Egyptians, transferred into the goddess of corn and husbandry,—the Ceres of the Greeks and Romans,—the rites of her worship, it is said, indicated the progress of agriculture in the Roman Empire.

But if agriculture was the third and most improved stage in the gradation of human civilization, and if it be ascertained that the *Cerealia* grow nowhere spontaneously, Lord KAMES's question, where the seeds should come from, when the pastoral tribes took to cultivating the ground, is one that must puzzle the supporters of the classical theory. If the spontaneous growth of these plants, to the extent of serving for human food, be taken for granted, the necessity of agricul-

* Nat. Hist. of Society, ii. 87. † Nouv. Dict. xxiv. 27. ‡ Ibid.

§ THEOPHRASTUS and PLINY give the Indies as the country of barley.

‖ Sketches of the History of Man, i. 45.

¶ "In the East, it was in Babylonia, according to HERODOTUS and DIODORUS SICULUS, where the grains grew naturally,—the very place which may be regarded as the cradle of civilization."—DES LONGCHAMPS, in Dict. des Sciences Naturelles, tom. xix. Art. FROMENT.

tural operations to produce them is not apparent; and if it be supposed that the pastoral tribes of the second stage were agriculturists to a certain extent, so as to render the transition to the further cultivation of the soil a matter of slight change in habits, then the characterizing this mixed state as a purely pastoral one, is not in consonance with the stages theoretically marked out in the progress of civilization. But there is no evidence of the *Cerealia* growing spontaneously to any extent, even in those countries where the geographical range adapted to their culture has been considered most favourable. The discovery by botanists of plants of wheat, or barley, or oats, in particular situations, where men are or have been, is no evidence of spontaneous production. In Europe none of the *Cerealia* are found growing spontaneously; and even where seeds have been left on the fields by accident, two or three years has been ascertained to be the limit of reproduction, at least as to wheat and barley. A single unfavourable season—a premature frost (as M. Dureau de la Malle observes)—might be sufficient to destroy the uncultivated and ungathered grains in the greater part of Europe, and the species be exterminated, were not the seeds collected and preserved by human care. Similar atmospheric or other causes may act upon the *Cerealia* wherever cultivated, and hence the failure of observers to recognise the country where these plants, so necessary to man, grow in spontaneous luxuriance.

M. Dureau de la Malle has besides remarked, that to ascertain the fact of the cultivated grains growing spontaneously, would require the observation of years. In the Bois de Boulogne, for instance, where some of the allied troops had bivouacked, the seeds of the oat vegetated and reproduced from 1815 to 1819. And travellers, finding wheat or barley growing, from a similar cause, in countries near the supposed natural habitat, or the places of their original cultivation, might be wrong in recording, from one observation, that peculiar locality as the native country of the plant. Nay, more than this, the evidence of even years of reproduction would scarcely be sufficient to establish a country as the native or original one of any of the cultivated grains; for in the neighbourhood of Buenos Ayres, the oat, introduced by Europeans, has been said to reproduce itself for more than forty years. This statement is made on the authority of M. Aug. de St Hilaire, who for six years witnessed the fact.

Of the identity of the presently cultivated species of wheat and barley with the grains known under these names by the ancients, there is no sort of doubt; for, by the preservation of these grains in the monuments of Egypt, the fact has been ascertained beyond all question. The wheat preserved in vessels found in the Tombs of the Kings at Thebes, and of which the colour and form are unaltered, appeared to M. Delille and others perfectly indentical with modern wheat.* The culture of this grain has in Egypt been continuous for ages; and the spikes of

* Ann. des Sciences Nat. ix. 61.

wheat sculptured upon the Zodiacs of Thebes and Esné, are apparently of the same species as at present cultivated. In the bread found in the tombs of Upper Egypt, Mr Brown found many grains of barley entire, and perfectly similar to those of the present day. This fact is corroborated by other observers. M. Raspail having examined specimens of the grains found by M. Passalacqua in an Egyptian tomb, ascertained them to be the *Triticum vulgare* and the *Hordeum vulgare* of modern botanists. The grains in this case appear to have been partly roasted, were of a reddish colour, and larger than the European wheat.* For three thousand years, then, it is proved that these grains have undergone no perceptible change; so that all the theoretical speculations of fanciful writers as to the improvement of the Cerealia by the cultivation of ages, and the loss of the original type, fall to the ground, and the merit of "converting a sterile herb into corn," remains with the inventor of the tale.

M. Dureau de la Malle concludes, that, besides the valley of the Jordan, the chain of Lebanon, or the portion of Palestine and Syria which adjoins Arabia, ought to be considered, with great probability, as the native country of the Cerealia.

That the valley of the Jordan, and the districts now mentioned, are places where the Cerealia were cultivated in ancient times, is at once conceded; because they are countries which were early tenanted by families of men. But that the valley of the Jordan, or Palestine, were the sole places from whence the cultivated grains emanated, is not warranted from any source but the traditions of the Egyptian priesthood, retailed by historians. The true history of the Cerealia, similar to what has been stated as to the domestication of cattle, may be traced to a more distant period, and to a higher source, than the Isis of the Egyptians and the banks of the Jordan. I have no hesitation in stating, upon the authority of the most sacred of all records, and that statement is corroborated in every particular by the facts of history, the observations of naturalists, and the nature of things, that the knowledge and cultivation of the Cerealia must have been communicated to the first man. Far from the hunting state—or the pastoral state—being the earliest or the most natural state of man, it is declared in Scripture, in express terms, that man's first occupation was to "dress and keep" the garden in which he was placed; and after the fall, his eldest son was an agriculturist—"a tiller of the ground." There is no gradation of savage to barbarous—from barbarous to civilized life; no evidence of brute intelligence, to be improved in after ages to the height of reason, and the moral responsibilities of an intelligent being. Man was created a being as perfect as any of his future race; and however the knowledge of his descendants was to

* Mém. du Mus. d'Hist. Nat. xv. 145.—The wheat grown in Abyssinia is, according to Bruce, smaller than the Egyptian wheat.

be acquired, from infancy to manhood, from his example and instruction,—*his* knowledge was the immediate gift of God to the first of his creatures.*

The negative proof, then, of naturalists being unable to refer to any particular country where the Cerealia grow spontaneously,—the positive evidence that the wheat and barley of the ancients were precisely of the same species as those now cultivated,—and the fact that these grains have never been found wild or growing spontaneously, but in places frequented by man, lead to the conclusion, that the knowledge and cultivation of the Cerealia were coëval with man's existence. It may be mentioned, besides, as strongly indicative of the fact that the Cerealia are nowhere found in quantity but where man has carried them in his progress, that in the Lists of Plants stated as indigenous in the Levant, almost all the genera *Triticum* and *Secale* are found; while in South America, MM. HUMBOLDT and BONPLAND found no species of wheat (*Triticum*), and only one species of barley (*Hordeum ascendens*). This fact would also indicate, that the early wanderers from the Old Continent had been driven thither by accident, or extended their families in circumstances which prevented them from carrying with them the cattle or the grains of their forefathers. Unlike many other plants with a circumscribed geographical range, wheat, barley, oats, and rye, are found in almost every place where there are tribes of men. And it is farther a curious and unaccountable circumstance, except in one view, that these grains are never found in a wild state, available to any extent for the purposes of man. Their continuance depends upon their cultivation. Everywhere they are found to die out, if left to the spontaneous care of nature. Even in modern agriculture, and over most of Europe, a change of seed is occasionally necessary to ensure good crops; and the business of the farmer is a kind of continued experiment upon the soil he cultivates, to stop the retrograde tendency of the grains to become less prolific. This remarkable fact verifies in a striking manner the truth of that denunciation passed on the father of our race, "*In the sweat of thy face shalt thou eat bread*;" and affords another instance of the coincidence which students of nature are often obliged to remark between what is taught in the Book of Nature, and the Book of Revelation.†

* The Cerealia seem to be particularly indicated in the following passage of Genesis:—"Behold I have given you *every herb bearing seed*, which is upon the face of all the earth;"—and to distinguish these seed-bearing herbs from trees, the latter are mentioned by name in what follows:—"and every tree in which is the fruit of a tree yielding seed; to you it shall be for meat." (Gen. i. 29.) And connecting this with what is related in a subsequent passage, when the denunciation was passed on our race—"In the sweat of thy face shalt thou eat bread," it appears almost certain that the Cerealia were known to man from his origin.

† The sire of gods and men, with hard decrees,
Forbids our plenty to be bought with ease,
And wills that mortal man, inured to toil,
Should exercise, with pains, the grudging soil.

VIRGIL, *Georg.* B.

But even supposing, with some philosophers, that Man at his creation had to acquire by slow experience a knowledge of the objects around him, what could his boasted reason, exerted for the first time, have taught him, either regarding the taming of cattle, or the cultivation of the Cerealia? How could he, *a priori*, know that, by burying in the ground the ripened seeds springing spontaneously for his use, they would reproduce their seeds again, increased twenty-fold for one? The thing is impossible. Accident, in the course of years, might have thrown such an instance in the way of a rational creature trained to observation from infancy, and reflection might have suggested analogies between the annual reproduction of fruits and that of grains; but long years of experiment must have retarded the general acquisition of such knowledge. Neither is it very evident how individuals, living on fruits of much superior size to the cultivated grains, could first come to the knowledge that the minute seeds of the grasses might be made available for food. And it is more surprising still, that these early agriculturists should fix at once upon all the available Cerealia, proper for the food of man, and leave nothing to be added to the stock of cultivated grains, by the ingenuity of the thousands of generations who have succeeded them. "All animals," says Lord Monboddo, "are directed by instinct to search for, to find out, and to make use of the food which Nature has provided for them. But it has not directed nor instructed them to multiply that food, and to make the earth produce more than it naturally produces. In other words, instinct does not teach us to till, sow, or plant."* If the first man had been created a savage, and left to the acquisition of knowledge by his own unaided efforts, ages might have elapsed and found him a savage still. And without the aid of cultivated grains from the commencement of his progress, and a knowledge of their use as food, the population of the globe would have been limited to scattered tribes of rude savages, scantily extended over the wastes of creation.

It is besides a strong presumption of the truth of the views now submitted as to the domestic animals, and the knowledge of the cultivation of the Cerealia being the gift of his Maker to the first man, that in all the traditions of the ancient nations, the discovery of the grains—the domestication of cattle—the invention of writing, &c. are specially referred to their divinities or divine benefactors in the earliest periods of the world. The worship of Isis in Egypt, and the rites of Ceres in Greece and Rome, have reference to knowledge communicated to the human race by means beyond human; and thus the scattered traditions of distant nations not only afford evidence of man's origin from a common stock, but confirm in a singular manner the recorded facts of his early history.

Having thus shewn, as far as the limits of a single paper permitted me, that man was not created a mute savage, but a rational and intelligent being, endowed by his Maker with all the attributes of man in his best estate, it remains to be

* Origin and Progress of Language, vol. i. Book ii. p. 273–4.

accounted for how the race should have declined to barbarism and savage life. Between the period of the Creation and the Deluge, no facts are recorded concerning individual portions of the race, with the exception of one family. Beyond that family "all flesh had corrupted their ways." Whether a physical degradation accompanied this moral declension, is not apparent; but as far as regards the civilization of the race, and the knowledge of the arts previously practised, the survivors of the Deluge were the depositaries and the examples to their future descendants. That their situation was not one of savage barbarity, nor of feeding upon acorns, or hunting wild animals, is evident from the Sacred Record. And it is fairly presumable, that all the arts and sciences of the antediluvians—their knowledge of the true God and his worship,—were the property of the second progenitors of the human race, and communicated to their descendants.

What the state of the human race was at this time, is apparent from the Sacred Record. Agriculture, horticulture—the vine and the olive—flocks and herds,—were the known resources of the children of Noah; and however from soil, climate, or relative situation, it became necessary for particular families, his descendants, to choose the pastoral or agricultural life as their chief employment or means of support, yet the adoption of the one did not necessarily exclude the knowledge or practice of the other. No one, from finding a country more fit for pasturage than tillage, necessarily excludes from his mind or practice, the arts by which grains are raised and food provided; on the contrary it is presumable, and indeed certain, that even the pastoral tribes raised a certain quantity of corn for the supply of their families and flocks.* Besides, there are large portions of the globe unfitted for the operations of agriculture. The Peninsula of Arabia, abounding in vast sandy deserts, is almost wholly occupied by nomade races; and Central Asia, for the most part a bare table land, without forests, has been inhabited by wandering tribes of pastoral people from the commencement of history. The nature of the country fixes the wandering and pastoral mode of life upon the races inhabiting such districts; and when the want of irrigation and other causes renders the raising of crops to any extent impossible, pasturage over immense districts is the necessary and chief occupation of the inhabitants.

The Book of Job, written, it is conjectured, centuries before the time of Moses, and at least a thousand years before the poems of Homer and Hesiod, may be referred to as evidence of the state of civilization and knowledge at that early period of the history of Man. The vine and the olive were cultivated (xv. 33.); the ground ploughed for the growth of corn (i. 14.); metals used for domestic purposes (xxxvii. 18.); the horse trained for war; musical instruments were in use; written characters employed (xix. 23, 24.), and astronomy studied. The evidences of a highly civilized society are prominent in all the details of this vivid picture of ancient manners; and as to its own composition, according to Dr Mason Good,

* Genesis. xxiv. 32.

"nothing can be purer than its morality, nothing sublimer than its philosophy, nothing simpler than its ritual, nothing more majestic than its creed."*

The state o fsociety after the Deluge may thus be considered as one in a comparatively high degree of civilization. Soon after that event, the multiplied descendants of the patriarch combined to erect a city and a tower; and his great-grandsons, Nimrod and Asshur, are recorded as the founders of large cities,† the ruins of which now remain, a testimony of the truth of the Sacred Record. There is here therefore no grounds for the supposition that the race were savage, or had risen from savage progenitors. The arts, the animals, and the grains of former ages, were the property of the descendants of Noah, who dwelt in the plain of Shinar; and from this starting-place are to be traced, in the traditions, histories, and monuments of the race, the dispersion of the various families who were to people the most distant quarters of the world.‡

The mode in which the human race spread over the world after this period—the routes pursued by the various families—and the foundation of cities and civilized governments,—it is no part of my intention, even if I were qualified for the task, to enter upon. I confine myself to establishing the propositions, That man was at his creation a civilized being, endowed with all the physical and intellectual powers necessary to his state as a moral and responsible agent, and requisite to enable him to acquire a more intimate knowledge of the creation around him: That the domestic animals, created for his use, were his companions, and obedient to his will, from the beginning: That the cultivation of the Cerealia was the earliest occupation of the human race: That, prior to the Deluge, the human race had planted cities and practised many of the more useful arts: and, That the survivors of the Deluge started with all the knowledge of their predecessors—the possession of the domesticated animals, and the grains necessary to their processes of agriculture. These propositions being granted, it belongs to the philosopher or statesman, to trace the causes, physical and moral, which have reduced the descendants of a highly civilized people to the degraded state in which many tribes of men are now found. Is there a downward tendency in the constitution of man?—Have nations, like individuals, their beginning, their increase, and their end—their rise and fall? Does history and observation demonstrate, that the

* The Book of Job literally translated, by J. Mason Good, F.R.S. Introd. p. i.

† Genesis, xi. 4.—x. 10.

‡ "The boundless riches of the Babylonian fields gave birth," says Mr Alison, "even in the first ages, to those stupendous cities from whence the enterprise of commerce dispersed the human race in every direction through central Asia; while the uniform pasturage of the Scythian wilds spread before them a vast highway stored with food, by means of which they could penetrate with ease to the remotest extremities of the old world."—Alison on Population, i. 22.

"Supposing Babel or Babylon to have been the centre of irradiation—how easy was the transit for Ham's descendants into Africa by the Isthmus of Suez; into Europe the path was still more open for those of Japhet; and as the stream of population spread to the east, the passage to America was not difficult to those who had arrived at Behring's Straits."—Kirby's Bridgewater Treatise, i. 76.

tendency of the human race is in most circumstances to degenerate from the civilized to the savage state—in place of rising from savage to civilized life? Whatever be the elements of this retrograde progress, there is no doubt of the fact. All history is full of instances, of regions occupied by civilized people, being now the abodes of hordes of barbarians. The almost obliterated remains of Babylon—the ruins of Thebes—the monuments of Egypt—the total disappearance of the ancient commercial republics of Carthage and her allies—even the ruins of Athens and Rome—teach the lesson, that neither science nor art, neither philosophy nor religion, have been hitherto effective in stopping this downward progress—this descent to barbarism and savage life.

With regard to the progress of this degradation, it must necessarily have been gradual, as to the first emigrants from the centre of civilization and knowledge. Removed by choice or from necessity, as their numbers augmented, to greater distances in the yet unpeopled wastes, much of the original knowledge in the arts might have been lost, from no call being made in their circumstances for their use. The occupations of the new settlers would naturally depend much upon the nature of the country and climate in which they found themselves eventually placed. And it is no stretch of imagination to suppose, that many of these scattered families or tribes, in their migrations through unpeopled wastes, might gradually lose much of the knowledge of their forefathers. It is besides not improbable, that many parties of the earlier wanderers, cut off by accidental circumstances easily supposed—driven out to sea or carried down a river in their primitive boats—might in many cases be transported beyond reach of communication with other families of their race; and thus, deprived of the domesticated animals and the use of the grains, degenerate into a ruder and more savage mode of life.* That in some such way as this the American Continent has been peopled is rendered pro-

* "Very few of the numerous coral islets and volcanoes of the vast Pacific, capable of sustaining a few families of men, have been found untenanted; and we have therefore to inquire whence and by what means, if all the members of the great human family have had one common source, could these savages have migrated. Cook, Forster, and others, have remarked that parties of savages in their canoes must often have lost their way, and must have been driven on distant shores, where they were forced to remain, deprived both of the means and of the requisite intelligence for returning to their own country. Thus Captain Cook found on the island of Wateoo three inhabitants of Otaheite, who had been drifted thither in a canoe, although the distance between the two isles is 550 miles. In 1696, two canoes, containing thirty persons, who had left Ancorso, were thrown by contrary winds and storms on the island of Samar, one of the Philippines, at a distance of 800 miles. In 1721 two canoes, one of which contained twenty-four, and the other six persons, men, women, and children, were drifted from an island called Farroilep to the island of Guaham, one of the Marians, a distance of 200 miles."—Lyell's Principles of Geology, iii. 157.

Kotzebue mentions an instance of four persons being drifted in an open boat to the distance of 1500 miles. Captain Bligh with eighteen persons in an open boat traversed, in forty-one days, a distance of 3618 miles, from near Otaheite to Timor in the Indian Ocean; and a number of other instances might be mentioned from the narratives of travellers, of the spread of the race in circumstances where the knowledge and habits of civilized life might be so far lost in the necessities of their situation.

bable, from the circumstance that the horse and the ox were not known in America till a late period—that at the point where the continents approach, and strolling hunters might pass from the one into the other, there are some animals common to both—and that all the traditions of the different tribes refer to ancestors more civilized, from whom they have descended.

The numerous remains of a more civilized people than the present races in North and South America—remains of the same nature in the wilds of Tartary and Siberia—the tombs, the tumuli, and fragments of ancient art, found all over both continents,—demonstrate not only the identity of the races, but the degradation that has followed since their original settlements.

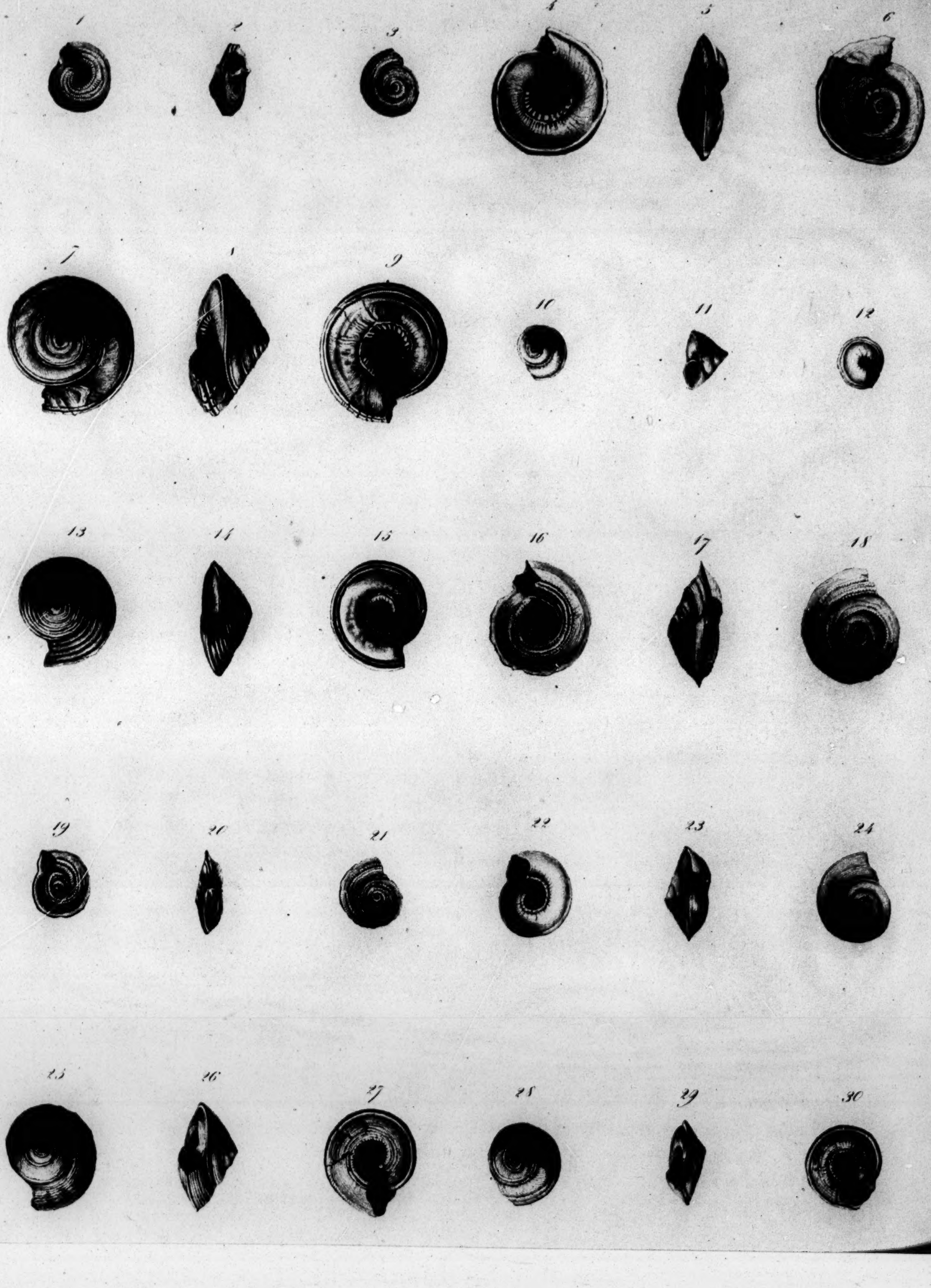
"The barbarism that prevails throughout these different regions," says HUMBOLDT, "is perhaps less owing to a primitive absence of all civilization, than to the effects of a long degradation. The greater part of the hordes which we designate under the name of savages, descend probably from nations more advanced in cultivation."* And in another place he says, "Savages are for the most part degraded races, remnants escaped from a common shipwreck, as their languages, their cosmogonic fables, and a crowd of other indications seem to prove."

If such has been unquestionably the downward progress of the human race in all past ages, and in regard to even the most civilized and greatest communities which have ever existed, the cause of that declension is a subject for the deepest consideration, in regard to the stability of our own unparalleled state of social life. What is calculated, in our case, to arrest us in the climax of our national greatness—to stop the flowing tide that has swept away the arts and civilization of every former people? At the pinnacle of power, "beyond all Greek, beyond all Roman name," is it inevitably necessary that we should decline!—that our sun should go down as theirs—and all our arts and sciences, and improvements, be lost in a flood of barbarism! It is the business of the philosopher, the duty of the statesman—the object of all—to inquire into the causes of the apparently fated decline of all human communities; and to ascertain whether moral degradation, like the same cause among the antediluvians, may not be the forerunner of national ruin.

* Personal Narrative, iii. 208. "How can we distinguish the prolonged infancy of the human race," says HUMBOLDT, "if it anywhere exists, from that state of moral degradation in which solitariness, want, compulsory misery, forced migrations, or the rigour of the climate, obliterate even the traces of civilization? If every thing which is connected with the primitive state of man, and the first population of a continent, could from its nature belong to the domain of history, we should appeal to the traditions of India, to that opinion so often expressed in the laws of Menore and in the Ramajan, which considers savages as tribes banished from civil society, and driven into the forests."—HUMBOLDT's Personal Narrative, iii. 203.

NOTE.—Since the preceding remarks were submitted to the Society, a Memoir, by M. J. J. VIREY, entitled "Des Causes Physiologiques de la Sociabilité chez Animaux, et de la Civilization dans l'Homme," has been read before the Royal Academy of Medicine at Paris. An abstract of this paper is given in the "Bulletin de l'Académie Royale de Médecine" for February last. M. VIREY, in this memoir, follows the classical theory of the gradual progress of society from the savage state to civilized life; asserts intellectual superiority to be the universal cause of subordination; and, with regard to the domestic animals, endeavours to shew that debility of constitution is the physiological cause of their submission to man. As none of the considerations adverted to by this gentleman affect the statements I have submitted to the Royal Society, I have not considered it necessary to notice them further.

PLATE II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XII.—*De Solariis in Supracretaceis Italiæ Stratis repertis.* Auctore JOANNE MICHELOTTI.

Abdita quid prodest generosi vena metalli
Si cultore caret? LUCAN. *Carm. in Pison.*

(Read 15th March 1841.)

MULTIPLICES *Solariorum* species, genus testaceorum constituentes, ad *Turbinacea* LAMARCK, ad *Gasteropoda* CUVIER, pertinens, quæ in geologicis Italiæ stratis reperiuntur, monographico modo investigare fortasse præstat. Ratio nec ambigua; variæ enim dubitandi rationes vigebant, tum circa numerum et nomina specierum, tum circa earundem citationes, et auctorum dissensiones, nec non potissimum circa relationes specierum quæ in Italicis divisionibus *supracretaceæ* formationis adsunt, sive in se spectatis, sive prout cum viventibus conveniunt aut discrepant.

Hisce ambagibus præcludendi viam ratus, ubi hujus pulcherrimi generis specierum enucleationem componerem, ad hoc animum revolvi, historiam ejusdem prorsus omittens, quoniam eam persolverunt viri clarissimi T. et G. B. SOWERBY,* DES HAYES,† KIENER.‡

Quum autem in qualibet *monographia* icones maxima præbeant subsidia, atque ea ex natura et oculari inspectione depromere oporteat (quod optimus amicus, atque zoologiæ cultor HENRICUS MELLA, mihi præstitit), itaque eas solummodo species enumeravi, quas speciatim conspicere potui, magisque vulgatas in Taurinensibus collectionibus esse comperi; nec inutilem prorsum laborem suscepisse ratus, ubi et insignis hujus Regiæ Societatis favor, et naturalis historiæ cultorum indulgentia atque humanitas accedat.

Dabam, 22. Mense Majo 1840. Aug. Taurinorum.

Spec. N. 1. SOLARIUM STRAMINEUM, *Lamarck.* Nobis fig. 1, 2, 3. Tab. II.

Testa orbiculato-convexa, tranversim sulcata, longitudinaliter striata; ultimo anfractu ad peripheriam planulato-bisulcato; umbilico patulo, leviter crenulato; sutura canaliculata.

Habitat penes Tranquebar, et in Mari Mediterraneo.

* The Mineral Conchology of Great Britain,—Genera of Shells.

† Coquill. Foss. des environs de Paris, vol. ii. pag. 212.

‡ Collect. des Coq. viv. cahier de *Cadran.*

Fossile in agro Vicentino, Parmensi, Astensi: specimina parva: item in collibus Dertonensibus, specimina majora sed rariora.

GMELIN, Syst. Nat., pag. 3575. Trochus stramineus.
CHEMNITZ, Conch. Cab. vol. v. tab. 172, fig. 1699.
LISTER, Conch. tab. 635, fig. 23.
BRANDER, Foss. Hanton. pag. 10. tab. 1, fig. 78.
SOLDANI, Saggio, tav. x. fig. 61.
LAMARCK, Anim. s. Vert. vol. vii. pag. 4. Solarium stramineum.
BROCCHI, Conch. Foss. Subap. vol. ii. pag. 359. Solarlum canaliculatum.
SOWERBY, Miner. Conch. vol. vi. pag. 43, tab. 524, fig. 1.
DEFRANCE, Dict. des Scienc. Natur. tom. 55, pag. 485.
DES HAYES, Coq. Foss. tom. ii. pag. 220.
PHILIPPI, Enum. Mollusch. Sic. pag. 173. Solar. stramineum et Sol. canaliculatum.
BRONN. Lethaea Geogn. pag. 1039.
KIENER, Spec. Général des Coq. viv. 28, Liv. genre Cadran, pag. 11, tab. iii. 4.

Hæc species trochiformis est, spiram gerit depressam: ejus anfractus numero quinque, leviter convexi sunt, sutura aliquantulum canaliculata sejuncti; ultimus subrotundus; duobus cingulis marginalibus tertioque minore ad peripheriam instructus: sulci transversi, striis longitudinalibus decussati, indeque granulati; umbilicus leviter crenulatus, ad latera subcanaliculatus; carina mediana conspicua.

Quatuor species recenset clariss. LEA, in opere cui titulus, Contributions to Geology, nominibus Solarium Henrici, Sol. ornatum, Sol. elegans, Sol. cancellatum, quos varietates tantum Solarii straminei esse autumo.

Spec. N. 2. SOLARIUM PSEUDO-PERSPECTIVUM, *Brocchi*. Nobis, fig. 4, 5, 6. Tab. II.

Testa orbiculata, subconvexa, lævi; anfractibus margine exteriore acuto, superne bisulcato, subtus sulco unico, umbilicum amplum, plicato-crenatum cingente; apertura depressa.

Habitat ——

Fossile in Etruria, in agro Boroniensi; Parmensi, atque collibus Dertonensibus.

BROCCHI, Conch. Foss. Subap. pag. 359, tav. 5, fig. 18, *a. b.*
BORSON, Sagg. Oritt. pag. 88.
DEFRANCE, Dict. des Scienc. Natur. vol. 55, pag. 488. [*Solarium complanatum.*
BRONN, Italiens tert. Gebilde, pag. 62.
PHILIPPI, Enum. Mollusch. Sic. pag. 174.

De hac specie verba faciens clar. BROCCHI ait, Dubitare se num in *Solario hybrido* quidpiam consentaneum *Solario pseudo-perspectivo* adsit; verumtamen, sive forma conoidea quam apertura, et sulcorum dispositiones *Solarii hybridi*, satis

superque ostendunt maxumam differentiam inter *Solarium pseudo-perspectivum* et *Solarium hybridum* intercedere.

Clariss. Bronn, in suo Indice Testaceorum Italiæ, proprium nomen vocabulo a Brocchi adhibito ad hanc speciem indicandam adjicere posse autumavit; quam agendi methodum modis omnibus impugnare oportet. Primo enim Linnæana nomina Linnæanis speciebus adjicimus, licet generibus adauctis; ita non absimili modo Brocchi auctoritatem præ oculis habere debemus, ubi de iis agatur speciebus quæ eundem laudant auctorem.

Hisce alia accedunt: nemo est qui ignoret Brocchi, in pretiosissimo suo opere Testaceorum Italiæ (ut claris. Anglici scriptoris voce utar*), indicasse genera sive Linnæi, sive Lamarckii in speciebus ab eodem enumeratis, nihil itaque erat cur nostra ætate Risso, Bronn aliique, Brocchi auctoritatem expungant ut propriam apponant. Neque hic evanescit difficultas: nostrum est specierum et scriptorum auctoritatem firmam tenere, neque ita partitiones jactare ut temporis progressu nihil certi remaneat.

Spec. N. 3. Solarium luteum, *Lam.* Nobis, fig. 10, 11, 12. Tab. II.

Testa parvula, orbiculato-conoidea, ad peripheriam bisulcata, ultimo anfractu ad marginem inferne bicingulato, lævigato; umbilico angusto, crenis sulco discretis.

Habitat in maribus Novæ Hollandiæ et penes Messinam.

Fossile in collibus Taurinensibus, raro.

Lamarck, Anim. sans Vertebr. vol. vii. p. 5. viv.
Philippi Enum. Mollusch. Sic. pag. 174, N. 2, tab. 10, fig. 22.
Bronn, Lethaea Geogn. pag. 1047.
Kiener, Coll. Coq. viv. pag. 9, tab. iv. fig. 9.

Anfractus hujus speciei sunt convexiusculi, lævigati; peripheria duobus cingulis distincta; facies inferior vix convexa, lævissima, umbilici margine sulco parum profundo sejuncto. Operculum tenuissimum est, corneum, multispiratum.

Nescio qua de causa in stratis supracretaceis superioribus nec adhuc reperta est, quum ipse eam in collibus Taurinensibus invenerim.

Differentia est et extat inter icones quos præbet clar. Kiener, et quos exhibet claris. Philippi; specialem posterioris scriptoribus cognitionem sequi malo.

Spec. N. 4. Solarium neglectum, *mihi.* Fig. 7, 8, 9. Tab. II.

Testa orbiculato-conoidea, apice obtuso; anfractibus convexiusculis, lævigatis, ad suturam tribus sulcis granulosis instructis; ultimo ad peripheriam angulato-rotundato; basi lævigata, umbilico mediocri, margine crenato, crenis sulco distinctis; apertura mediocri, depressa.

* Mantell, The Wonders of Geology, vol. i. pag. 193.

Habitat ——

Fossile in colle Taurinensi, raro; in agro Astensi, Parmensi, frequenter.

BON. Cat. del Muz. Zool. di Torino, M. S. n. 571. *Solarium sulcatum.*
PUSCH, Polons Palæont. tab. x. fig. 11, *a. b.*

Hæc species ferme conica, circa sex anfractus ostendit, qui penes suturam tribus sulcis ferme æqualibus præditi sunt, atque in interstitiis adsunt duæ parvulæ costulæ granulosæ. Cæterum facies sive inferior quam superior lævigata est; peripheria marginis posterioris anfractus rotundata est, atque in ima facie duos sulcos cernimus; umbilicus vix elatus, atque ejus latera distincte canaliculata sunt; apertura aliquantulum depressa atque dilatata est.

Nonnullæ species huic affines eidem accedere videntur; inter quas potissimum numerabo *Solarium patulum* atque *Solarium caracollatum* LAMARCKII. Circa priorem speciem, quum nimis brevis ejusdem exaratio sit penes LAMARCK, quum eadem pluribus aliis conveniat speciebus, ideo ad descriptiones clar. vir. SOWERBY atque DES HAYES confugiendum.

De Solario patulo hæc habet SOWERBY: " The umbilicus is curiously and beautifully ornamented with a crenulated border, surrounded by a row or two of small denticulæ. The flattish disk-like surface swelling a little, has longitudinal striæ with more or less fine transverse marks over it. The outer angle of the shell is sharpest, the upper surface of the edge is milled, as it were, with oblique transverse striæ, causing small oblong risings like the oblique milled edges of guineas. The shell is also longitudinally striated beneath." (*Vide* Miner. Conch. of Great Brit. vol. i. p. 35). Ex sulcis longitudinalibus itaque, ex aperturæ indole, atque margine, metienda sunt quæ differentias indicant inter *Solarium sulcatum* et *Solarium neglectum*.

Verumtamen quum species *Solarii patuli* a LAMARCK descripta, ea prope Lutetiis inveniatur, idcirco confugere debemus ad descriptionem claris. DES HAYES ut ejus characteres cognoscamus: idem ait: " Ce cadran est orbiculaire, sa spire, plus ou moins élevée, est toujours obtuse au sommet; les tours qui la composent sont au nombre de neuf ou dix; ils sont étroits, ordinairement aplatis, quelquefois un peu concaves transversalement. Ils sont lisses, et cependant le dernier examiné à la loupe offre quelques striés transverses obsolètes. La suture est superficielle, très-fine; elle est bordée en dessus de granulations très-fines, et on remarque quelquefois au dessous de petits plis longitudinaux. La circonférence du dernier tour est fort aigue, carénée; la base de la coquille est largement ouverte par un ombilic simple et continu, dont le bord interne, non saillant, est couronné par un rang de granulations, qui s'effacent presque entièrement dans quelques individus. L'ouverture est un peu oblique à l'axe; elle est petite, quadrangulaire; ses côtés sont presque égaux: ses bords," &c.

Patet igitur, sive ex numero anfractuum, eorumque suturis, et depressione,

nec non ex peripheria et apertura in *Solario patulo*, distinctiones adesse inter *Solarium neglectum* et *Solarium patulum*.

Claris. Pusch, in egregio opere circa fossilia Poloniæ, varietatem *Solarii caracollati* agnoscere credidit in *Solario neglecto;* sed minus apte, sulcorum enim præsentia vel defectus alicujus momenti est: hoc fundamento innixi, Lamarck, Sowerby, Des Hayes, aliique, species constituerunt; alioquin vero vel ipse Pusch discrimen inter *Solarium carocollatum*, et *Solarium umbrosum* Brongniart admisisse constat; ergo potiore jure discrimen inter *Solarium neglectum*, et *S. caracollatum* admittendum: adde, in *Solario neglecto* strias longitudinales deficere, tres sulcos penes anfractuum suturas extare, granulationibus interpositis, basimque depressam esse.

Celeb. Bonelli in hac specie *Solarium sulcatum* Lamarck agnoscere credidit, quam speciem in agro Parisiensi tantummodo reperiri Lamarck scripsit; licet autem et Des Hayes altum de hac specie silentium servet, notare juvabit, genericam nimis Lamarckii definitionem esse, atque, hac seposita, ex peripheria, anfractuum suturis, differentia existere.

Spec. N. 5. Solarium ambrosum, *Brongniart*. Nobis 13, 14, 15. Tab. II.

Testa orbiculato-depressa, subdiscoidea; anfractibus planis, sutura canaliculata discretis, sulcis transversis, profundis; ultimo anfractu ad peripheriam obtuse angulato, angulo utrimque marginato: umbilico magno canaliculato, late crenato; crenis sulco distinctis.

Habitat ——

Fossile penes Sultia, raro; in colle Taurinensi, frequenter.

Brongniart, Mem. sur le Vicentin, pag. 57, tab. ii. fig. 12.
Bronn, Ital. tert. Geb. pag. 63.
De la Beche, Man. of Geol. tert. gr.

Hæc species ferme discoidea spiram gerit obtusam; anfractus numero quinque aliquantulum convexi, atque disjuncti sunt in vim cavitatis: externa facies pluribus sulcis instructa est, at in ultimo anfractu quinque tantum sunt: interstitia in supremis anfractubus sunt granulosa, in postremis lævigata. Margo obtuse angulosus, utrinque duobus sulcis profundis distinctus; umbilicus late patet, atque crenatus; crenis sulco profundo discretis; apertura magis lata, quam longa.

Clar. Brongniart, in opere citato, notat *Solarium plicatum* Lamarck magis accedere ad hanc speciem quam aliæ; verumtamen discriminis ratio non in eo ponenda quod in Solario umbroso granulationes desint; vidimus enim in superioribus anfractibus *Solarii umbrosi* earundem signa adesse, sed quia in *Solario umbroso* desunt longitudinales plicæ quas observamus in *Solario plicato:* præterea in *Solario plicato* sulcus dilatatus est circa granulationes, quum stricte et minus profunde pateat in *Solario umbroso*.

Neque relationes omnino deficiunt inter *Solarium umbrosum*, et *Solarium lævigatum;* sed ubi Kiener audiamus disserentem de *Solario lævigato*, liquido patet hujus speciei umbilicum coarctatum esse, ejusdemque granulationes parvas, et sulcum granulosum; præterea strias tantummodo, non sulcos, in superna facie adesse.

Sp. N. 6. Solarium millegranum, *Lamarck.* Nobis fig. 16, 17, 18. Tab. II.

Testa orbiculato-convexa, ad peripheriam compressa-angulata, scabra; striis sulcisque transversis, granulosis: inferna facie convexa, striis longitudinalibus, creberrimis; umbilico patulo, crenato.

Habitat ——

Fossile in agro Parmensi, frequens; in collibus Dertonensibus, raro; in colle Taurinensi, rariss.

Var. α variet. minor, periph. leviter sulcata.
Lamarck, Anim. sans Vert. vol. vii. pag. 6, n. 8.
Bronn, Ital. tert. Geb. pag. 64, n. 335.
Jan, Catal. pag. 6, n. 10.

Hæc species ferme conoidea sex anfractuus gerit, quorum priores aliquantisper planulati sunt, inferior convexus est, variis granulationum ordinibus, et qui in supremis minus patent: inferior facies pariter granulationibus gaudet, eædemque potiores circa umbilicum quam circa marginem: umbilicus est amplissimus; apertura subrotunda.

Spec. N. 7. Solarium pulchellum, *mihi.* Fig. 19, 20, 21. Tab. II.

Testa superne planulata; anfractibus sulcis granulosis, regularibus, ultimo ad peripheriam angulato-carinato, inferne convexo; sulcis transversis; interstitiis granulosis prædito; umbilico lato, margine crenato, crenis duplicatis.

Habitat ——

Fossile in collibus Dertonensibus, raro.

Hæc species valde præcedenti accedit, atque forsitan aliis inventis ejusdem varietas censebitur; verumtamen usque nunc talia discrimina adsunt, ut non dubitem novum eidem tribuere nomen, in vim potissimum quod omnino superne planulata sit, quod in superna facie sulci longitudinales omnino deficiant, quod interior canalis minus elatus sit, atque peripheria minus acute carinata.

Spec. N. 8. Solarium canaliculatum, *Lamarck.* Nobis Fig. 25, 26, 27. Tab. II.

Testa orbiculato-conoidea, apice obtuso; anfractibus convexiusculis cingulatis; cingulis granosis, depressis, peripheria rotundato-carinata, transversim sul-

cata; facie inferna ad marginem canaliculata, sulcis longitudinalibus exarata; umbilico mediocri; apertura ampla, subquadrangulari.

Habitat ——

Fossile in agro Astensi, Parmensi; in Etruria, in Anglia.

Brander, Foss. Hant. pag. 10, tab. 1, fig. 7, 8.
Lamarck, Ann. du Mus. tom. iv. pag. 54, N. 3.
Brocchi Conch. Foss. Subap. vol. ii. pag. 360 (variet. *Solarii pseudo-perspectivi.*)
Lamarck, Anim. sans Vert. vol. vii. pag. 5, N. 3.
Borson, Mem. Orilt. vol. Accad. di Torino, pag. 89, N. 3.
Defrance, Diction. des Scienc. Natur. tom. lv. pag. 485.
Deshayes, Coq. Foss. vol. ii. pag. 220, N. 8. tab. xxiv. fig. 19, 20, 21.
Bonelli, Coll. Mus. Zool. M. S. *Solarium crenulosum.*

Hæc species ferme conoidea superius, inferne convexiuscula, superficies sulcis longitudinalibus et transversis utrinque signata; umbilicus mediocris, canali elevato; plicæ interiores nec adeo distinctæ, nec adeo frequentes, ut in *Solario millegrano.*

De hac specie sentiebat Brocchi, dun varietatem *Solarii pseudo-perspectivi* indicabat; hinc ille " cingulis argute crenulatis;" et alibi, " tutta la superficie e segnata da cordoncini piatti elegantemente cancellati," &c. Non itaque uti Bronn placuit de alia specie sentiebat.

Nuperrime cum optimo amico L. Bellardi hanc speciem enumeravimus, in memoria quam Regiæ Academiæ Scientiarum Taurinensibus voluminibus comprehensa est.

Spec. N. 9. Solarium Lyellii, *mihi.* Fig. 28, 29, 30. Tab. II.

Testa conica suturis granulosis, interstitiis sulcis longitudinalibus, obliquis prædítis; peripheria angulato-crenata; facie inferna subconvexa, sulcis transversis, striisque longitudinalibus; umbilico mediocri, crenato; crenis dilatatis, canali elato; apertura subrotundata.

Habitat ——

Fossile in collibus Dertonensibus, raro.

Mira sane species, possidens quatuor vel quinque anfractus, qui superias circa suturas granulosi sunt; cæteroquin variis sulcis longitudinalibus, confertis, deflexis instructi sunt; peripheria angulata, sulcis quoque transversis distincta: basis convexiuscula, umbilicum gerit mediocrem, ex quo crenulationum indicia ad peripheriam se protrahunt, et sulcos transversales secant; apertura ferme rotundata, depressa est.

Ex hisce liquet, in vim præcipue peripheriæ *Solarium variegatum* et *Solarium luteum* differre ab hac specie; in vim sulcorum et apertura differre a *Solario lævigato,* uti cæterorum adjuncta huic separationi comparate ad alias species favet.

Hanc speciem claris. Car. Lyell dicavi, cui aliquid ob benemerita ejusdem studia circa fossilia Italiæ pro viribus meis tribuendum esse duxi.

Spec. N. 10. SOLARIUM HUMILE, *mihi*. Fig. 22, 23, 24. Tab. II.

Testa superne subconica, inferne valde convexa: anfractibus superne costalis frequentibus, transversis, granulosis, æqualibus, suturis indistinctis; peripheria acutissima, inferne late canaliculata; umbilico mediocri, canali elevato: apertura trigona.

Habitat ——

Fossile in colle Taurinensi.

Hæc species possidet quatuor anfractus, qui superius planulati sunt, atque ita conjuncti ut vix ac ne vix quidem suturæ indicium habeas: in omni superficie adsunt tot costulæ transversim eleganter granulosæ, atque inter se æquales. Ultimi itidemque valde convexi anfracti peripheria est acutissima, sulco inferne late patente circumdata: ex hoc sulco usque ad umbilicum adsunt minuti sulci transversi; umbilicus mediocris est, creni crassiores, et apertura subtrigona.

Nuper recensitæ species probant duas tantummodo species cum viventibus convenire; sed altius res repetenda est; omnes enim quum pertineant ad *miocenica* vel *pliocenica strata*, patet tres species unice pertinere ad antiquiora strata, scilicet, *Sol. umbrosum, Sol. Lyellii, Sol. humile;* quatuor species communem habere sedem, scilicet, *Sol. neglectum, Sol. millegranum, Sol. stramineum, Sol. pseudo-perspectivum.* Illud insuper adjiciendum, unicam speciem quæ reperitur in stratis pliocenicis convenire cum viventibus, scilicet *Solarium luteum;* qui, licet nec adhuc repertum sit in stratis supracretaceis superioribus, tamen extitisse constat, quum et antiquitus viguerit, et hodie vivat.

Quum melioris notæ scriptores judicent ferme omnes hujus generis species in Indico mare degere, uti major testaceorum pars ad Gasteropoda pertinens, concludendum itaque est principium illud, quod clarissimi geologiæ cultores* posuerunt, in *miocenica* ætate feliciorem aërem in hisce Italiæ regionibus viguisse, sensimque eam mutationem prout ea hodie est secutam fuisse, jure meritoque defendi posse.

* LYELL'S Principles of Geology, vol. iv. WEBSTER, Geolog. Trans. SEDGWICK et MURCHISON, Proceedings Geol. Society of London, 4. ed. 1829. DESHAYES, Coquill. Foss. vol. ii. pag. 772. et seq. DE LA BECHE, Geol. Manual. PHILIPPS, Guide to Geology. LEA, Contribut. to Geology, pag. 14. MANTELL, Wonders of Geology, vol. i. pag. 83.

SEISMOMETER.

Fig. 1.

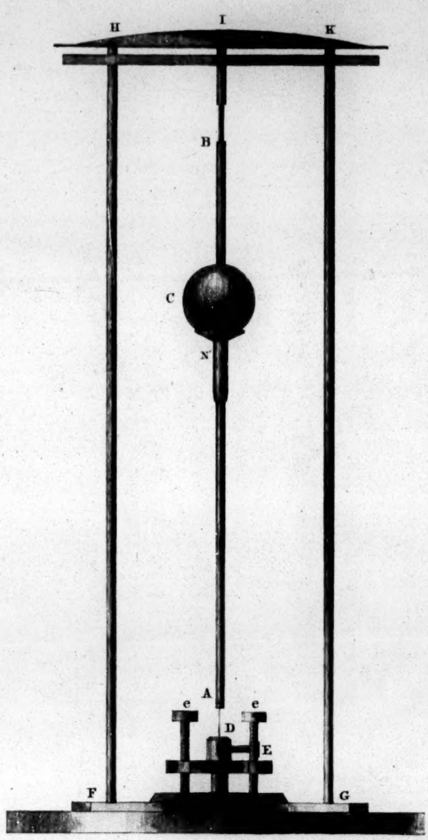


Fig. 2.

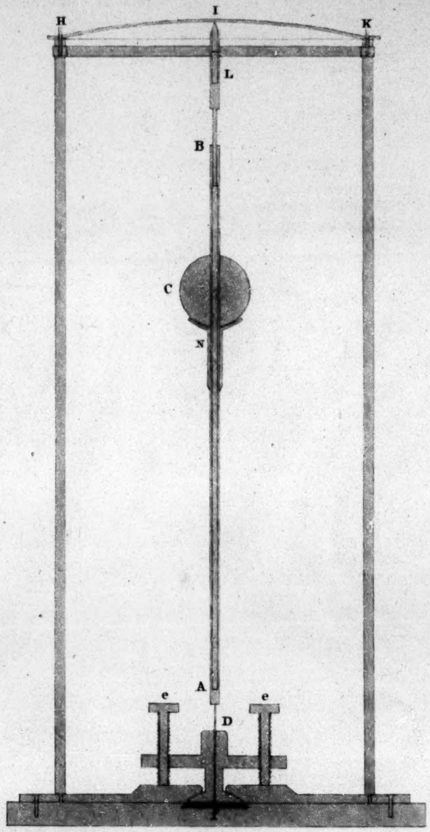


Fig. 3.

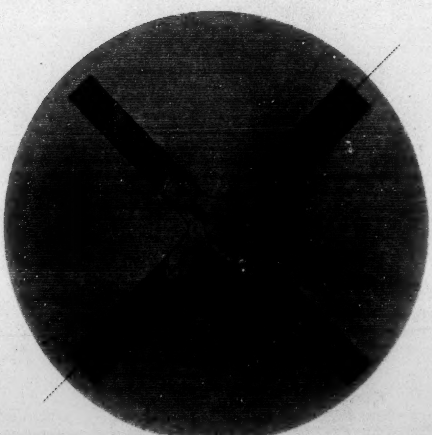
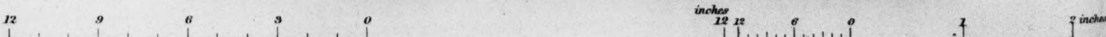


Fig. 4.



Scale to Plan Elevation & Section.

Scale to Enlarged view of Pencil piece.

J. R. Wilson Del.

XIII.—*On the Theory and Construction of a Seismometer, or Instrument for Measuring Earthquake Shocks, and other Concussions.* *By* JAMES D. FORBES, Esq. *F.R.S. Sec. R. S. Ed., Professor of Natural Philosophy in the University of Edinburgh.*

(Read 19th April 1841.)

HAVING been requested to act on a Committee of the British Association, appointed to devise and apply methods for measuring the comparative intensity of earthquake shocks, and having been shewn several ingenious contrivances by Mr DAVID MILNE (who suggested the inquiry) Lord GREENOCK, and other persons, an apparatus occurred to me which should unite the requisites of Simplicity, Compactness, Comparability, and an easy adjustment of Sensibility according to circumstances.

Mr MILNE had not failed to distinguish the ends for which instruments (which for obvious reasons were to be self-registering) ought to be devised, such as the measurement of horizontal concussions, of vertical elevation, and of heaving or angular motion of the surface. It is no part of my present object to consider the probable movements of the soil in earthquakes. I limit myself to the description of a single instrument intended to measure *lateral* shocks, such as are experienced by objects placed upon a table which is abruptly shoved forwards.

A heavy pendulum suspended from a frame in such a manner that the inertia of the bob should cause it to oscillate when its centre of suspension had been displaced by the movement of the frame with which it was connected, had already been suggested for the purpose. To obtain sufficient sensibility, a pendulum of great length would be required, nor could the sensibility be altered according to circumstances, being wholly independent of the weight of the bob. The unwieldiness of a pendulum ten or twenty feet long alone forms a strong objection to this apparatus.

The elegant inverted Pendulum or *Noddy* contrived by the late Mr HARDY,* suggested to me a different arrangement. The instrument is seen in Elevation, Section, and Plan, in Plate III. Figures 1, 2, and 3. A vertical metal-rod A B, having a ball of lead C moveable upon it, is supported upon a cylindrical steel-wire D, which is capable of being made more or less stiff by pinching it at a shorter or

* Described by Captain Kater, Phil. Trans. 1818.

greater length by means of the screw E. It is evident that, by adjusting the stiffness of the wire, or the height of the ball C, we may alter to any extent the relation of the forces of Elasticity and of Gravity, and consequently render the equilibrium of the machine in a vertical position stable, indifferent, or unstable. Since, then, a lateral movement, which carries forward the base of the machine, can only act upon the matter in C through the medium of the elasticity of the wire, the stiffness being diminished, or the weight increased, the tendency of the rod to right itself may be diminished in any proportion, and that irrespectively of the dimensions of the instrument.

The wire D being cylindrical, the direction of the displacement occasioning the shock will at once be indicated by the plane of vibration of the pendulum, which, being once put in motion, will oscillate backwards and forwards many times before coming to rest. The pendulum is adjusted to the vertical position by four antagonist screws *e e e e*, acting on a ball and socket arrangement *f*.

The self-registering part of the apparatus, which Mr David Milne has termed a *Seismometer*, was arranged by that gentleman and by Mr James Milne, the ingenious artist who constructed it. It consists of a spherical segment H I K of copper lined with paper, against which a pencil L, inserted in the top of the pendulum-rod, is gently pressed by a spiral spring. The marks thus traced on the concave surface indicate at once the direction and maximum extent of the pendulum's vibration. The arrangement of the pencil is seen upon a larger scale in Fig. 4, where L is the pencil as before loosely fitting the cylinder *b c*, and pressed upwards by the spring *a*. The whole pencil-case moves stiffly on the extremity B of the pendulum-rod, so as to adjust the pressure against the paper.

Hardy's instrument was intended simply for ascertaining the stability of the support for a clock. The spring was a piece of flat watch-spring—the plane of its motion was parallel to that of the pendulum of the clock whose influence was suspected, and the time of oscillation being adjusted accurately to seconds by screwing the bob up or down, the repetition of impulses always isochronal, though individually feeble, at length urged it into considerable arcs of vibration, if the beam or wall on which it stood was not perfectly stable. The instrument under consideration, on the other hand, has a free vibration in every vertical plane, the time of its oscillation is immaterial, except in so far as the sensibility is increased as the time is greater; Hardy's instrument collects the effect of a series of isochronous impulses, this one registers the maximum effect of a single and insulated one in direction and in intensity: Hardy's was an indicator of instability, this (as we shall see) furnishes a measure of the cause of a concussion.

The admirable advantage which the balance of the gravitating and elastic forces affords will appear from the following considerations:—

I. We must first attend to the *friction* which must be overcome in order to carry the pencil across the surface receiving the trace. The moving force of the

pendulum will be greater as its inertia increases, in consequence of which the bob lags behind the movement of the frame to which it is attached by the elastic wire, which frame carries along with it the concave surface over which the pencil will therefore be dragged. *To overcome the friction of the pencil, we must therefore increase the mass of the pendulum.*

II. The mass of the pendulum cannot be changed without modifying the sensibility of the apparatus; that is, the maximum vibration which a given shock will produce. But the desired sensibility is easily maintained by the pinching screw E, which must be employed to shorten the free part of the elastic wire (or a thicker wire may be introduced), until the sensibility is exactly as great as may be required.

III. Hence one and the same instrument may have any required sensibility given to it, and that *wholly irrespective of its dimensions*. The sensibility depends upon the force tending to restore the pendulum to its position of rest when the displacement = 1. The time of vibration depends on this quantity. Hence *the time of vibration is the test of sensibility*. As the condition of equilibrium approaches to indifference, the sensibility increases without limit.

IV. However weak the spring may be, and however great the sensibility, it is plain that, on the present construction (for others might easily be suggested which should give a different result), the displacement of the bob and pencil cannot by possibility exceed the forward motion which the earthquake is understood to communicate to the stand (which may be screwed to a floor). The inertia of the pendulum cannot do more than leave the extremity B as much behind A as the earthquake has shifted A forwards. If this effect be worth measuring at all, the lateral vibration of the ground must be a sensible quantity, and there is no difficulty in constructing an instrument on any scale, from an inch to 10 feet in length, in which (the time of oscillation being the same,—say one second) the maximum vibration shall have the same linear magnitude. The only consideration is, that the range may be sufficiently great to exhibit the stronger shocks without giving an inconvenient curvature to the apparatus; and for that purpose I have thought that a radius of 20 inches and a diameter of 10 inches for the spherical segment is sufficient.

V. If it be desired to *magnify* the scale of displacements, this may still be done without any increase of dimensions. Let the bob C (Plate III. Fig. 1,) be lowered upon the rod A B, so as to stand at only one-half or one-third of its height:—let the mass be increased so as to overcome the friction of the pencil as efficaciously at this diminished leverage; and let the spring be adjusted so as to give the same sensibility as before; the displacement of the bob will be the same as at first, but the displacement of the pencil will be magnified two or three times, according to their relative radii.

In practice, the pencil will not describe precisely lines upon the sphere,

but very elongated ellipses. Hence it will be easy to distinguish the mark made by the first oscillation of the pendulum, which will always be contrary to the direction in which the vibration of the ground takes place.

There is one peculiarity arising from the construction of the instrument, which, at first sight, perhaps, we should scarcely expect. The maximum displacement we have seen to depend solely upon the time of one vibration, and it may be the same (for small shocks) on whatever scale the instrument is constructed. We might expect, however, that the taller instruments would oscillate longest, and be most easily set in motion; but the contrary is the fact. This arises from the circumstance, that the stiffness of the spring must increase with great rapidity as the length of the pendulum becomes greater,—that, consequently, the elastic wire bends in all its length, unlike the feeble flat spring of HARDY'S instrument, which doubles over almost at a point. The elastic wire, therefore, tends to vibrate back and forwards many times before the inertia of its load has suffered a complete vibration to take place, and even the flexure of the pendulum-rod, by the powerful elastic action of the wire, will cause it to perform subordinate oscillations, which have a tendency to destroy one another, and to bring the whole to rest. This is a decided advantage when the object is (as in the present case) merely to register the first or maximum displacement, and I find that, with the size of the instrument which I have recommended (a 20-inch pendulum), the effect is sufficient and well marked.

In proceeding to investigate mathematically the action of such an instrument, and to shew how it may be most advantageously adjusted to inform us of the intensity of earthquake shocks, I must repeat, that I proceed upon the very limited hypothesis that the kind of shock desired to be measured consists in a lateral heaving of the earth's surface through a certain space with a uniform velocity, commencing and terminating abruptly. Except under such limitations, it is impossible to obtain rigorous conclusions. Experience alone can shew how far such conditions correspond with fact; but, unless theory indicate arrangements for testing their admissibility, our knowledge is likely to remain as indefinite as it is at present.

I. The pendulum (Plate III. Fig. 1.) being displaced, to find the force tending to redress it.

Let F = the force in grains, which, when applied to the rod AB at distance = 1 from the centre of motion (a point in the spring D), will balance the force of the spring when the lateral displacement is also = 1.

k = radius of gyration of the pendulum, which will be nearly equal to the distance of the centre of the ball C from the middle of the wire D.

M = weight of pendulum in grains.

θ = angular displacement.

The elastic force for unity of displacement at radius k is equal to $\frac{F}{k^2}$.

For an angular displacement θ or distance $k\,\theta$, it is $\frac{F}{k}\theta$. This tends to *redress* C. The effect of gravity in *displacing* C is

$\mathrm{M}k \sin\theta$,

or $\mathrm{M}k\,\theta$ nearly, when the displacement is not large.

Hence the pressure on C tending to stability is

$$\left(\frac{F}{k} - \mathrm{M}k\right)\theta.$$

The accelerating force on C is

$$\left(\frac{F}{\mathrm{M}k} - k\right)\theta g; \quad . \quad . \quad . \quad . \quad . \quad \text{Eq. } (a).$$

g being the accelerating effect of the force of gravity. Hence equilibrium is

$$\left.\begin{array}{l}\text{Stable}\\ \text{Indifferent}\\ \text{Unstable}\end{array}\right\} \text{as } \frac{F}{M} \begin{array}{c}>\\ =\\ <\end{array} k^2.$$

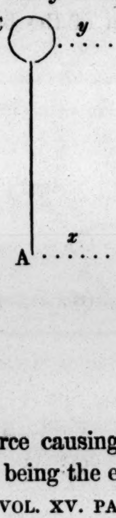
When the pendulum-rod forms an angle θ with the vertical, the displacement s of the ball, which moves with a radius k, is $k\,\theta$. Hence $\theta = \frac{s}{k}$. and the redressing force is (by Eq. (a))

$$\left(\frac{F}{\mathrm{M}\,k} - k\right)\frac{s}{k}\cdot g = \left(\frac{F}{\mathrm{M}\,k^2} - 1\right)s\,.\,g.$$

And if ϕ represent the Accelerating Force for unity of displacement of the ball

$$\phi = \left(\frac{F}{\mathrm{M}\,k^2} - 1\right)g \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (b).$$

II. To determine the motion of the pendulum when a uniform velocity is suddenly communicated to the base.



x = movement of stand,

y = movement of ball,

$s = y - x$ = relative movement of ball.

Let the motion of A be uniform for a certain time t with velocity V, then $x = \mathrm{V}\,t$,

$$s = y - \mathrm{V}\,t \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1).$$

$$\frac{ds}{dt} = \frac{dy}{dt} - \mathrm{V} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2).$$

$$\frac{d^2 s}{dt^2} = \frac{d^2 y}{dt^2} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3).$$

Force causing C to follow A (having always an opposite sign from s), $= \phi\,s$ (ϕ being the elastic action of the wire on C for $s = 1$) as above; Eq. (b).

Therefore . $\frac{d^2 y}{d t^2} = - \phi s.$

$$\frac{d^2 s}{d t^2} = - \phi s \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4).$$

$$\left(\frac{d s}{d t}\right)^2 = - \phi s^2 + c.$$

When $s = 0$ and $y = 0$, $\frac{d s}{d t} = - \mathrm{V}$ (for the point A begins to move with a velocity V),

$$\frac{d s}{d t} = \sqrt{\mathrm{V}^2 - \phi s^2}$$

$$d t = \frac{d s}{\sqrt{\mathrm{V}^2 - \phi s^2}} \quad \text{and} \quad t = \frac{1}{\sqrt{\phi}} \sin^{-1} \frac{s}{-\mathrm{V}} \sqrt{\phi} + c'.$$

When $s = 0$, $t = 0$ $\therefore$ $c' = 0$,

$$s = - \frac{\mathrm{V}}{\sqrt{\phi}} \sin (\sqrt{\phi} \, . \, t) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5).$$

The greatest value of $s = \pm \frac{\mathrm{V}}{\sqrt{\phi}}$, ... (5*).

occurs when $\sqrt{\phi} \, . \, t = 90°, 270°$, &c.

or when $t = \frac{\pi}{2\sqrt{\phi}}, \frac{3\pi}{2\sqrt{\phi}}$, &c. . . . . (6).

In order that the movement should not be oscillatory, $\sqrt{\phi} \, . \, t$ must be always

$$\angle \, 90° \text{ for all values of } t;\text{—or } \sqrt{\phi} \angle \frac{\pi}{2 t}.$$

When t is infinite $\sqrt{\phi} \angle 0$, which is impossible, if ϕ be a redressing force at all.

III. To determine the motion of the pendulum after the motion of the base suddenly stops.

Whilst x varies uniformly (with velocity = V)

$$s = - \frac{\mathrm{V}}{\sqrt{\phi}} \sin \sqrt{(\phi \, . \, t)}.$$

Let x abruptly cease to increase when $t = \mathrm{T}$ (and then let $s = \mathrm{S}$)

$$\mathrm{S} = \frac{-\mathrm{V}}{\sqrt{\phi}} \sin (\sqrt{\phi} \, . \, \mathrm{T}) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (7).$$

By (1) the displacement S is then $(y - \mathrm{VT})$ . . . . (8),

$$y = \mathrm{VT} + \mathrm{S} = \mathrm{VT} - \frac{\mathrm{V}}{\sqrt{\phi}} \sin (\sqrt{\phi} \, . \, \mathrm{T}).$$

The absolute velocity of C is then $\frac{d y}{d \mathrm{T}} = \mathrm{V} - \frac{\mathrm{V}}{\sqrt{\phi}} \cos (\sqrt{\phi} \, . \, \mathrm{T}) \sqrt{\phi}$

$$= \mathrm{V} \{1 - \cos (\sqrt{\phi} \, . \, \mathrm{T})\} \quad . \quad . \quad . \quad . \quad (9);$$

C is proceeding in space with this velocity, and under the action of a force ϕs, always tending to the point A (now stationary).

In this second stage

$$\frac{d^2 s}{dt^2} = -\phi s \text{ as before;}$$

$$\left(\frac{ds}{dt}\right)^2 = -\phi s^2 + k.$$

At the moment that $s = S$, $\frac{ds}{dt}$ became $= \frac{dy}{dT}$, because x ceasing to increase, y henceforth $= s$.

Hence, by (9) when $s = S$, $\frac{ds}{dt} = V\{1 - \cos(\sqrt{\phi} \,.\, T)\} = U$ . . . . (10).

But $\quad U^2 = -\phi S^2 + k$, or $k = U^2 + \phi S^2$;

$$\frac{ds}{dt} = \sqrt{U^2 + \phi S^2 - \phi s^2} \quad . \; . \; . \; . \; . \; . \; . \quad (11)$$

$$dt = \frac{ds}{\sqrt{U^2 + \phi S^2 - \phi s^2}}$$

$$t = \frac{1}{\sqrt{\phi}} \,.\, \sin^{-1} \frac{\sqrt{\phi}}{\sqrt{U^2 + \phi S^2}} s + k'.$$

When $t = T$, $s = S$

$$T = \frac{1}{\sqrt{\phi}} \sin^{-1} \sqrt{\frac{\phi}{U^2 + \phi S^2}} \,.\, S + k'$$

$$t - T = \frac{1}{\sqrt{\phi}} \left\{ \sin^{-1} \sqrt{\frac{\phi}{U^2 + \phi S^2}} \,.\, s - \sin^{-1} \sqrt{\frac{\phi}{U^2 + \phi S^2}} \,.\, S \right\} \quad . \quad (12).$$

Let $\quad \sin^{-1} \sqrt{\frac{\phi}{U^2 + \phi S^2}}\, S = \Theta$

$$\sqrt{\phi} \,.\, (t - T) + \Theta = \sin^{-1} \sqrt{\frac{\phi}{U^2 + \phi S^2}} \,.\, s$$

$$\operatorname{Sin}\{\sqrt{\phi}\,(t - T) + \Theta\} = \sqrt{\frac{\phi}{U^2 + \phi S^2}} \,.\, s \quad . \; . \; . \; . \; . \; . \; . \; . \quad (13).$$

The greatest ($\pm$) value of s is when $\sqrt{\phi}\,(t - T) + \Theta = 90°, 270°$ &c.; and it is then

$$s_{,} = \sqrt{\frac{U^2 + \phi S^2}{\phi}} \,. \quad . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \quad (14).$$

It recurs regularly ($\pm$) as $\sqrt{\phi} \,.\, t$ increases by π, or as

$$t \text{ increases by } \frac{\pi}{\sqrt{\phi}} \quad . \; . \; . \; . \; . \; . \; . \quad (15).$$

By (14) and (10)

$$s_{,} = \sqrt{\frac{V^2\left(1 - \cos(\sqrt{\phi} \,.\, T)\right)^2 + \phi S^2}{\phi}} = \sqrt{\frac{V^2\{1 - 2\cos(\sqrt{\phi} \,.\, T) + \cos^2(\sqrt{\phi} \,.\, T)\} + \phi S^2}{\phi}}$$

By (7) $S = \frac{-V}{\surd\phi} \cdot \sin(\surd\phi \cdot T)$, and $\phi S^2 = V^2 \sin^2(\surd\phi \cdot T)$

$$s_{,} = \sqrt{\frac{2V^2\left(1 - \cos(\surd\phi \cdot T)\right)}{\phi}} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (16),$$

$$\text{or } s_{,} = V \cdot \sqrt{\frac{2 \operatorname{versin}(\surd\phi \cdot T)}{\phi}} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (17).$$

Hence;

[1.] The total displacement $s_{,}$ is greater as V (the velocity of the shock) is greater.

[2.] $s_{,}$ is a maximum when

$$\cos(\surd\phi \cdot T) = -1$$

$$\text{or } \surd\phi \cdot T = 180°, \text{ \&c.}$$

$$\text{or } T = \frac{\pi}{\surd\phi}, \ \frac{3\pi}{\surd\phi}, \text{ \&c.} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (18).$$

That is, by (Eq. (5)) when C is in its mean position with respect to A, or $S = 0$, and $\frac{ds}{dt}$ *positive*, i. e. C moving to the right hand of A.

[3.] The value of $s_{,}$ is then (16) $\frac{\sqrt{4V^2}}{\surd\phi} = \frac{2V}{\surd\phi}$ . . . . . (19)

or *twice the greatest value* of s (Eq. (5*)). [The reason is evident, for C is then moving positively with its maximum velocity relatively to A, or V;—the stopping suddenly of A doubles the relative velocity.]

[4.] $s_{,}$ is equal to *nothing*, or the motion is destroyed if

$$\cos(\surd\phi \cdot T) = +1$$

$$\text{or } \surd\phi \cdot T = 0°, 360°, \text{ \&c.}$$

$$\text{or } T = 0°, \ \frac{2\pi}{\surd\phi}, \text{ \&c.}$$

[5.] Suppose the shock to be violent, or V very great in comparison with the velocity which the elasticity of the wire is capable of generating in a unit of time, that is, V to be large compared to ϕ, the point A must be moved forwards before C has sensibly changed its position. This follows from Eq. (16) which is equivalent to

$$s_{,}^2 = \frac{2V^2 - 2V^2\cos(\surd\phi \cdot T)}{\phi} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (20).$$

To find the value of $s_{,}^2$ when $\phi = 0$ (compared to V).

Differentiating numerator and denominator,

$$\frac{2V^2 T \sin(\surd\phi \cdot T) \cdot d\ \surd\phi \cdot}{d\phi} = \frac{V^2 T \sin(\surd\phi \cdot T)}{\surd\phi}$$

which is still $\frac{0}{0}$ when $\phi = 0$.

Differentiating again, $\frac{V^2 T^2 (\cos \surd\phi \,.\, T)\, d \surd\phi}{d \surd\phi} = V^2 T^2 \cos(\surd\phi \,.\, T)$ . . (21).

When $\phi = 0$, $s_{,}^2 = V^2 T^2$ or $S_{,} = V\,T$, ... (22)

or exactly the displacement of A.

[6.] Hence (where ϕ is small, and T not very great, so that $\cos(\surd\phi \,.\, T)$ is nearly 1), $s_{,}$ is greater as ϕ is less, and its greatest value is V T.

[7.] Since, by the action of a short sudden blow, $s_{,}$ can never be greater than V T, there is no advantage obtained by using a tall instrument, since, evidently, the smallest and largest alike can only exhibit a deviation due to the whole lateral displacement of the foot of the pendulum.

IV. To deduce the duration and measure of the lateral shock of an earthquake from observation.

For a given velocity V, and given stiffness of wire ($\surd\phi =$ const.), the final deviation will increase from $T = 0$ to $T = \frac{\pi}{\surd\phi}$ $\left(\text{by (18)}\right)$.

Therefore, by having instruments for which $\surd\phi$ varies, we may make sure that $T < \frac{\pi}{\surd\phi}$, and between these limits the displacement will *measure the duration* of the shock for a given velocity V.

To eliminate the velocity; Let different instruments be provided for which $\surd\phi$ varies. This is inversely as the time of one vibration backwards or forwards, determined by the difference of two values of t in (6), viz. $\frac{\pi}{\surd\phi}$.

Then the maximum vibration of each instrument (consistent with the limitation of T) being *observed*, may be called $s_{,}$ and $\sigma_{,}$, the corresponding forces being ϕ and ϕ'.

By (16)

$$\left.\begin{aligned} \phi\, s_{,}^2 &= 2\,V^2 (1 - \cos(\surd\phi \,.\, T) \\ \phi'\, \sigma_{,}^2 &= 2\,V^2 (1 - \cos(\surd\phi' \,.\, T) \end{aligned}\right\} \quad . \; . \; . \; . \; . \quad (23).$$

Dividing the second by the first,

$$\frac{\text{versin}(\surd\phi' \,.\, T)}{\text{versin}(\surd\phi \,.\, T)} = \frac{\phi'\,\sigma_{,}^2}{\phi\, s_{,}^2} \quad . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \quad (24),$$

from which T, the duration of the lateral shock, may be deduced. For this purpose let the pendulums be so arranged that the time of vibration of one shall be double that of the other (but for the longest let $T < \frac{\pi}{\surd\phi}$, as above); then, since the times of vibration are as $\frac{1}{\surd\phi}$, let $\surd\phi' \,.\, T = 2 \surd\phi \,.\, T$.

The following table may give a sufficient approximation :—

$\surd\phi\,.\,T$	$\surd\phi'\,.\,T$	Log. $\frac{\phi'\sigma_{,}^{2}}{\phi\,s_{,}^{2}} = 4\,\frac{\sigma_{,}^{2}}{s_{,}^{2}}$	$\surd\phi\,.\,T$	$\surd\phi'\,.\,T$	Log. $\frac{\phi'\sigma_{,}^{2}}{\phi\,s_{,}^{2}} = 4\,\frac{\sigma_{,}^{2}}{s_{,}^{2}}$
0°	0°	0.6020600	50°	100°	0.5166115
5	10	0.6012329	55	110	0.4979178
10	20	0.5987485	60	120	0.4771213
15	30	0.5945972	65	130	0.4541184
20	40	0.5887629	70	140	0.4298366
25	50	0.5812230	75	150	0.4009933
30	60	0.5719475	80	160	0.3705679
35	70	0.5608990	85	170	0.3373218
40	80	0.5480316	90	180	0.3010300
45	90	0.5332907			

From the preceding table the values of $\surd\phi\,.\,T$ and $\surd\phi'\,.\,T$ will be found by looking in the third column for the logarithm of $\frac{\phi'\,\sigma_{,}^{2}}{\phi\,s_{,}^{2}}$, or of four times the ratio of the squares of the extreme oscillations of two pendulums, derived from observation.

Since $\surd\phi$ and $\surd\phi'$ are known by the time of oscillation of the pendulums, the value of T may be found, or the duration of the shock.

The velocity of its motion will then be obtained from that of $s_{,}$ by Eq. (17).

I am not aware of the methods which have been employed for estimating the lateral shocks on railways (see Mr NICHOLAS WOOD's Report on the Great Western Railway), but I apprehend that some modification of the preceding instrument might be found useful for such experiments.

EDINBURGH, 27*th February* 1841.

XIV.—*Experimental Researches on the Production of Silicon from Paracyanogen.* By SAMUEL M. BROWN, M.D. Communicated by DR CHRISTISON.

(Read 3d May 1841.)

IN a Memoir on the Preparation of Paracyanogen submitted to the Royal Society some weeks ago, I laid down the proposition, that two equal and similar molecules may enter into the state of chemical union, the combination produced being indissoluble by every known agent of analysis; and I endeavoured to establish this proposition partly on certain abstract physical considerations, and partly by a series of experiments on the production of paracyanogen by the decomposition of the bicyanide of mercury under pressure and at high temperatures.*

The processes now to be described were deduced through a series of hypothetical inferences from the proposition referred to; and the experimental results at which I have arrived seem to support the hypothesis which conducted to them. But as the theory in question cannot be properly enunciated without the confirmatory evidence of farther investigations of a similar kind, and as the facts already discovered are, on the one hand, independent of all theory, and, on the other, must appear of themselves sufficiently startling to the chemist, I have thought it advisable to make my observations known now in their simplest form, and waive for the present what I conceive to be their theoretical explanation.

In my former paper it was stated, that I had been led to infer from experiment that two familiar substances, long and universally considered distinct elements, are really modifications of one and the same material form. Having frequently repeated and varied the experiments which led to this conclusion, and having endeavoured in every conceivable way to discover some source of fallacy, but in vain, I now venture to announce, as the result of my inquiries, that carbon and silicon are isomeric bodies, and that the former element may be converted into a substance presenting all the properties of the latter.

The present communication consists of five parts. The first treats of the production of silicon from paracyanogen; the second, of the formation of amorphous mixed compounds of silicon with copper, iron, and platinum, by the reaction of paracyanogen on these metals; the third, of the quantity of nitrogen se-

* Transactions of the Royal Society of Edinburgh for 1840–41.

parated from paracyanogen, when it is changed into nitrogen and silicon; the fourth contains processes for the preparation of amorphous, semicrystalline and crystallized disiliciurets of iron from the paracyanide of iron and the ferrocyanide of potassium; and the fifth gives an easy process for the preparation of silicic acid, on any scale of operation, by the reaction of the ferrocyanide of potassium on the carbonate of potassa.

I.—*On the Decomposition of Paracyanogen by Heat, out of contact with the Air or with any Body possessed of a stronger attraction for Carbon than for Silicon.*

The following experiments were performed on paracyanogen procured by a process implying the same principle as was inculcated in the Memoir already referred to, but with a somewhat different and better apparatus.

A B is a tube-shaped bottle of hammered iron, six or seven inches long, about an inch and a half in diameter, and furnished with a female screw at the mouth. C D is a screwing stopper, adapted to A, and perforated from top to bottom by a canal a line and a half in diameter. The bottle is filled with bicyanide of mercury, pressed down with a ramrod and hammer. The perforation having been stuffed with stucco, the stopper is screwed firmly in, and the closed apparatus is laid on the surface of a coke fire kept at a very low red heat. Mercurial vapour soon begins to stream through the pores of the gypsum; no purple flame ever appears, and if the tube be suddenly snatched from the fire, there is not observed any odour of cyanogen at the stopper. As soon as the appearance of mercury has ceased, the apparatus is removed, and there is obtained a full bulk of paracyanogen, of very nearly the same weight as the salt contained of cyanogen. This is not, however, a case of the plenary transformation of cyanogen into paracyanogen; for, as has been already noticed in my paper on paracyanogen, the former substance is largely absorbed by the latter. The product is paracyanogen as much impregnated with cyanogen as possible at the temperature and under the pressure of the shut vessel in which it is produced. It must be removed by clearing out the tube with whalebone or a feather; for it is not readily dislodged by agitation, and harder substances are apt to bring out traces of iron, unless the operation of ignition have been conducted with great care.

1. A quantity of paracyanogen was introduced into a small tube of common glass, and the open end drawn into a fine capillary. It was fused, although with difficulty, into a thick paste, by the highest heat procured from a large gas-flame by the blowpipe; care having been taken to handle the tube so as to prevent the liquefaction and dropping of the glass. The semiliquefied matter moved about in the tube, spreading itself over the sides, shrinking from the fire, and rapidly

effervescing when caught on the point of the flame. After twenty minutes of this treatment, fusion and effervescence had ceased, paracyanogen had disappeared, and was replaced by a greenish-white crust, lining the inside of the tube. On examination, it was found that the glass had been deeply corroded, and was encrusted, not with paracyanogen or carbon, but with a dense saline substance, of a siliceous character, less fusible than glass.

These appearances and effects are alike unintelligible on the supposition, which has been already shewn to have been unfounded,* that heat resolves paracyanogen into cyanogen; and inexplicable by the only other conjecture which can be made in conformity with the present theory of chemistry, viz. that paracyanogen is decomposed by heat into nitrogen and carbon. By the former, even if it were allowable, the powerful action on the substance of the tube is left without explanation; and if the latter view were adopted, it would be equally necessary and impossible to account for the disappearance of the carbon.

2. A larger quantity was driven, in the same manner, from place to place in a green glass-tube of German manufacture, containing no oxide of lead, and very refractory in the fire. In this instance, there was very little, if any, action on the glass; and there was procured, instead of a saline crust, a dark-brown infusible substance like charcoal. A part of this product was immersed in concentrated sulphuric acid, which had no reaction on it even in the state of ebullition; so that it contained no paracyanogen. It was incombustible in the oxidating flame of the blowpipe, having been only deepened in hue and rendered denser by the ignition; and when heated in a great excess of melted chlorate of potassa, it did not burn and disappear, although it had been previously reduced to a state of very fine division; and thus it was not carbon. Projected into fused carbonate of potassa, it dissolved with effervescence; and there remained a white saline product. The only known elementary bodies which possess these qualities are boron and silicon; and it is not easy to conceive any compound of nitrogen and carbon likely to display such characters. The last mentioned saline product was moistened with sulphuric acid, and it was found that alcohol, kindled over the mixture, burned with a flame of the ordinary hue. The mixture, therefore, contained no boracic acid, and, consequently, the matter projected into the carbonate was not boron. It sunk in sulphuric acid of the density of 1.8. In short, it corresponded in every distinctive property with silicon.

3. A little crucible, of Berlin porcelain, was filled with paracyanogen; and, after the lid had been tightly luted on, it was imbedded in stucco paste within a larger crucible. The gypsum having set and dried, the apparatus was kept at a white heat for an hour and a half. The residue of this process resembled the last,

* Op. cit. sect. iii. p. 168, *et seq.*

only that it was denser and darker. It was thrown into carbonate of potassa, previously fused with a little nitre in a platinum crucible; reaction and effervescence speedily ensued, and ended in its solution and the production of a transparent liquefied compound. On refrigeration, this was decomposed by an excess of hydrochloric acid in the crucible in which it had been formed. The acid solution was evaporated to dryness, and the product ignited. Water now dissolved away chloride of potassium, and left a fine, white, gritty powder, insoluble alike in acids and alkalis, infusible in melted microcosmic salt, but instantly dissolving with effervescence in fused carbonate of potassa, and then forming a saline solution in water, from which it was separated by acids as a bulky gelatinous hydrate, which was dissolved by both acids and alkalis, and became insoluble in the former by desiccation at a temperature short of redness, and in both by the process of ignition. The properties thus presented by the white gritty powder are the most remarkable and distinctive properties of the silicic acid. Such is one method of preparing silicic acid from paracyanogen and carbonate of potassa. The carbonate employed in this and all the following experiments was prepared from the bitartrate of potassa, and ascertained to be pure.

It must be added, that the operation now described is difficult of performance. If the heat be too low, an outer crust of silicon protects the paracyanogen from complete decomposition. If it be so high as to fuse the paracyanogen, the decomposition is swift and perfect, but the product is spread over the interior of the crucible, which is lined with a thinner or thicker coating of pure silicon, incapable of being separated from the porcelain by mechanical means. Even in this case, however, it is easy to prove that the lining film is silicon, as the reactive characters of that substance are quite unique. This may eventually be an economical and convenient way of lining porcelain with silicon for experimental purposes.

4. Two grains of the dark ignited substance procured by the last process yielded 4.11 grs. of silicic acid; 1 gr., 2.06 grs.; and 0.8 gr., 1.57 gr. The subject of these synthetic experiments was inferred to have been pure silicon, because the latter numbers of the series are within hundredths of the weights of silicic acid which should be produced from the corresponding weights of silicon, according to the generally received atomic weight of that body.

Although it appears to be the direct inference from these observations that silicon was produced from paracyanogen, some may suppose it equally probable that, by some unintelligible act of substitution, the silicon may have been brought out of the glass tubes and the porcelain crucible in which it made its appearance. Now, it is impracticable to remove this objection to the composition of the apparatus as a source of fallacy in this form of the experiment; because the pro-

tracted high temperature, at which it is conducted, always plasters more or less of the silicon produced over the inside of the tube or crucible; and that circumstance, together with the formation of microscopical quantities of silicic acid at the expense of the included air of the apparatus, and the difficulty of obtaining numerical results in white-heat operations, rendered it impossible to estimate the loss sustained by given weights of paracyanogen, during the prolonged ignition, with any thing like exactitude. Accordingly, having been persuaded that the product of these decompositions was ignited silicon, I endeavoured in vain to discover how much could be procured from different quantities of paracyanogen; for, it is to be observed, no metallic vessel could be used on account of the quick and powerful reaction of nascent carbon and silicon on all the ignited metals. As only an already silicated apparatus could be employed, it became desirable to effect the decomposition at a temperature which should neither admit of the partial combustion of nascent silicon in the included air, nor corrode the instrument. Now GAY-LUSSAC observed that nitrogen appeared in his cyanogen product of the decomposition of bicyanide of mercury when paracyanogen was left in the retorts; I found, in experiments on the condition of crude paracyanogen, that whenever it is raised to near the lowest visible red heat, absorbed cyanogen is driven away mixed with nitrogen; and, in some unrecorded experiments, I had observed that no temperature, except that of full white, was so conducive to the extrication of nitrogen, as one just short of a visible glow; so that there was a clue to the solution of the problem. The next experiments illustrate this point.

5. A quantity of paracyanogen, black and voluminous, was closely shut up in a Berlin crucible, and kept twenty days in a sand-bath, the temperature of which was maintained as steadily as possible at about 800° or 900° Fahr., and always, at all events, below the point of ignition. It was then found that the decomposition was complete, there being left not a trace of either paracyanogen or carbon. The product, however, was remarkably different in appearance from those of the former experiments. Not nearly so dark, it resembled burned coffee in colour; in form it was a soft light powder, very hygrometric, and soiling the fingers, instead of a compact cinder. In this condition it burned before the flame of the blowpipe for a little, producing a white ash, and then shrunk without fusion into a denser and darker form, in which it was not readily combustible. In fine, it was unignited silicon; for it answered to the other characters of that substance, which have been already indicated in connection with the analogous ignited product of the same operation, performed at a high white heat in the course of an hour or two. This simple experiment has been often repeated, and it has been observed, by periodical examinations of the material during the process of reduction, that the change is gradually effected, and that twenty days are very nearly the time required for the full decomposition of 10 grs. of paracyanogen. One

specimen, examined on the fifteenth day, contained traces of both paracyanogen and cyanogen, although the latter was present in comparatively inappreciable quantities. It is curious that crude paracyanogen, kept at these temperatures, should retain absorbed cyanogen with such tenacity; but the fact is quite intelligible when viewed in connection with the astonishing retentive powers of such bodies as sulphur and carbon over hydrogen and some other light gases. This well known property of these and some other inflammable simple radicals seems to have confirmed DAVY in his ingenious hypothesis of the compound nature of the so-called elements, if, indeed, it did not suggest the conception of it to his mind. That great investigator imagined that these obdurate bodies might all be composed of hydrogen and an unknown basis of intense affinities, the only difference, for example, between oxygen and the densest of the noble metals being, that they contain the two ingredient constituents of all bodies in different proportions; and he endeavoured to establish this conjecture by experiments with the voltaic pile.* The unexpectedly large quantities of hydrogen procured from selenium, and some other apparently simple bodies, seemed to indicate the likelihood of the wonderful hypothesis in question, although it was so much at variance with the whole physiognomy of the elemental scale that it never received the attention even which it deserved. To return from this short digression: The manner in which these mixed specimens of silicon, paracyanogen, and traces of cyanogen were analyzed was this: the product having been triturated with a large excess of chlorate of potassa, and heated till the salt was wholly decomposed, the resulting chloride of potassium was washed off the pure silicon, which, in its turn, was oxidated by fusion with carbonate of potassa, and separated in the form of silicic acid. A substance, suspected to be silicon, could not pass through a severer ordeal.

6. A tube of German glass, three inches long, weighing 99 grs. was charged two-thirds full (8 grs.), and heated first one hour to about 800° Fahr. and then another to the lowest visible glowing temperature. The object of this experiment was to produce some silicon without acting on the apparatus. The contents having been thrown into melted chlorate of potassa, the unchanged paracyanogen was burned away, and silicon remained diffused through the chloride; carbonate of potassa was added in excess, and the whole once more ignited in the same platinum crucible; the white saline product was decomposed by hydrochloric acid, and the solution dried and ignited, all in the original crucible; and there was obtained 1.7 gr. of silicic acid. The weighed tube was cleaned, weighed again, and found to have sustained no loss; and there was no visible trace of action on the glass. Similar results were obtained by several repetitions of this mode of making the experiment of the reduction of paracyanogen. This observation

* Elements of Chemical Philosophy, pp. 478–489.

supplies the desideratum in the first three, and establishes the conclusion that the silicon, which appears in these experiments, is produced from the paracyanogen itself, and is not extracted from any part of the apparatus in which it is conducted through the steps of the operation; and abundant additional evidence of this will be adduced in the progress of the investigation.

The success of these last two experiments, however, does not provide us with the means of determining with exactitude that the nitrogen of a given weight of paracyanogen is wholly dispelled, and that the four equivalents of carbon are the sole factors of the silicon which remains; for they were made with crude paracyanogen, which, as has been already observed, always contains both cyanogen and traces of silicon itself, previously produced by the transformation of carbon. Here it is worthy of remark, as a criticism on the manner of analysis followed by Mr JOHNSTONE in his examination of this substance, that a specimen might contain both of these impurities in any proportion, and yet yield to the reaction of oxide of copper, or chromate of potassa, carbonic acid and nitrogen mingled in the ratio of 2 to 1; and this both explains and reconciles his results with the seemingly incongruous observation of GAY-LUSSAC, that the cyanogen, driven by high temperatures from the bicyanide of mercury, always contains traces of nitrogen, and my own, that paracyanogen, prepared by fire, almost invariably contains appreciable traces of free silicon.* It is equally deserving of observation, however, as a comment on the great process of the history of sciences, that, but for the partial and consequently erroneous procedure of the distinguished analyst in question, the isomerism of cyanogen and paracyanogen might not have been yet discovered.

At first I thought that the difficulty resulting from the presence of silicon and cyanogen in crude paracyanogen might be overcome by having recourse to the purified substance; but, in fact, paracyanogen which has been subjected to the process of purification, by sulphuric acid and exposure of the solution to the damp of the atmosphere, is no longer one of the most attenuated of solid forms, but a pretty dense powder, intermediate in character between the crude and the ignited principles, which cannot be reduced by any elevation of temperature, however protracted, short of that of ignition; and the objection to such a temperature has already been stated. An accident, the investigation of which forms the subject of the next section, eventually led to a method which will be described in the third part of the present inquiry.

Before leaving the production of silicon from uncombined paracyanogen, there is another mode of operating to be mentioned, and it is equally remarkable for simplicity and freedom from any intelligible source of fallacy. As the nu-

* Op. cit. sect. i. compared with the results given in this section of the present paper.

merical result is somewhat unsatisfactory on account of the circumstances which have just been mentioned as affecting the condition of anhydrous paracyanogen, it is here presented in the form of an experimental formula. *Process*—Triturate crude paracyanogen with an excess of carbonate of potassa, and fuse the mixture two hours at a full white heat in a closed platinum crucible. Paracyanogen disappears; there is no free carbon in the white saline product; but it yields a conformable proportion of silicic acid, when treated in the ordinary method of analysis for that compound. There must be a considerable excess of carbonate; for, as will be shewn immediately, platinum is apt to draw off some of the silicon in its *processus è latenti*, unless it be well protected. This process is more striking when subborate of soda is substituted for potassa; for when the product is treated with acids there is no effervescence of carbonic acid; and it must be remembered, once for all, that in every professed process of transformation, the disappearance of carbon is to be accounted for, as well as the new formation of silicon.

In conclusion, the average results of my observations on crude paracyanogen, prepared in the apparatus described in the introduction, are, that it contains very nearly a third of its own weight of condensed cyanogen, and that it yields, to the three operations which have just been described, a weight of silicon never less than an eleventh, and never more than a twelfth, under the calculable weight of constituent carbon, the cyanogen of absorption being dissipated in the course of the processes.

II.—*On the formation of mixed Amorphous Compounds of Silicon with Copper, Iron, and Platinum, by the reaction of Paracyanogen on these Metals.*

1. The double copper-tube, described in the fourth section of my paper on Paracyanogen, having been packed with bicyanide of mercury, and accidentally kept at a white heat for more than an hour, it was found on examination to contain not a trace of paracyanogen. It was lined with a film, more than a line in thickness, of a very friable, reddish, metallic substance, which separated from the copper on concussion, and fell out in broken blistered scales. Pulverised, it lost its metallic lustre, and assumed a dingy brown-black colour. Nitric acid dissolved copper and left a fine black powder, like well triturated charcoal, which was observed to be distinctly brown when viewed by light transmitted through water holding it in suspension. The rigorous application of the reagents mentioned above (pp. 247–8) proved it to be silicon. The copper compound was several times made intentionally, and the result of three analyses was, that it contained from 30 to 40 per cent. of silicon. This and the two substances which follow are said to be mixed, because, in accordance with the principle of definite proportions, which is now universally recognised as the law of chemical constitution,

they must be regarded as mixtures of two or more definite compounds. The formation of these mixed metallic products explains the necessity of care in the performance of the process for paracyanogen.

2. An analogous mixed compound of iron is procured by a similar procedure with the iron high-pressure tube. It requires a higher temperature, and contains a larger proportion of silicon.

3. A new platinum crucible was half filled with paracyanogen, and ignited for three hours. On being opened, it was empty and clean, the metallic lustre having been only slightly dimmed. This was repeated, till the metal would absorb no more of what was yielded to it by the paracyanogen. It was now grey and brittle; and, on analysis of 20 grs. of the broken crucible, was found to have been composed of platinum and silicon, containing nearly 4 per cent. of the latter. This analysis was effected by the reaction of nitro-hydrochloric acid; and the undissolved residue, a mixture of silicic acid and a black powder, having been fused with carbonate of potassa, the silicic acid was separated by the ordinary process, and the constituent silicon was inferred by calculation.

The singularly powerful attraction of platinum for silicon has been often observed. DESCOTILS ignited it in contact with incandescent charcoal, and procured a frangible substance, which he represented as an indefinite combination of platinum and carbon. M. BOUSSINGAULT examined the same product, and found it to be a true siliciuret. BERZELIUS* accounts for its formation by supposing that the metal decomposes the silicic acid which is known to exist in charcoal, and appropriates the base, while, it is to be presumed, the oxygen disappears in the form of carbonic acid. This rationale is certainly incongruous with the general plan of chemical reaction as now understood, and appears to be a mere evasion of an apparently insurmountable difficulty; for silicic acid, whether nascent or produced, may be ignited in platinum crucibles with true carbon, in any excess, without being decomposed, the carbon being taken in, and the silicic acid left untouched. But for this the common analysis for silica would in many cases be nugatory. It must be remembered that charred wood is not carbon, but contains a large proportion of some exorganic compound of that element, especially if it have been produced at low temperatures; it forms artificial tannin with nitric acid, another exorganic proximate which I have tried in vain to produce with true carbon, however finely divided, such as is prepared by the decomposition of the solid iodide of carbon. Grant that common charcoal contains a compound radical analogous in constitution to paracyanogen, and BOUSSINGAULT's siliciuret of platinum is explained; it may have been produced by transformative decomposition. This postulate is rendered probable by Mr JOHNSTONE's observations on the coals,

* Traité de Chimie—BERZELIUS, par VALERIUS, 1838, p. 426.

and those charred products of the ferroprussiates which will be investigated in another section; and especially by BOUSSINGAULT's own observation that the reaction of carbon, yielding no silica on combustion, produced no siliciuret, for such carbon must have been truly mineral, and not exorganic like charcoal.

III. *On the quantity of Nitrogen separated from Paracyanogen when it is changed by heat into Nitrogen and Silicon.*

It has been already implied that the quantity of silicon obtained from paracyanogen corresponds with that of the carbon known to exist in this compound. The accidental observations of the foregoing section suggested the following method of determining that during the transformative process the whole of the nitrogen is given off as such.

1. Five grains of paracyanogen, prepared with extreme care, were mixed with little shreds of fine platinum foil, weighing twice as much as would have sufficed to absorb all the silicon which could be extracted from the paracyanogen employed. The weighings and mixture having been made in a glass-tube, closed at one end, the open extremity was drawn out just above the contents, and bent over. A spirit-flame was applied to every part of the containing tube in succession, and the separated gases collected over mercury. Without interval it was raised to a full red heat by means of the spirit-blast lamp,* and this temperature was kept up as long as any thing was given off. After the operation, the little retort was found to contain nothing but the siliciuretted platinum described in II. 3. As for the gaseous products, it may be observed that as much of the air of the apparatus as possible was expelled before the application of the spirit-lamp and the collection of the products; and, being altogether certainly not more than 0.01 gr. as well as partly balanced by the attenuated nitrogen which could not be driven out of the tube at the close of the operation, it could not introduce any error worthy of observation in an analysis of this kind. The whole product was 8.9 cubic inches at 60° Fahr. and 30° bar. Potassa removed C. I. 3.8 of cyanogen, or 2.1 grs., and shewed that the original 5 grs. was equivalent to only 2.9 grs. of true paracyanogen. There thus remained C. I. 5.1 or 1.56 gr. of nitrogen, produced from 2.9 grs. of paracyanogen, the calculations from measures to weights being made on the data of GAY-LUSSAC, and the due corrections according to rule.

2. Fifteen grains of the same parcel were ignited in a clean little platinum crucible, from which the air was carefully excluded. The loss sustained was 10.59 grs.,

* The convenient little furnace, here referred to, is described and figured in Dr CORMACK's Monthly Journal of Medical Science for March 1841.

and, according to the foregoing experiment, 6.3 grs. of this were cyanogen, so that 4.29 grs. of nitrogen were driven away from 15—6.3=8.7 grs. of paracyanogen. The inference from these experiments is, that two equivalents of nitrogen are thrown off from paracyanogen when it is changed into silicon and nitrogen; that, in other words, silicon is isomeric with carbon. This is only indirect evidence that the silicon is derived solely from the carbon of the paracyanogen; but I cannot devise another method of determining the point, and it appears to be sufficiently decisive. It may be added that the solid products of these experiments were examined, and found to consist solely of platinum and silicon, the weights of the latter, calculated from the silicic acid produced by it, being conformable to the combining proportions of paracyanogen and carbon.

In the repetition of these analyses, it must always be borne in mind that different specimens of paracyanogen contain different proportions of condensed cyanogen. One specimen yielded me more than 40 per cent. It depends partly on the temperature at which it is formed, there being also less risk of appreciable traces of silicon in the product the lower the temperature; and partly on the form of apparatus employed, or rather on the degree of pressure under which the bicyanide is decomposed. Let a specimen be tested before it be analyzed: If it be wholly soluble in concentrated sulphuric acid, it is fit for analysis, and the quantity of cyanogen it contains will be discovered in the course of the operation.

IV. *On the production of the Siliciuret from the Paracyanide of Iron.*

The fourth part of the inquiry was devoted to the production of siliciuret of iron from the paracyanide of the same metal; and particular attention is solicited to it, because the experiments are simple in design, infallible, and easy of execution on large quantities of material, while some of the products are as beautiful as they are striking. The results, which have been obtained in the course of a lengthened investigation, are embodied in the following formulæ for the preparation of the siliciuret from the paracyanide, a method which is resorted to for the sake of brevity

It is necessary to premise, that the paracyanide of iron used in my experiments was obtained from the ferrocyanide of potassium by the action of sulphur, in the following manner:—An equivalent proportion of well-dried ferrocyanide of potassium, intimately mixed in powder with three equivalents of sublimed sulphur, was heated six hours to the lowest glowing temperature of iron in the dark, in a strong sealed tube, from which the access of air was prevented by drawing the open end into a capillary. The product, having been quickly reduced to a fine powder while yet warm and in a dry atmosphere, was introduced into a percolating tube three feet in length, half an inch in diameter, and tapering at the dropping extremity to the width of a line. Linen having been bound over the dropping hole,

a column of alcohol was poured upon it; and after it had passed through, in the course of eight hours the same quantity of pure water was sent through, and was followed in its turn by alcohol. The magma was shaken up a little way in the tube, and the aperture sealed; and the open extremity, hitherto corked, was then drawn out to a capillary bore. The adhering spirit was distilled away towards the capillary, and the product obtained dry in the form of a light mouse-brown powder. A careful analysis, and the observation of its chemical properties, appear to warrant the conclusion that this body is composed of paracyanogen and iron, and that it is the true compound radical of the ferrocyanides; but it would only interrupt the continuity of the present investigation to discuss this subject in the present place, and it must consequently be reserved for a separate memoir. It is sufficient to mention at present, that the substance in question contains nitrogen, carbon, and iron, in the ratios of 1, 2, and 1, and that it is called the paracyanide of iron in this section and the next. It needs scarcely be added, that, whether or not it be the radical of the ferrocyanide of potassium, that salt certainly contains its coefficients, and may accordingly be substituted for it in transformative experiments, the supernumerary cyanide of potassium being calculated for as an incidental and inactive ingredient. The compound, which has given rise to these remarks may be readily procured in larger quantities, by putting the mixture of sulphur and ferrocyanide of potassium into one of the porous clay-bulbs of LESLIE's hygrometer, luting up the little stem, and heating it a few hours to the lowest visible heat of iron in the dark, immersed in a sand-bath or, better still, in stucco powder. Sulphocyanide and cyanide of potassium are, as it were, filtered through the sphere, and the paracyanide of iron is left within in such a state of aggregation, that it may be washed in water with impunity if it be not exposed to the air. The processes of the section may now be introduced.

1. Take the paracyanide of iron, aud having introduced it into a crucible of Berlin porcelain, lute on the lid with a strong fire-clay. Put it within two Hessian crucibles, filling the empty space with stucco powder, and placing some heavy body above the enclosed crucible. Apply the fiercest heat of a powerful wind-furnace for two hours.

There are two products in this experiment. The crucible is lined with a crust of an intensely hard, greenish-black substance, resembling obsidian, and contains a coaly powder, aggregated into little masses. These are of the same chemical composition, the latter being unfused and amorphous, the former fused and semicrystalline; but the description of the semicrystalline product is reserved till the next paragraph. The amorphous substance, treated according to rule, yields silicic acid and peroxide of iron. This and all the following experiments may be performed, either in porcelain crucibles, or vessels of hammered iron. With the latter there can scarcely be any fallacy; and, if there be, it is entirely removed

by fusing carbonate of potassa in them, and discovering not a trace of silicic acid in the salt; and, even with the former, it is easy to remove the possibility of error by producing, in successive operations, more siliciuret in each than its own weight.

It is evident that the ferrocyanide may be substituted for the paracyanide in this formula, the cyanide being sublimed away by the heat of the furnace. In one crucible of the capacity of an ounce, and weighing 500 grs., one operation produced 165 grs. of the semicrystalline siliciuret of iron, and the third repetition raised the weight above that of the crucible itself. By working a strong iron tube, like that which is figured in the introduction, several times, and then removing the iron, partly by oxidation and partly by solution in acid, there was obtained 234 grs.

The analytical method by which this product was examined, consisted in reducing it to a state of fine division in a steel mortello, rubbing it up with a large excess of pure carbonate of potassa, and fusing the mixture in a platinum crucible; the silicate of potassa, having been dissolved away and filtered, was then decomposed by an excess of hydrochloric acid, and the silicic acid was separated by desiccation, ignition, and elutriation; and the constituent silicon was calculated for. The very same operation, performed in an iron crucible, separated the iron in a peculiar condition which will be explained at the end of the next section. The average result of the application of this double process to the following products was, that they are all of the same composition, and contain 28.5 per cent. of silicon, the remaining weight being iron; and the inference is that they are disiliciurets, as might have been divined from the composition of the paracyanide and the conclusion regarding the relation of silicon to paracyanogen, which has been already stated.

2. The best way to procure the semicrystalline product of the foregoing process is this: Mix ferrocyanide of potassium with its own bulk of cyanide of potassium, and treat it exactly in the same manner, only for thrice as long a time; the supernumerary cyanide acts as a non-reactive flux, and is ultimately driven off by the fire. In this case there is no amorphous siliciuret, but the crucible, or iron tube, is lined with a fine smooth cake of the semicrystalline substance, to the depth of a line and a quarter for every operation, and it may be repeated till the vessel be nearly full. It adheres to the porcelain; but, if the crucible be broken into fragments, it may be picked off with a knife. It is stratified, and readily separates into layers. Jetty as it is in mass, when pulverized it is a light-coloured powder, of a greenish-grey hue, being the whiter the more finely it is triturated; but, whenever it is immersed in water, or moistened by any other liquid, it resumes the dark appearance of the mass. When broken down and examined by the microscope, it is seen to be perfectly transparent; and in fine powder, the

little fragments are colourless like glass. In fine, it is possessed of the crystalline structure without the crystalline form. Its specific gravity is 2.11.

3. Introduce an ounce of anhydrous ferrocyanide of potassium, mixed with twice as much cyanide of potassium,* into a Berlin crucible of the capacity of five ounces, and lute the lid tightly on, without leaving any aperture for the escape of volatile products. Put the whole into a large earthen crucible, half full of gypsum paste, and then filled with the same, so as to include the smaller crucible in a mass of stucco. After setting, and desiccation at 400°, let it be heated to the highest red in a wind-furnace for eight hours.

The design of this process is twofold; to hinder, by pressure, the quick decomposition of the constituent paracyanogen, and prevent the sublimation of the ingredient and superadded cyanide; and, by this double artifice, to produce the slow transformative decomposition of the former floating free in the liquefied excess of the latter, in the expectation that, in accordance with the indications of a great many proximate trials, the siliciuret should be evolved crystalline in both structure and form. The product is interesting: water dissolves out the cyanide, and there remains neither paracyanide nor carburet, but a large-grained sediment of transparent, nearly colourless, and very hard little crystals of siliciuret of iron. They resemble white sand, or cut-glass; and can be pulverized only in a steel mortello. Reduced to an impalpable powder, and ignited in the air, they oxidate, and are changed into a red calx, from which hydrochloric acid extracts peroxide of iron and separates silicic acid. Chlorate of potassa may be deoxidated on them, in any excess, without producing the slightest effect. They decompose melted carbonate of potassa with effervescence, silicated alkali and iron, in the peculiar condition already noticed, being the products of the reaction. When prepared during a longer time, and at a lower temperature, than has been directed above, they are opaque, like white enamel; heated to a red heat out of the air, they become clear and colourless; and, when subjected to the power of a full white heat in shut iron tubes, they assume a green tinge, and resemble the chrysolite. Their specific gravity is 2.53. This number was found by introducing 10 grs. of the sand into a common density-bottle, which was then weighed after it had been filled up with distilled water: The latter weight, subtracted from the known weight of the bottle when filled with water alone increased by 10 grs., gave the weight of a volume of water equal to the bulk of 10 grs. of the substance under examination. They are infusible by the common blast-furnace of the smithy and the ordinary blowpipe, so that the sand cannot be run into large masses; but,

* The cyanide of potassium employed in the performance of these experiments was partly prepared from the ferrocyanide of potassium by heat, and partly procured from a London manufactory; but in both cases it was ascertained to be completely free of silicic acid.

when the operation is conducted on a large scale, there often occur imperfect crystals of considerable dimensions, varying from a fourth to half an inch. Sometimes they are found in the form of irregular globular bodies, pitted on the surface, and cellular within; and, indeed, the particles of the sediment under examination generally present a somewhat rounded appearance at the edges, although they affect the octohedral shape, and are many of them seen to be very perfect octahedres when viewed on the field of the microscope. In one operation I procured half an ounce of these little eight-sided crystals, and have worked with glass, porcelain, black lead, iron, and platinum vessels, with equal success. This is a difficult process, however, and one must be content to make several trials before a fine product be obtained; but, although it is not easy to produce a perfect specimen, it is the simplest thing in the world to satisfy one's self of the change which is effected. Nay, it is impossible to make cyanide of potassium by the common process without performing this transformation; for, if the charred product be washed, and inspected with a good microscope, the crystals are seen bright and clear among the paracyanide; and the latter may be burned and dissolved away, so as to leave the siliciuret by itself. The more care bestowed on the operation, the greater the proportion of crystallized product; and the quantity is only increased by previously adding cyanide, and observing the precautions of the formula which has been given. I have examined many such products, prepared by others in ordinary routine as well as by myself, and have never failed to confirm this observation, till I have at length come to the conclusion, that the production of silicon from paracyanogen has been performed by every one who has decomposed the ferrocyanide of potassium by heat, in order to procure the cyanide of the same metal. This explains the fact, that Berzelius found the charred product under consideration to yield its own weight of peroxide of iron on simple combustion, and inferred that it was therefore a bicarburet of that metal: It may have been paracyanide mixed with some unobserved and unburned siliciuret. Analogous compounds of copper, bismuth, and some other metals, have been formed in a similar way, but it is unnecessary to describe them at present.*

* These products were described in a paper read before the British Association in 1839, and published, in abstract, in vol. ix. of its Transactions. They were described as crystallized carburets; but I distinctly stated that I had not analyzed them, and grounded my conclusion regarding their composition solely on synthetic evidence; and so far I was really in the right, for, although they are siliciurets, their ingredients are carbon and iron. I then believed them to be true carburets, having been misled by the observation, that a mixture of the crystalline and the uncrystalline products gave carbonic acid with oxide of copper, the uncrystallized product being now known to be mere unreduced paracyanide of iron. As my inaugural dissertation was never published (except an unimportant subsection of it), I gratefully acknowledge the honour conferred on it by the Medical Faculty of the University of Edinburgh, by taking this opportunity of stating that the investigation of this process was the main subject of one of the four Prize Theses for 1839.

In conclusion, it is worthy of remark, that the preparation of these crystals illustrates the formation of the diamond by natural operations, inasmuch as there is quite as great, and the very same, difference between the amorphous and the crystallized siliciurets, as there exists between the carbon of the laboratory and the gems of Golconda. In further exemplification of the principle, it may be added that I have obtained crystallized granules, which appear to be pure silicon, by submitting a mixture of paracyanogen, and a large excess of cyanide of potassium, to the operation which has been described in this paragraph; but their purity has not yet been certified by a synthetic experiment.

V. *On the preparation of Silicic Acid by the reaction of Carbonate of Potassa on the Paracyanide of Iron, free and combined.*

The facts contained in the preceding section, taken in connection with the process for preparing silicic acid which is mentioned at the conclusion of the first, naturally led to the experiments which form the subject of this, the last part of the inquiry.

1. A quantity of paracyanide of iron was mixed with four times its weight of carbonate of potassa, and the mixture ignited in a shut crucible, made of hammered iron, during the space of four hours, and at a full white heat. On being opened, the saline product presented a fine rose-red colour, which disappeared on the affusion of water; by the action of which the whole mass was resolved into a transparent solution, and a loose, partially aggregated substance, resembling spongy platinum in external appearance, which will be alluded to presently. Suffice it here, that the latter is a pure metallic oxide. In repeating this experiment, it was occasionally found that the solution of the saline product was tinged blue, that colour being changed into a fine rose tint by evaporation to dryness, and restored by the action of water. But, if the operation be not prolonged beyond four hours or thereabouts, for 3000 grains, it is wholly or nearly colourless. Hydrochloric acid dispelled carbonic acid, and threw down hydrated silicic acid from the solution under examination; and, after desiccation, followed by ignition and elutriation, the latter was obtained anhydrous and insoluble. 3.04 grs. of silicic acid were extracted from 5 grs. of paracyanide of iron. In performing this process, and especially with a view to numerical results, it is almost always necessary to purify the silicic acid. The mode of purification which I adopted simply consisted of a repetition of the process of separation: The product of silicic acid was ignited in three times its weight of carbonate of potassa; the silicate of potassa was dissolved out by water, and the potassa was more than neutralized by hydrochloric acid; the acid solution having been evaporated to dryness, the solid residue was ignited in a clean iron cru-

cible; the chloride of potassium was removed by water, and the silicic acid left anhydrous, undissolved, insoluble in boiling acids, decomposing fused carbonate of potassa with effervescence, and forming by the last reaction either a deliquescent salt or a dry glass, according as the proportion of potassa is greater or less. I have sometimes found it necessary to repeat this twice, especially when working with the ferrocyanide of potassium on the large scale.

2. The same process was performed on the common ferrocyanide of potassium, and with the very same result. 5.4 grs. of silicic acid were procured from 30 grs. of the ferrocyanide of potassium.

3. An iron crucible, in which the foregoing experiment had been made on a large scale, was, in the interval between two operations with the ferrocyanide of potassium, filled with carbonate of potassa, and heated to a full white heat for five hours. The salt was then tested in vain for the presence of silicic acid. This appears to be a crucial experiment; for, if it were possible that hammered iron should contain silicon or silicic acid, and yield them up to the action of carbonate of potassa with paracyanide of iron, in sufficient quantities to account for these results, it should certainly, *a fortiori*, produce the same effects with potassa alone. Besides, the same iron crucible was, in one instance, employed seven times successively, and no difference was observed in the several products.

4. The same process was tried twice in a platinum crucible, with complete success.

5. The same experiment was performed on the ferrocyanide of potassium, with the borate of soda instead of the carbonate of potassa. The product was quite analogous to that which has just been described, with the important difference, that hydrochloric acid produced no effervescence of carbonic acid, a circumstance which illustrates the fact, that the carbon of the materials is not changed into carbonic acid, even if such a supposition were allowable. In fine, as has been once observed already, the disappearance of the carbon of the substances subject to these operations, has to be considered and explained, as well as the production of silica from them.

6. During the last week, a crucible of the capacity of a pound and a half has been worked seven times with 3334 grs., 2000 grs., and other similar quantities of the ferrocyanide of potassium in succession. The products were all preserved; and, after ignition and purification, there were obtained 9334 grs. of silicic from 3240 grs. of ferrocyanide, although some of the product was lost in two of the operations. The only purpose to be served by the notice of a rude experiment

like this, is to indicate the scale of operation on which the process has been tried, and found sufficient.

It must now be added in conclusion, that in experiments described in this section as well as some of those previously mentioned, the carbon of the ferrocyanide of potassium is not the only element which seems to undergo transformation. The loose partially aggregated substance like spongy platinum, which is left undissolved during the action of water on the red matter in the crucible, does not present the characters of iron or any of its oxides, but possesses, so far as I have yet examined it, all the characteristic properties of the inferior oxide of rhodium. In particular, the metal which may be extracted from it has the colour and infusibility of rhodium; it does not undergo any change whatever on being acted upon by the concentrated or diluted acids either with or without the aid of heat; it is not corroded even by nascent chlorine; but it becomes oxidated when heated in contact with the air; and, when it is projected in powder into melted bisulphate of potassa, sulphurous acid is given off, and there is produced a yellow salt, wholly soluble in water. This subject I have investigated in its details, and intend to make them public ere long. At present it appears to be necessary to mention the facts in a general way, lest the occurrence of them should embarrass any one who may be induced to revise the researches which form the main object of this memoir.

Such are the experiments which have been made on this case of elemental transformation. It must be confessed that the results which have been obtained are of a kind, not only so unexpected, but so directly contrary to the doctrine of chemical affinity, which has been entertained ever since the conception of the force productive of combination was first expressed by that term, as to warrant the scepticism of men of science. In truth, although I have performed the act of transformation more than a hundred times, it has always been with fear that the products of the several processes have been examined; but nature has, in every instance, either disappointed my apprehensions, or rendered apparent failures both intelligible in themselves, and confirmatory of the general initiative of the inquiry. Accordingly, it is with some confidence that I solicit the repetition of these experiments, although I will await the issue of this appeal to the incorruptible judgment of experience with anxiety proportioned to the earnestness with which I have pursued the investigation.

HADDINGTON, 20*th May* 1841.

XV.—*On the Anatomy of Amphioxus lanceolatus*; Lancelet, YARRELL. *By* JOHN GOODSIR, *M.W.S., Conservator of the Museums of the Royal College of Surgeons in Edinburgh.*

(Read 3d May 1841.)

THE genus *Amphioxus* was instituted by Mr YARRELL for the reception of a singular little animal which he received from Mr COUCH. The characters of this genus, as given in the History of British Fishes,* are, "Body compressed, the surface without scales, both ends pointed; a single dorsal fin extending the whole length of the back; no pectoral, ventral, anal, or caudal fins; mouth on the under part of the head, narrow, elongated, each lateral margin furnished with a row of slender filaments."

My attention was particularly directed to Mr YARRELL's description of the Lancelet, by an announcement by my friend Mr FORBES, at the Newcastle Meeting of the British Association, of the capture of two specimens on the Manx coast. With his characteristic liberality, that gentleman has put these two specimens into my hands, with a request that I would employ them for the purpose of drawing up a detailed account of the animal.

Unwilling to mutilate both, I have confined my dissections to one of the individuals, and have been fortunate enough to detect its leading anatomical peculiarities, to verify most of the observations of the anatomists who have preceded me in the investigation, and to correct what appeared to me to have been errors in their observations. To complete the history of the Lancelet, however, an examination of it when alive in sea-water must be undertaken. In this way only, can certain points in its structure and actions be explained, and light be thrown on the economy of one of the most anomalous of the vertebrated animals.

The first notice which we have of the Lancelet is in the Spicilegia Zoologica of PALLAS,† who received his specimens from the coast of Cornwall. Although he observed its ichthyic characters, he allowed himself to be misled by its other peculiarities, and particularly by the membranous folds of the abdomen. He described it well, but placed it in the genus *Limax*, under the designation *Limax lanceolatus*.

Professor JAMESON has directed my attention to the first volume of STEWART's Elements of Natural History,‡ in which the Lancelet is described as a *Limax* with

* YARRELL's History of British Fishes, vol. ii. page 468. † PALLAS, Spic. Zool. x. p. 19. t. i. fig. 11.
‡ STEWART's Elements of Natural History, 2d ed. vol. i. p. 386.

the specific designation *lanceolaris*. Mr STEWART's description is evidently an abstract from that of PALLAS, to whom he refers. He had, however, a right appreciation of the essential characters, as he states that the animal is "hardly a Limax."

It is to Mr YARRELL, however, in his most valuable work on British Fishes, that we are indebted for the first detailed account of this animal. He recognised, in his solitary specimen, the *Limax lanceolatus* of PALLAS. In his description, which is in other respects most correct, he has omitted the lateral membranous folds of the abdomen, so well observed and embodied in the description of PALLAS. Mr YARRELL observed the vertebral column, the ichthyic lateral muscles, dorsal fin, intestines, and ovaries, and transferred the animal, therefore, to the Vertebrata. He placed it in the family Petromyzidæ, near the cyclostomous fishes, as he considered the fringed mouth, the armed lingual bone, the absence of eyes, and the want of pectoral and ventral fins, to be structural characters sufficient to connect it with the Lamprey and Myxine. For its reception he constituted a new genus, AMPHIOXUS, and described the species under the designation *lanceolatus*, looking upon it as the lowest in organization in the class of Fishes.*

Mr COUCH, the indefatigable ichthyologist of Polperro, who supplied Mr YARRELL with his specimen, published in the Magazine of Natural History, July 1838, a short paper, in which he gave some additional details of structure observed before the animal had been immersed in spirits. He considered it to be a fish with sixty vertebræ. He observed the anal fin, which had escaped Mr YARRELL in the preserved specimen; he also described what he considered an anomalous kind of fin rays, in the form of transverse bows or arches, the curve forming the support of the fin, the pillars probably resting on transverse spinous processes of the vertebræ. He observed that these peculiar rays did not extend to the caudal portion of either the dorsal or anal fins. In his second specimen, on which the observations of structure were made, he could detect none of the ova which were so conspicuous in the first.

I was not aware till I had almost finished my examination of the Lancelet, that any thing farther had been published on the subject. A few weeks ago, however, I observed in the Proceedings of the Berlin Academy for 1839,† an abstract of a paper on Amphioxus lanceolatus by Professor MÜLLER. From this abstract it appears that Professor RETZIUS of Stockholm has written a short memoir on the subject, in which he has announced the fact, observed by himself and Professor SANDEVALL, that the chorda dorsalis does not pass into a cranium, but terminates at a point behind it. Professor RETZIUS describes the spinal marrow as terminating considerably behind the anterior extremity of the chorda dorsalis, in a brain

* YARRELL's British Fishes, *loc. cit.*

† Bericht über die zur Bekanntmachung geeigneten Verhandlungen der Konigl. Preuss. Akademie der Wissenschaften zu Berlin. Nov. 1839, p. 197.

exhibiting scarcely any dilatation. He perceived a dark point which he supposed might be the rudiment of an eye, but he could observe no cerebral nerves. He saw numerous ribs, but no branchial clefts, and described a large opening at the posterior extremity of the gill-cavity, which he supposed to be a branchial opening similar to those in the myxine.

MÜLLER's own observations were made upon Mr YARRELL's specimen, and also upon two sent to him by RETZIUS. He verified RETZIUS' and SANDEVALL's account of the chorda dorsalis, on the sheath of which he perceived circular fibres. The oral filaments he described as consisting of central and tegumentary portions. The black spot or rudiment of an eye he could not detect. He observed the general structure of the branchial cavity, ribs, and vessels, but did not determine the existence of the branchial aperture described by RETZIUS. He states that the intestine terminates anteriorly in a cul-de-sac, a little behind which the branchial cavity opens into it on the left side. He supposed that some glandular streaks on the walls of the cul-de-sac of the intestine represented the liver, and considered a row of glandular masses on each side, consisting of cells containing dusky oval bodies as the ovaries. After some remarks on the structure of the muscles and skin of the Lancelet, Professor MÜLLER states the necessity for farther observations to ascertain the details of its structure.

The only specimens of the Lancelet, then, which have been examined are PALLAS' specimen, Mr COUCH's two specimens, one of which is in the possession of Mr YARRELL, the specimens examined by RETZIUS, SANDEVALL, and MÜLLER, and the two in my own collection. Two specimens, I believe, exist in the Museum of the Zoological Society of London.*

Having now stated what has already been done in the anatomy of this remarkable animal, I shall proceed to describe in detail the structure of the specimen I have depicted, reserving for the concluding part any general observations I may have to make on its structure and zoological character.

The dimensions and weight of the specimen of Amphioxus which has afforded the materials for this paper, are, length 2 inches; depth, a little before the middle, 2 lines; weight 8 grains. The other specimen in my possession is half an inch shorter, and not so favourable for examination. They were dredged up by Mr FORBES from a sand-bank, in deep water, on the east coast of the Isle of Man, were extremely active, and resembled, on superficial inspection, small sand-eels. On each side of the abdomen are two longitudinal membranous folds, and behind them an anal fin, omitted in Mr YARRELL's description. The folds commence, minute, on each side of the hyoid apparatus, pass back on the sides of the abdomen, increasing in breadth till they are as broad as one-fifth of the depth of the

* Magazine of Natural History, July 1838.

animal; they then diminish and terminate at the point where the lateral muscles approach on each side of the intestine, that is, at the junction of the middle and posterior thirds of the animal.

The anal fin is a fold of integument, which, commencing at the point where the abdominal folds terminate, is continued to the anus, where it is interrupted, but reappearing behind it, and becoming broader, passes on to be continuous with the dorsal fin at the extremity of the tail. The existence of a median fin in front of the anus is, as has been observed by MÜLLER, very remarkable; but it is in exact accordance with a fact mentioned to me by Professor AGASSIZ, that in certain fresh-water fishes, the development of which he had watched, a fin of this kind, with rays, exists for a short period of their embryonic existence, and then disappears.

ANATOMICAL DESCRIPTION OF THE AMPHIOXUS.

Osseous System.

Neuro-skeleton.—The osseous system, properly so called, consists of a "chorda dorsalis" tapering at both ends, without the vestige of a cranium, and of a dorsal and ventral series of cells, the germs of superior and inferior interspinous bones and fin rays. The "chorda dorsalis" consists of sixty to seventy vertebræ, the divisions between which are indicated by slight bulgings, and lines passing obliquely from above downwards on the sides of the column. In this way a separation into individual vertebræ is rather indicated than proved to exist; for although the column has certainly a tendency to divide at the points above mentioned, yet that division is rather artificial than natural. There is no difficulty in ascertaining above sixty divisions, those at each end above the number stated run so much into one another that no correct result can be obtained.

The chorda dorsalis is formed externally of a fibrous sheath, and internally of an immense number of laminæ, each of the size and shape of a section of the column at the place where it is situated. When any portion of the column is removed, these plates may be pushed out from the tubular sheath, like a pile of coins. They have no great adhesion to one another, are of the consistence of parchment, and appear like flattened bladders, as if formed of two tough fibrous membranes pressed together.

As the fibres of the sheath are principally circular, provision is made for longitudinal strains on the column by the addition of a superior and inferior vertebral ligament, as strong cords stretching along its dorsal and ventral aspects. The superior ligament lies immediately under the spinal cord, and may be recognized as a very tough filament, when the column is torn asunder, or some of the vertebræ removed. The inferior ligament may be raised from the inferior surface of the column in the form of a tough ribbon. From the sides of the column apo-

neurotic laminæ pass off to form septa of attachment between the muscular bundles; and along the mesial plane above the column, a similar lamina separates the superior bundles of each side, and by splitting below and running into the sides of the column, forms a fibrous canal for the spinal cord. Foramina exist all along the sides of this canal for the passage of the nerves. A similar septum is situated along the inferior part of the column, from the part where the inferior muscular bundles unite at the anus, to the extremity of the tail. Along the superior edge of the aponeurotic septum, between the dorsal muscular bundles, and stretching from the anterior point of the vertebral column to a point beyond the anus, and half embedded between the superior extremities of the muscles, is a series of closed cells of a flattened cylindrical form, adhering firmly to one another by their bases, so as to present the appearance of a tube flattened on the sides with septa at regular distances. Each of these cells is full of a transparent fluid, in the centre of which is an irregular mass of semi-opaque globules, apparently cells. This series of cylindrical sacs consists of the rudiments of interspinous bones, and probably of fin rays, and is attached below to the fibrous intermuscular septa, half covered on each side by the lateral muscles, and enclosed above by the tegumentary fold which constitutes the dorsal fin.

A similar series of cells, with the same relations, is situated on the ventral surface of the body, and stretches from the spot where the abdominal folds terminate, to a point nearly opposite the termination of the dorsal series.

Splanchno-skeleton.—The splancho-skeleton consists of a hyoid apparatus and a series of branchial ribs, seventy or eighty on each side. This division of the skeleton will be described along with the respiratory apparatus, with which it is intimately connected.

Nervous System.

The spinal cord is situated on the upper surface of the chorda dorsalis, enclosed in the canal formed in the manner above described. When the whole length of this canal is displayed by removing the muscles, and then carefully opened, the spinal cord is seen lying in the interior, with nerves passing out from it on each side. It stretches along the whole length of the spine, is acuminated at both ends, and exhibits not the slightest trace of cerebral development. In its middle third, where it is most developed, it has the form of a ribbon, the thickness of which is about one-fourth or one-fifth of its breadth; and along this portion, also, it presents on its upper surface a broad, but shallow groove. The other two-thirds of the cord are not so flat, and are not grooved above, are smaller than the middle third, and taper gradually; the one towards the anterior, the other towards the posterior extremity of the vertebral column. A streak of black pigment runs along the middle of the upper surface of the cord. It is situated in the groove already described, and is in greater abundance anteriorly and posteriorly,

where the nerves pass off at shorter intervals, than at the middle or broadest part of the organ. From fifty-five to sixty nerves pass off from each side of the cord; but, as the anterior and posterior vertebræ are very minute, and run into one another, and as the spinal cord itself almost disappears at the two extremities, it is impossible to ascertain the exact number, either of vertebræ or of spinal nerves. These nerves are not connected to the spinal marrow by double roots, but are inserted at once into its edges in the form of simple cords.

The nerves pass out of the intervertebral foramina of the membranous spinal canal, divide into two sets of branches, one of which run up between the dorsal muscular bundles (dorsal branches); the other (ventral branches) run obliquely downwards and backwards on the surface of the fibrous sheath of the vertebral column; attach themselves to the antero-posterior aspect of each of the inferior muscular bundles, and may be distinctly traced beyond the extremity of each bundle. When an entire animal is examined by transmitted light, and a sufficient magnifying power, the anterior extremity of the spinal cord is observed, as before mentioned, to terminate in a minute filament above the anterior extremity of the vertebral column. The first pair of nerves is excessively minute, and passes into the membranous parts at the anterior superior angle of the mouth. The second pair is considerably larger, and, like the first pair, passes out of the canal in front of the anterior muscular bundle. The second pair immediately sends a considerable branch (corresponding to the dorsal branches of the other nerves) upwards and backwards, along the anterior edge of the first dorsal muscular bundle. This branch joins the dorsal branch of the third pair, and, passing on, joins a considerable number of these in succession, and at last becomes too minute to be traced farther. After sending off this dorsal branch, the second pair passes downwards and backwards on each side above the hyoid apparatus, and joins all the ventral branches of the other spinal nerves in succession, as its dorsal branch did along the back. This ventral branch of the second pair is very conspicuous, and may be easily traced along the line formed by the inferior extremities of the ventral divisions of the muscular bundles, the ventral branches of the other nerves joining it at acute angles between each bundle. It may be traced beyond the anus, but is lost sight of near the extremity of the tail. Twigs undoubtedly pass from the spinal and lateral nerves towards the abdominal surface of the body, but, on account of their minuteness, and the difficulty of detecting them in detached portions of the abdominal membrane, they could not be satisfactorily seen.

When a portion of the spinal cord is examined under a sufficient magnifying power, it is seen to be composed entirely of nucleated cells, very loosely attached to one another, but enclosed in an excessively delicate covering of pia mater. The cells are not arranged in any definite direction, except in the middle third of the cord, where they assume a longitudinal linear direction, but without altering their primitive spherical form. The black pigment, formerly mentioned as existing

more particularly on the upper surface and groove, is observed to be more abundant opposite the origin of the nerves; and, as it is regularly arranged in this manner in dark masses along the anterior and posterior thirds of the cord, the organ in these places, on superficial inspection, resembles much the abdominal ganglionic cord of an annulose animal. Along the middle third the pigment is not so regular, but appears in spots at short intervals. When any portion of the cord, however, is slightly compressed, and microscopically examined, it becomes evident that there is, along the groove and mesial line of its upper surface, a band, consisting of cells of a larger size than those composing the rest of the organ. Some of these cells only are filled with black pigment, but all of them contain a fluid of a brown tint, which renders the tract of large cells distinctly visible. When the compression is increased the cells burst; and the fluid which flows from the central tract is seen to contain jet-black granules, which may be detected as they escape from the cells.

The nerves consist of primitive fibres, of a cylindrical shape, with faint longitudinal striæ. The primitive fibres of a trunk pass off into a branch, in the usual way, without dividing; and, where the trunks join the spinal cord, the primitive fibres are seen to approach close to it, but without passing into it. The greater part of the slightly protuberant origin consisting of the nucleated cells of the cord, with a few pigment cells interspersed, the exact mode of termination of the central extremities of the primitive nervous fibres could not be detected.

Muscular System.

This system is highly symmetrical, consisting of a series of lateral muscular bundles, corresponding in number, size, and position, to the vertebræ of the "chorda dorsalis." These bundles have a general resemblance to the division of the lateral muscles of the higher fishes. Each bundle consists of a dorsal and ventral portion. The dorsal passes from the lateral line, on a level with the vertebral column, backwards and upwards; the ventral passes from the same level, downwards and backwards. The inferior bundle is the longest; and both of them have a somewhat conical shape, and are attached to the spinal column and skin by the aponeurotic septa formerly described. The fibres of these muscles pass respectively from before, obliquely upwards and downwards, almost, but not completely, in the direction of the muscular bundle, along that portion of the trunk occupied by the branchial portion of the intestinal tube. The ventral bundles pass half-way over the dilated cavity, and terminate in blunted extremities, which are attached to the skin, and to the walls of the branchial compartment, so as to dilate it for the reception of sea-water. Beyond the anus the ventral bundles are attached to each side of the fibrous septum above described, meeting below in a sharp ridge. Between the anus and the branchial cavity, where these muscles inclose the digestive portion of the intestinal tube, they do not meet completely below, but are

connected by an aponeurosis, which forms a strong tendinous arch at the point in front, where the muscles separate more completely. The whole cavity of the trunk, which is occupied by the intestinal tube, is lined by a fine aponeurotic membrane, which, about the lower edge of the lateral muscles, becomes muscular, and shuts in the whole of the inferior part of the trunk from the mouth to the tendinous arch formerly described. This abdominal muscle consists of two layers —an external, apparently longitudinal; an internal, transverse.

The only muscle in the lancelet for performing a special function is a flat bundle, connecting and bringing together the two halves of the hyoid apparatus, for the purpose of closing the mouth.

Under the microscope the primitive fibres of the lateral muscles exhibit the usual transverse striæ, but are not collected into fasciculi, constituting immediately the mass of the tissue. Transverse striæ are not observable in the fibres of the abdominal muscle.

Intestinal System.

This system appears as a tube passing nearly in a straight line from mouth to anus. It consists of two portions—an anterior, large and dilated, and appropriated to the respiratory function; and a posterior, small, of pretty uniform calibre, and constituting the proper digestive apparatus. The respiratory portion of the canal will be described afterwards in connection with the vascular system. The mode in which the digestive communicates with the branchial department of the tube could not be satisfactorily made out. It appeared, however, that the branchial cavity, becoming smaller, curved slightly of itself towards the left side, and then proceeded directly, and without any change in its calibre, to the anus. The anus is in the form of a longitudinal slit.

There is not the slightest trace of a liver, or of any other assistant chylopoietic viscus. The gut was full of a brown granular matter, tinged, probably, by a bilious secretion from the walls of the bowel.

Respiratory System.

This system is constituted by the anterior compartment of the intestinal tube, on the walls of which a peculiar vascular arrangement exists for the aeration of the blood, and the complicated skeleton superadded, for the efficient performance of that function.

In connection with the respiratory apparatus, I shall, as formerly proposed, describe the splanchno-skeleton. This division of the osseous system consists of a hyoid apparatus, and of a range of branchial ribs.

The hyoid apparatus supports the mouth, and guards its entrance. The mouth is in the form of a longitudinal slit, and is bounded on each side by the two divisions of the hyoid apparatus. Each of these consists of seventeen pieces

articulated together. From each of these pieces, except the first, a ray proceeds, those at the extremities of the two divisions being shorter than those at the centre. The anterior extremities of the two divisions, or branches of the apparatus, meet at the anterior superior angle of the mouth; and the two posterior, after curving forward, meet at the posterior inferior angle. The various pieces of which this apparatus consists have the consistence of cartilage. They are hollow along the bases, and to the points of the rays. Their cavities appear to be full of a transparent fluid, containing here and there masses of globular cells, exactly similar to those in the interspinous bones. This part of the skeleton is covered by the integuments, and by the membrane of the branchial cavity. A fringe of the integument surrounds the hyoid rays, extending a little beyond their bases. This fringe must be considered as the lip or margin of the mouth, the hyoid rays, although occasionally dependent, belonging properly to the cavity of the mouth. The rami of the hyoid are brought together, and the mouth closed, by the transverse muscle formerly described.

Immediately behind the hyoid apparatus the branchial cavity commences, and continues as a dilated tube, which at last contracts, and becomes continuous, as formerly described, with the digestive portion of the intestine. The walls of the two anterior thirds of the branchial cavity are strengthened on each side by a series of transparent cartilaginous, highly elastic, hair-like ribs, which are imbedded in their substance. The upper extremities of these ribs are fixed in two streaks of a tough white substance which runs along on each side of the inferior surface of the chorda dorsalis, on the sides of the inferior longitudinal ligament. The inferior extremities of the ribs terminate in a more complicated manner. Each alternate pair of ribs bifurcates. The inferior branch of the rib on each side meets its fellow of the opposite side at an angle in the median line. The superior branch curves up also, and meets that of the other side. The non-bifurcated ribs are shorter, and terminate in a line with the bifurcation of the neighbouring pairs. There results from this arrangement a sort of skeleton canal, the walls of which are completed by membrane. There are from seventy to eighty ribs on each side. Their general direction is from above downwards and from before backwards, but more perpendicular than the ventral bundles of the lateral muscles, with which they form acute angles. Along the edges of these ribs vessels pass for the performance of the respiratory function, and the canal above described contains the branchial artery or heart.

Vascular System.

In the canal which has been described as passing along the inferior wall of the branchial compartment of the intestinal tube, a vessel runs. This vessel diminishes anteriorly; and, posteriorly, it also diminishes, and is lost in the direction of the digestive tube. Valves, if they exist, have not been detected in this tube.

At the extremities of each pair of bifurcated ribs the abdominal vessel just described gives off a primary branch, which passes along the edge of the rib, sending secondary branches at regular intervals and at right angles to the other primary branches on each side. Along the opposite sides of *all* the ribs another set of vessels may be seen, passing on to the chorda dorsalis, enlarging as they advance, and sending off secondary branches at right angles. When near the heads of the ribs, these vessels anastomose in semicircular loops, the canals of which are of large calibre, and the walls provided with distinct circular fibres. From the angles between each of these loops, and continuous, therefore, with the primary branches, smaller trunks pass on to the median line, and enter, opposite to their fellow at the other side, into a small longitudinal vessel which runs along the whole length of the chorda dorsalis, between the heads of the ribs, and on the inferior surface of the inferior longitudinal ligament. This vessel is the Aorta, and distributes arterial branches to the various parts of the body.

Generative System.

This system consists of a series of somewhat irregular, bean-shaped, granular bodies, situated each on the inside of the inferior extremity of the ventral portions of twenty or thirty of the muscular bundles of the middle third of the animal. These masses are attached to the internal surface of the aponeurotic lining of the abdomen, on the outside of the branchial chamber. No duct or outlet could be detected. Each mass, under the microscope, displayed a congeries of cells of various sizes, evidently incipient ova or sperm cells. The individual did not appear to be in season.

Tegumentary System.

The skin is remarkably thin, but tough; and exhibits neither scales, pigment, nor metallic lustre, except at the base of the dorsal fin, along which, or the upper edge of the interspinous bones, a silvery band of considerable strength passes. The skin, under the microscope, displays minute parallel striæ, which occasionally cross one another. The beautiful iridescent tints which it exhibits, both before and after detachment, appear to be caused by these striæ; and the same structure probably produces similar phenomena in the aponeurosis which lines the cavity of the abdomen.

Concluding Remarks.

At a very early period in the development of every vertebrated animal, the cerebro-spinal axis presents the appearance of a white elongated streak. At the same period, and in accordance with this simple condition of the nervous central organ, the skeleton consists of a chorda dorsalis, and, very soon afterwards, of some of the peripheral elements of the spinal column. A central organ of circu-

lation, in the form of a tube on the anterior inferior aspect of the embryo, invariably coexists with the simplest forms of the nervous and osseous systems. Branchial clefts and a liver are parts of the embryo of the vertebrated animal which are never found to accompany a cerebro-spinal axis of the simplest form, or a heart before it becomes divided into compartments.

No adult vertebrated animal has hitherto been described which at all approaches in organisation the simplicity of the embryonic forms to which allusion has just been made. Such an animal, a being perfected before the appearance of branchial clefts, might have been conceived; and, from the laws of organic development, its position in the system might have been indicated. As *Amphioxus* makes a close approximation to this simplicity of type, it may be useful to consider the relation of its different organs one to another.

One of the most remarkable peculiarities in the Lancelet is the absence of the brain. RETZIUS, indeed, describes the spinal marrow as terminating considerably behind the anterior extremity of the chorda dorsalis, in a brain which exhibits scarcely any dilatation; but careful examination of the dissection of my own specimen, which I have also submitted to the inspection of Dr JOHN REID, and of other competent judges, has convinced me that the spinal cord, which may be traced with the greatest ease to within 1–16th of an inch of the extremity of the chorda dorsalis, does not dilate into a brain at all. It may be urged that we ought to consider the anterior half of the middle third of the spinal marrow, where it is most developed, to be the brain, and all that portion of the chorda dorsalis which is in connection with the branchial cavity, as the cranium. That this does not express the true relation of the parts, is evident from the fact, that this portion of the cord, to its very extremity, gives off nerves, which are too numerous to be considered as cerebral, but more especially from the mode of distribution of the first and second pairs, which, in my opinion, proves the anterior pointed extremity to be the representative of the brain of the more highly developed vertebrata. A brain of such simplicity necessarily precludes, on anatomical grounds alone, the existence of organs of vision and of hearing. These special organs, developed in the vertebrata at least, in a direct relation with the cephalic integuments and the brain, could not exist, even in the form of appreciable germs, in the Lancelet. The black spot which RETZIUS took for the rudiment of an eye may probably have been, what also deceived me at first, a portion of the black mud which floats about in the branchial cavity, and which adheres obstinately to the parts in the neighbourhood of the oral filaments. The first pair of nerves, although very minute, in accordance with the slight development of the parts about the snout, and the want of special organs of sense, might, from their position and relations, be considered as corresponding to the trifacial in the higher vertebrata. The second pair appears to be the vagus, not only from its distribution as a longitudinal filament on each side of the body, as in other fishes, but also from its rela-

tions to the hyoid apparatus and branchial cavity, to which division of organs the eighth pair of fishes is specially devoted. The distribution of a branch of this nerve, however, along the base of the dorsal fin, and the course of the posterior part of the main branch, would appear to shew that this nerve, which I have provisionally denominated the Vagus, is, in fact, the trifacial, which, in the higher fishes, is not only distributed to all the fins, but holds exactly the same relations to the dorsal and anal fins, and to the spinal nerves, as the nerve now under consideration in the Lancelet.

The peculiarities in the structure of the spinal cord are not less remarkable than those of its configuration. It is difficult to understand, according to the received opinions on the subject, how a spinal cord destitute of primitive fibres or tubes, and composed altogether of isolated cells, arranged in a linear direction only towards the middle of the cord, can transmit influences in any given direction; and more especially how the tract of black or grey matter, if it exercises any peculiar function (excito-motary) communicates with the origin of the nerves. The nerves, also, are remarkable, originating in single roots, and containing in their composition one kind only of primitive fibres (cylindrical).

In reference to the skeleton of the Lancelet, it is evidently of the simplest kind. If we limit the term skeleton to the Neuro-Skeleton, this animal possesses only the primitive form of such a skeleton—a chorda dorsalis without any cranial enlargement, with a dorsal and ventral series of germs of interspinous bones and fin rays—peripheral elements of a spinal column.

From a consideration of the particular class of embryonic forms to which this fish corresponds, we could not expect either bone or cartilage in the composition of its skeleton. Accordingly, the skeleton consists of a series of sacs, assuming particular forms according to their several positions: flattened in the chorda dorsalis, cylindrical in the fin bones. These sacs are easily derived, according to established histological laws, from the primitive nucleated cells which constitute the tissue of their representatives in the embryo, and contain, in their interior, cells, or the nuclei of cells. This view of the tissue of the skeleton of the Lancelet is based on a law of organization which is not usually recognized in questions like the present, viz. that adult organs representing embryonic organs, are altered so as to be fit for the performance of their functions, but never so far as to depart, either in tissue or form, from the type of their corresponding embryonic organs. The arch-shaped fin rays, described by Mr Couch, are merely the dissepiments between the cylindrical germs of the fin bones.

The leading peculiarity of the Lancelet, considered as a representative of an embryonic form in the adult series, is the want of true gills or branchial arches—the deficiency of branchial clefts. Retzius, indeed, describes an opening at the posterior part of the branchial cavity, which he compares to the adominal openings in the Myxine; but as I have been unable to discover this opening in my spe-

cimens, I agree with MÜLLER in considering its existence as highly problematical, and I shall proceed to demonstrate that, in accordance with the plan on which the other organic systems of this animal are formed, such an opening into the branchial chamber could not exist. The abdominal openings in the Myxine are the result of the closure of its numerous branchial clefts by the integuments. They are analogous, in fact, to the branchial orifices of the tadpole, immediately before cessation of the aquatic respiration. The respiratory apparatus of the Myxine, then, although inferior in functional activity to that of other fishes, is actually referable to a more elevated type. The Myxine possesses a brain in which the central masses are considerably evolved, and a nervus vagus of sufficient development. The brain of the Lancelet, again, is reduced to a mere filament, and the existence of a nervus vagus appears to be highly problematical. These considerations, and the fact that branchial openings have not been detected by YARRELL, COUCH, MÜLLER, or myself, must lead to the conclusion that this fish has either never had branchial clefts at any period of its existence, or that if it at any time had them, they must have totally disappeared. I am inclined to believe that the former is the real state of the case, not only from the views already urged in reference to the other organs in this animal, but also from the consideration that if these clefts had ever existed their traces would have remained. As the seventy or eighty pairs of branchial ribs cannot be looked upon as true branchial arches, and as we cannot suppose that any vertebrated animal could have so many branchial fissures, we are driven to the conclusion that the Lancelet never had at any period of its existence true branchial arches and clefts, and that the ribs have been developed for a special purpose—for a mode of branchial respiration hitherto undescribed in the class of fishes.

The Lancelet respires by receiving sea-water into the anterior compartment of its intestinal tube—this cavity is kept dilated by the elasticity of the numerous filamentous ribs, and this dilatation may be increased by the action of the superimposed ventral bundles of the lateral muscles. It is contracted by the action of the abdominal muscle. This is a mode of respiration similar to that which prevails in the tunicated mollusks. It is interesting to observe that the branchial membrane of the Lancelet is exactly similar in its peculiar vascularity (ramifications at right angles) to that which lines the branchial cavity of the mollusks just specified.

If the branchial membrane were examined in the living animal, it would undoubtedly exhibit cilia in as great abundance as in the branchial membrane of the *ascidiæ*, and such a ciliary arrangement must constitute one of the active agencies, not only in renewing the supply of water for respiration, but also in conveying food to the orifice of the digestive portion of the intestinal tube. As in the *ascidiæ*, the entrance of the intestino-respiratory canal is guarded by filaments. The hyoid filaments of the Lancelet performing the same office as the filaments at the oral

orifice of the *ascidiæ*, acting as a sieve in preventing the entrance of foreign bodies, or of food, which it has neither jaws to comminute, nor powers of stomach to digest.

The branchial ribs I do not consider as parts of the neuro-skeleton, as they bifurcate to inclose the heart, this organ in the Lancelet being contained in a sac resembling the cartilaginous pericardium of some other fishes. They are repetitions of the hyoid bone developed for a new form of branchial apparatus. They are true splancho-ribs, parts of a splanchno-skeleton, and analogous to the cartilages of the trachea and branchial tubes (also repetitions of the hyoid bone) of the higher vertebrata. Some of these splanchno-ribs, had branchial clefts been developed, would have become true branchial arches; but just as in the vertebrata above the fishes, in which the branchial clefts have disappeared, and tracheal cartilages have become developed, so in this animal, in which the branchial clefts have never appeared, cartilaginous arches have become necessary for its peculiar aquatic respiration.

The hyoid filaments of the Lancelet must not be considered as the analogues of the branchiosteogous rays, which spring from the peripheral aspect of the bone, but as developed forms of the teeth or tubercules which are ranged along the central aspect of the branchial apparatus of the higher fishes, and which are occasionally highly developed for similar purposes. As the upper jaw is developed from a cranium, and the lower jaw is formed at a period posterior to the appearance of the hyoid bone—the absence of these two bones is a necessary consequence of the inferior position of the Lancelet in the series of vertebrate forms.

The plan of the circulation is simple, and in accordance with the primitive condition of the respiratory apparatus, both functions being performed in a manner closely resembling that observed in certain annulose animals. The dorsal vessel corresponding to the heart or branchial artery, and the abdominal vessel to the aorta of the Lancelet, the lateral communicating vessels of certain of the rings in the annelide performing the respiratory function, like the vessels of the branchial chamber already described. The development of cardiac septa and of a liver follow closely, if they do not accompany, the branchial fissures. The absence of such fissures in the Lancelet sufficiently explains this deficiency of parts usually considered essential to the vertebrated animal.

For similar reasons, true renal and generative organs do not appear in this animal.

The double row of isolated generative organs are in the normal position of their embryonic representatives, and not more advanced in organization than the Wolffian bodies at their first appearance. How the contents of these ovisacs or spermsacs are conveyed to the exterior, it is difficult to say. If the abdominal opening described by Professor Retzius actually exists, it appears to me much more probable, that it is an opening, not into the branchial, but into the peritoneal

cavity, as in certain of the higher fishes, and that it performs the double function of admitting sea-water for peritoneal respiration, and for allowing of the exit of the ova and sperm from the cavity of the abdomen, into which they are cast from the glandular organs attached to its lining membrane. This hypothesis, which I have had no opportunity of verifying, gets rid of the difficulty in a satisfactory manner, explains to a certain extent the observation of RETZIUS, and is in accordance with the type of formation in the class.

Viewed as an entire animal, the Lancelet is the most aberrant in the vertebrate sub-kingdom. It connects the Vertebrata not only to the Annulose animals, but also through the medium of certain symmetrical ascidiæ (lately described by Mr FORBES and myself*), to the Mollusks. We have only to suppose the Lancelet to have been developed from the dorsal aspect, the seat of its respiration to be transferred from its intestinal tube to a corresponding portion of its skin, and ganglia to be developed at the points of junction of one or more of its anterior spinal nerves, and inferior branch of its second pair, to have a true annulose animal, with its peculiar circulation, respiration, generative organs, and nervous system, with supra-œsophageal ganglia, and dorsal ganglionic recurrent nerve.

As some fishes undergo metamorphoses after leaving the ovum, the question naturally suggests itself, is the Lancelet an adult fish? May it not be the young of some fish in one of the stages of growth? The uniformity of every specimen of it hitherto described, and the peculiar toughness and firmness of its tissues, appear to be decisive of its being a perfect animal.

In regard to the zoological position of *Amphioxus*, Mr YARRELL was correct in giving it the lowest place in the class of fishes; but if the details of its structure, and the anatomical considerations which this paper contains, be correct, the genus can no longer be retained in the same family with Petromyzon and Myxine, but will assume an ordinal value in any new arrangement of the class.

If genera allied to Amphioxus are at present in existence, they are probably not numerous; but in the ages which have passed since the development of animal forms commenced, a-branchiated fishes may have been more common; and the paleontologist, when his attention is directed to the subject, may probably be able to refer some anomalous organic remains to extinct fishes of this order.

* Report of the British Association, 1840.

EXPLANATION OF PLATES IV. V.

PLATE IV.

Fig. 1. A lateral view of Amphioxus lanceolatus. As the specimen when sketched was slightly compressed between two plates of glass, it is represented of greater depth than the animal exhibits in its natural condition. *a* The mouth, with the oral filaments; *b* the abdominal fold of the left side; the fold is semitransparent, so that the lower surface of the abdomen is seen through it; *c* the anus, with one fin before, and another behind it; *d* the dorsal fin; the vesicular germs of the rays are seen in all these fins, and the splanchno-ribs are also visible through the abdominal parietes; *e* the length of the specimen.

Fig. 2. The abdominal aspect of the specimen. *a* The mouth; *b b* the abdominal folds; *c* the anus; *d* the heart.

Fig. 3. A lateral view of the same specimen after the removal of the integuments, including the abdominal folds, and the soft parts of the fins. *a* The mouth, with the oral filaments; *b* the abdominal muscle, with the splanchno-ribs seen through it; *c c* the heart; *d* the anus; *e* the vesicular germs of the rays of the anterior; *f* those of the posterior anal fin; these germs do not, like the soft parts, extend to the extremity of the tail; *g* the germs of the rays of the dorsal fin, which, like those of the anal fin, do not extend along the tail; *h h* the lateral muscular bundles separated by the needle, so as to display in their intervals the " chorda dorsalis," and the dorsal and ventral branches of the nerves; *i* the first pair of nerves; *k* the second pair, analogous to the trifacial, the dorsal and ventral branches of which extend along the bases of the fins to join the branches of the other nerves. This dissected specimen is flattened by slight compression, in order to display the various parts with greater distinctness.

Fig. 4. The integuments have been removed from the tail, but the abdominal folds have been left. The abdominal muscle, and the branchial compartment of the intestinal tube, have been opened longitudinally, a little to the right side of the mesial line. *a a* The two divisions of the hyoid bone; *b b* the internal surface of the branchial chamber, through the walls of which the " chorda dorsalis," the nerves, and the ventral bundles of the muscles, are seen; *c c* the heart, with the splanchno-ribs passing off from it on each side towards the " chorda dorsalis;" *d d* the abdominal muscle; *e* the digestive portion of the intestinal tube proceeding to the anus; *f g g* the abdominal folds.

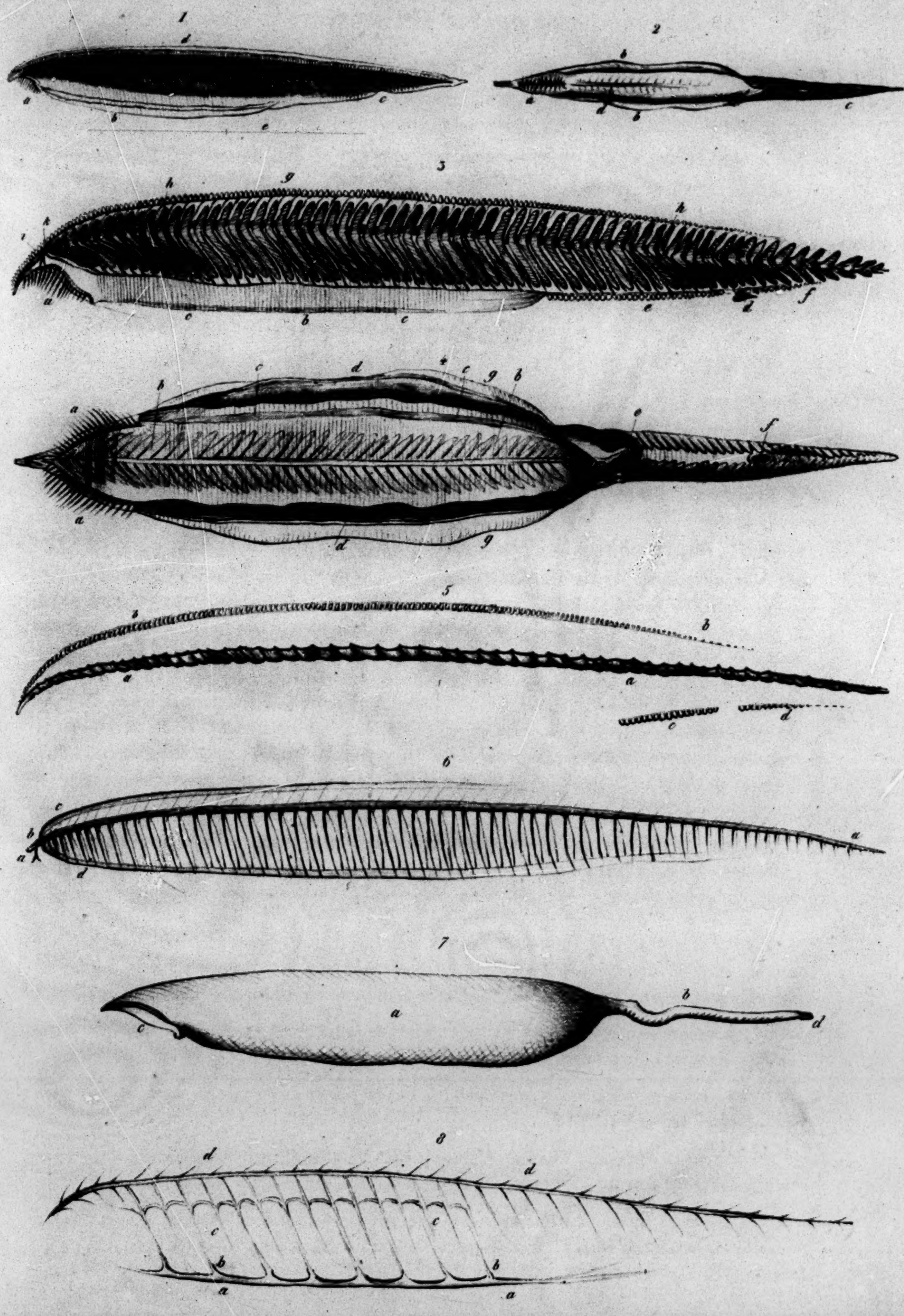
Fig. 5. The neuro-skeleton, consisting of *a a* the " chorda dorsalis," *b b* the vesicular germs of the dorsal fin rays, *c* those of the anterior, and *d* those of the posterior anal fins.

Fig. 6. The nervous system. *a a* The spinal cord; *b* the first pair of nerves; *c* the dorsal; *d* the ventral branch of the second pair.

Fig. 7. The intestinal system. *a* The branchial compartment; *b* the digestive compartment of the intestinal tube; *c* the mouth; *d* the anus.

Fig. 8. The vascular system. *a a* The heart; *b b* the primary branches, or branchial arteries; *c c* the branchial veins uniting in loops, from the angles between which, trunks convey the blood into *a a* the aorta.

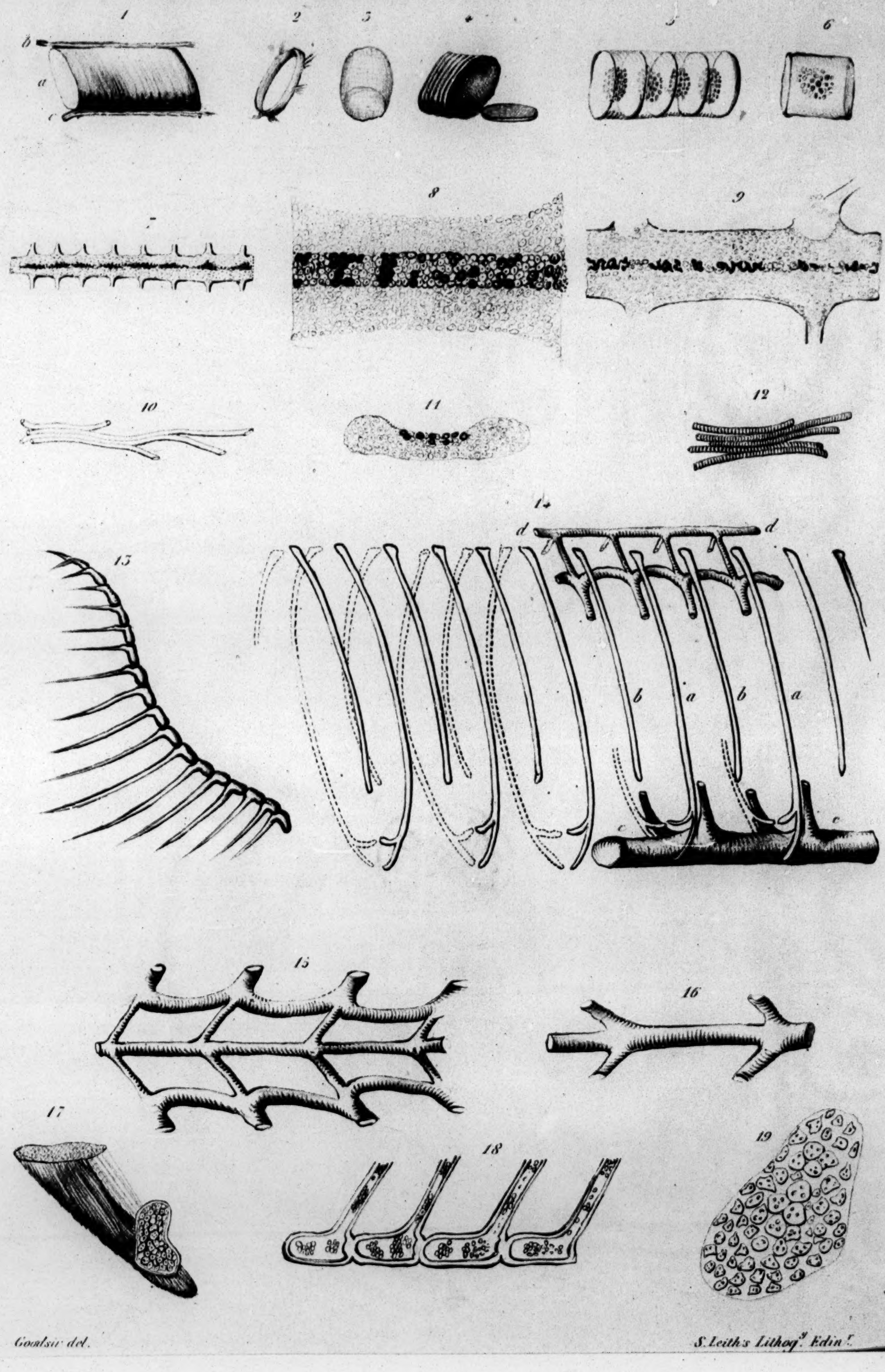
ANATOMY OF AMPHIOXUS LANCEOLATUS



Goodsir del.

J. Leith's Lithography Edin.

ANATOMY OF AMPHIOXUS LANCEOLATUS



Goodsir del.

S. Leith's Lithog. Edin.

PLATE V.

Fig. 1. *a* Portion of the "chorda dorsalis," to shew the circular fibres of the sheath; *b* the superior; *c* the inferior longitudinal ligament.

Fig. 2. A portion of the sheath; shreds of the aponeuroses adhere to it.

Fig. 3. One of the compressed vesicles which occupy the interior of the sheath, and compose the mass of the "chorda dorsalis."

Fig. 4. Some of the compressed vesicles removed from the sheath, to shew their relation one to another.

Fig. 5. Five of the cylindrical cells, from the dorsal fin, to shew their relative positions, and the masses of cells which they contain in their interior.

Fig. 6. A single cell.

Fig. 7. A portion of the spinal cord, from its anterior third, magnified, to shew the black matter which runs along the median line, and the origins of the nerves.

Fig. 8. A portion of the middle third, highly magnified to shew the nucleated cells of which it is composed, and the larger cells of the dark median band. Some of the cells of the dark band are filled with black pigment granules, which are represented escaping under the compression.

Fig. 9. A portion of the spinal cord, magnified to shew the origin of the nerves by single roots, and without the insertion of the primitive fibres of the nerves into the substance of the cord.

Fig. 10. Primitive fibres of a nerve.

Fig. 11. A transverse section of the spinal cord, to shew the groove on its upper surface, and the black matter on the floor of the groove.

Fig. 12. Primitive fibres from one of the lateral muscles.

Fig. 13. The eighteen pieces of one of the divisions of the hyoid bone. The anterior piece carries no ray.

Fig. 14. A few of the splanchno-ribs to shew their relations to the heart, aorta, and branchial vessels. *a a* The ribs which bifurcate; *b b* the simple ribs; *c c* a portion of the heart with four branchial arteries; *d d* a portion of the aorta with eight branchial veins.

Fig. 15. A portion of the aorta to show the mode of connection between the aorta and branchial venous trunks and loops.

Fig. 16. A portion of the heart to shew the mode in which the arteries leave it. The heart or ventral vessel is somewhat flattened, its upper and under walls meeting at an acute angle on each side.

Fig. 17. The lower end of one of the lateral muscular bundles, magnified to shew the position and configuration of one of the generative organs.

Fig. 18. Portions of four pieces from the hyoid bone, magnified to shew the mode of connection, also the cavities, and irregular masses of cells which they contain.

Fig. 19. One of the generative organs, highly magnified under compression, to shew the nucleated cells of which it is composed.

On the Action of Water upon Lead. By ROBERT CHRISTISON, M.D., F.R.S.E., Professor of Materia Medica in the University of Edinburgh.

(Read 7th February 1842.)

IN an experimental inquiry into the action of water on lead,* published by me in 1829, in continuation of some previous researches by GUYTON-MORVEAU, it was stated as the general result, that all very pure waters, such as distilled water, rain, and melted snow, act upon lead,—dissolving a trace of it, and causing the formation of an insoluble carbonate of lead in large quantity. It was likewise shewn, that this action is prevented by the existence of neutral salts in solution; so that most terrestrial waters, as they contain saline matter, act feebly and only in circumstances favourable in other respects. Farther, it appeared to flow from comparative experiments, that this preventive power depends upon the acids of the salts, and not upon their bases;—and that their energy as preventives, that is, the minuteness of the proportion required to annihilate the action, is in the ratio of the insolubility of the compounds which the acids of the salts are capable of forming with oxide of lead.

Since the time when the investigations now referred to were first made public, my attention has been repeatedly recalled to the subject by divers interesting facts connected with the economic use of lead, which have illustrated practically the conclusions drawn from experiments conducted in the laboratory. Two of these facts, which relate to the employment of lead as the material for water-pipes, are so remarkable in their circumstances, that I am induced to lay them before the Society. On the one hand, they shew that the action of water on lead, notwithstanding its serious consequences, and all that has been written respecting it, does not seem to have attracted the attention among such professional men as engineers, architects, and others, which it unquestionably deserves. And on the other hand, when taken along with the general principles formerly established by me, they point out the risk of the action of water on lead-pipes being unexpectedly developed, where due care is not taken, but at the same time fix the conditions in which the action may be foreseen, and likewise provide a simple and efficacious remedy.

As an appendix to this communication, I propose also to take notice of a topic of more purely scientific interest,—namely, a question which has arisen

* Treatise on Poisons, *first edition*, 1829, p. 384.

since I first wrote on this subject, as to the exact nature of the substance formed by the action of water upon lead.

The first fact to be mentioned has been already briefly alluded to by me in a former publication in 1836.* But its circumstances appear to merit a more detailed statement.

A few years ago, the water of a spring was introduced into the mansion of Dalswinton in Dumfriesshire, by a lead-pipe, from a distance of three-quarters of a mile. While on a visit there in the autumn of 1834, only a few days after operations were finished, and the water was flowing into the great cistern for supplying the house, my attention was called one morning by one of the guests to the water-bottle on his dressing-table, and a tumbler of water which had been poured from it. The bottle was lined with a thin white incrustation of a pearly lustre; and the water, which had stood some time exposed to the air, presented a thin film of the same appearance over its whole surface. The cause being at once suspected, we proceeded with our host, Mr Leny, to examine the great cistern into which the water was discharged directly from the pipe. Here we found the water entirely covered with a similar film, and the bottom and sides of the cistern lined with a loose pearly white incrustation, in such quantity as to appear as if painted with white paint. It was also remarked, that water fresh drawn from the pipe was perfectly transparent at first, but, on exposure to the air, quickly presented the white film seen in the tumbler. It needs scarcely be stated, that the appearances now mentioned were recognised as the result of the action of the water on the lead of the pipe. And it may be added, that the white substance was afterwards ascertained to be a carbonate of lead.

I confess that the observations thus made surprised me not a little. For being told, the previous autumn, that it was proposed to bring into the house in lead-pipes the water of this spring, which had long enjoyed a high character in the neighbourhood for purity, I made an experiment for the purpose of discovering whether it could be safely conveyed in lead; and finding that several pieces of fresh-cut lead retained their lustre almost untarnished when immersed for fourteen days in a tumbler of the water, I concluded that it contained salts enough to prevent corrosion of the lead. I did not at the time advert to the difference between an experiment in which some ounces of water were left at rest on a few square inches of lead, and one in which a column of water only three-quarters of an inch in diameter flowed constantly over a surface of nearly 800 square feet.

The means of clearing up the cause of the action, by analyzing the water, were not within my reach. It was presumed, however, from the general principles established by previous inquiries, that the reputation of the spring for extra-

* Treatise on Poisons, *third edition*, p. 489.

ordinary purity was not without foundation. Accordingly, it afterwards appeared from analysis to be very unusually pure. Oxalate of ammonia occasioned a white haze, and slowly a very scanty white precipitate, shewing a trace of lime. Phosphate of ammonia had no effect at first, on being added to the water after removal of the lime; but in the course of some hours, a few microscopic shining crystals formed on the glass, thus indicating a trace of magnesia. Nitrate of baryta, however, did not affect the water in the slightest degree, proving the extreme scantiness of sulphates. And nitrate of silver caused only a faint opalescent whiteness; so that even the muriates were present in unusually small quantity. On concentrating the water it was found that the salts existing in it were hydrochlorates, sulphates, and carbonates of lime, magnesia, and soda, and that the hydrochlorates greatly predominated. A minute estimate of the several ingredients was not attempted, because unnecessary. But their total amount proved to be only 0.554 of a grain in 11,860 grains, or $\frac{1}{21400}$th.* Water taken direct from the pipe, and kept for some days well corked in a bottle, was quite transparent when first poured out; but, on being slightly concentrated by boiling, a few white shining crystals of great delicacy were formed. These, when detached and washed with distilled water, disappeared in water acidulated with nitric acid; and on this solution being evaporated to dryness, there was obtained a trace of crystalline powder, which, when re-dissolved, gave a fine yellow precipitate with bichromate of potash, and a black one with sulphuretted hydrogen,—clearly proving the presence of lead dissolved in the water.

On referring to what has been stated in my account of my first experiments respecting the action of water on lead, it will readily appear why this water should have oxidated and dissolved the metal of the pipe. The spring is not only one of very great purity, but the protective salts contained in the water likewise consist chiefly of those whose preventive power is the feeblest of all the natural ingredients of springs. For in my experiments on the small scale, it did not appear that the hydrochlorates effectually prevented the action of distilled water, unless present in the proportion of a 2000th at least.

It remains to take notice of the remedy applied in this case. When a similar instance happened at Tunbridge, in 1814,—with the additional circumstance that its nature was not discovered till lead-colic began to appear among the inmates of the houses supplied with the water,—the only satisfactory remedy which could be thought of, was the expensive one of removing the pipes and replacing them

* On again lately analyzing the water, I found the solid residuum to be exactly one grain in 17,500. Nitrate of baryta, after twenty-four hours' rest, occasioned an exceedingly scanty deposite of sulphate; and the residuum left, on evaporating 17,000 grains, effervesced very slightly with diluted nitric acid. The sulphates and carbonates were thus again proved to exist in very minute proportion. In this water a stick of polished lead became tarnished in an hour; and in four days a little white powder formed on the bottom of the vessel under the lead.

with others of cast-iron; and this was accordingly done. Reflecting, however, upon what I had observed in many experiments with weak solutions of neutral salts, and remembering that in general, after the action had gone on slowly for some weeks, it gradually became less and less, while, at the same time, a firmly-adhering film formed on the lead, consisting of carbonate mingled with a salt of oxide of lead in union with the acid of the salt in solution, and that, when lead so lined was transferred even into distilled water, no action seemed to take place,—I conceived that an effectual remedy might be provided by producing, in like manner, an incrustation of the same kind on the inside of the pipe. For this end, it was proposed to leave the pipe for some months filled with a weak solution of phosphate of soda, in the proportion of a 25,000th part, which is somewhat stronger than what had seemed sufficient to prevent the action of distilled water on the small scale. It was hoped that a fine film of mixed carbonate and phosphate of lead would thus be formed while the water was at rest, which would adhere so firmly as not to be swept away when the water was allowed to flow, and which would serve as a lining to prevent the contact of the running water with the metal. Circumstances prevented this plan being tried at once; and in the mean time it was judged right to try the effect of forming a lining of carbonate of lead, by leaving the water at complete repose in the pipe, so as to allow the carbonate to crystallize slowly and firmly on its interior. This experiment was attended with complete success. The pipe was kept full of the spring-water, and without water being drawn from it, for four months. The water was then found to flow without any impregnation of lead, and has done so ever since.

The other incident I propose to describe occurred last year at Buchan-ness Lodge, a cottage-residence of the Earl of Aberdeen. It resembles the former singularly in all its leading circumstances.

In the beginning of June last, Mr Johnston of Peterhead was requested to visit professionally the housekeeper of the Lodge, who was affected with indigestion and constipation;—from which, however, under his directions, she speedily recovered on this occasion. Six weeks afterwards he was requested to visit her again, and found her then affected with vomiting, constipation, severe spasmodic pain at the pit of the stomach, retraction of the umbilicus towards the spine, great weakness of the limbs, and other symptoms of less note, which it is scarcely necessary to particularize in this communication, but which are proper to the severe form of colic occasioned by slow poisoning with lead. After treating the case judiciously for three days, Mr Johnston was surprised to find, that, notwithstanding frequent temporary benefit, no permanent amelioration had taken place. At last, on the third day, while considering the circumstances of his patient's illness, his attention was drawn to the water-bottle on her dressing-table. It was lined with a

white shining incrustation, and the surface of the water was covered with a film of similar appearance. On inquiry, he learned that the water always presented this appearance after being exposed for some time, but that it was quite transparent when first drawn; and he afterwards personally verified these facts.

Being well aware of the action of water on lead, and of the consequences of the insidious introduction of the compounds of that metal into the human body, Mr Johnston, with much discernment, although he had never seen a case of lead-colic before, was strongly inclined to believe that he had to deal with that disease. He suspended his ultimate decision, however, until he had an opportunity of examining chemically the substance deposited by the water. This he found to be soluble in weak acetic acid; and the solution gave a white precipitate with sulphuric acid, a white one with carbonate of potash, a yellow one with iodide of potassium, and a black one with sulphuretted hydrogen. The last test, an extremely delicate one, likewise made the water itself brown. As these results left no doubt whatever of the presence of lead in the water, he could as little entertain any doubt of the nature of the housekeeper's illness. She was treated accordingly, recovered slowly but steadily, and in October was quite well. She had resided in the house, and constantly used the water for eight months before her final severe illness; but for some months previous to that attack, she had been often annoyed with stomach-complaints and constipation. Her niece, a girl of twelve, who had been residing with her for a few weeks only, was also attacked with these premonitory symptoms of lead-colic; but she soon recovered under Mr Johnston's care. No other person had resided for any length of time at the Lodge during the period in question. Lord Aberdeen had been there with some friends for a few days only.

Mr Johnston, with whose approbation, as well as the sanction of Lord Aberdeen, the facts of this incident are made public, consulted me respecting it about the middle of September, and afterwards communicated much additional information,—partly, indeed, in reply to suggestions made by me. The following is a short narrative of the whole particulars. The water was first introduced into the house in the summer of 1840, by a lead-pipe from a spring at the distance of rather more than half a mile. The spring was purchased for the purpose, as it had been represented to be of fine quality; and an analysis, by a chemist in the neighbourhood, appeared to shew that it was of unusual purity, and contained very little saline matter. This will presently be seen to be by no means the case.

The pipe had been in use for several months before the housekeeper went to reside at the Lodge. When Mr Johnston first examined the water, it had been in constant use for twelve months. And yet it continued to impregnate itself with lead to the last: In the water, when fresh drawn, he could detect lead by sulphuretted-hydrogen even without concentrating it.

The architect, under whose directions the water had been introduced into the house, was slow to believe that the housekeeper really suffered from the effects of lead, or that the water was impregnated with this metal. Even the condition of the principal cistern, which was found in precisely the same state as at Dalswinton, did not open his eyes altogether to the truth, and indeed rather impressed him with the notion that negligence in cleaning the cistern was the source of any mischief that might actually have arisen. This was not surprising. For it was plausibly argued, that such accidents had not been observed in other places, and more especially at Aberdeen, where lead is prevalently used for conducting water; and the architect was probably unacquainted with the scientific details of the subject, as they have been hitherto little dwelt upon except in works on Toxicology.

It has been just stated, that the spring was purchased as one of great purity, represented from actual analysis to "contain but a very small quantity of solids." I therefore inferred, that, like the water of Dalswinton, it was too pure for the preventive power of the usual neutral salts of springs to be efficaciously exerted. Being anxious, however, to fix positively the circumstances connected with so remarkable an instance of the action of a natural water on lead, I obtained some of the water for examination. It was transmitted by Mr JOHNSTON with all due care to ensure its purity. The result is, that, although by no means the sort of water it was alleged to be, the circumstances of the case come precisely under the general principles established by me in 1829.

The water is clear, colourless, and without taste. Polished lead immersed in it becomes tarnished in a few hours, but undergoes no farther change in fourteen days. Twenty thousand grains evaporated to dryness left a residuum, which, after exposure to a low red heat, weighed 4.482 grains, indicating a 4460th of solids. Hence the water is far from being a pure spring-water: It is not more so than that of many streams in the Scottish Lowlands. It contains, in fact, so large a proportion of salts, that, if these were of the ordinary kind, lead would scarcely be acted on by it at all. But its ingredients are chiefly the least energetic in preventive power of all the salts usually found in terrestrial waters. Oxalate of ammonia has at first no effect, but slowly causes a slight haziness; which in some hours gives place to a scanty white precipitate, indicating the presence of a mere trace of lime. Phosphate of ammonia has no effect even after twenty-four hours; but when the water is much concentrated, this test occasions a crystalline precipitate, proving the existence of a minute trace of magnesia. Nitrate of baryta produces slowly a very scanty precipitate. As neither this precipitate, nor the saline residuum obtained by evaporating the water to dryness, presents any effervescence with diluted nitric acid, the water does not contain any carbonate. The barytic precipitate from 4375 grains of water weighed 0.218 of a grain, and therefore corresponded with the small proportion of only a 32,000th of some sulphate, either sulphate of soda or probably sulphate of lime. Nitrate of silver, however,

occasions at once a dense milkiness and white precipitate, shewing the presence of a large quantity of muriates. Lime and magnesia being present only in the most minute proportion, it is evident that the chief base in union with the muriatic acid is soda; which farther appears from the cubical tendency of the crystals obtained by evaporation. It therefore follows, that this water contains about a 4500th of its weight of muriate of soda, the merest traces of sulphates and muriates of lime and magnesia, and no carbonates of any kind. Since carbonates and sulphates are the most energetic of the preventive salts usually met with in terrestrial waters, and the muriates, the only salts here present in any material quantity, do not act as preventives effectually unless in at least double the proportion observed in my analysis, it is easy to understand why the lead was so readily attacked in this instance.

When the fact of the water being poisoned with lead was clearly ascertained, it was at first proposed at once to substitute iron pipes for those of lead. But Mr Johnston suggested that a trial should be made of a weak solution of phosphate of soda, as explained above, and recommended in 1836 in my "Treatise on Poisons." The experiment was accordingly tried by keeping the pipes constantly full of a solution containing a 27,000th of phosphate of soda. For three weeks no improvement took place; but it was found that owing to a leakage in the pipe, the solution required to be constantly renewed, and was therefore never completely at rest. As it appeared essential to secure this last condition, the leak was found out, though not without difficulty, and was properly stopped. Fourteen days afterwards the spring water was readmitted, and a manifest improvement was ascertained to have taken place, although lead was still contained in the water. The solution being replaced, another trial was made in the course of six weeks more; and sulphuretted-hydrogen then barely tinted the water. A third examination was made fourteen days later; and after the water had been running for some time, sulphuretted-hydrogen did not in the slightest degree affect it. The last report I have from Mr Johnston, dated the 27th of January, states that the solution had been withdrawn for a month previously,—that the water had been kept running constantly for several days before it was subjected to examination,—and that no trace of lead could be detected in it by careful analysis.

From the facts now detailed, together with the results of my former inquiries, the following conclusions may be drawn as to the employment of lead-pipes for conducting water.

1. Lead-pipes ought not to be used for the purpose, at least where the distance is considerable, without a careful chemical examination of the water to be transmitted.

2. The risk of a dangerous impregnation with lead is greatest in the instance of the purest waters.

3. Water, which tarnishes polished lead when left at rest upon it in a glass vessel for a few hours, cannot be safely transmitted through lead-pipes without certain precautions.*

4. Water which contains less than about an 8000th of salts in solution cannot be safely conducted in lead-pipes, without certain precautions.

5. Even this proportion will prove insufficient to prevent corrosion, unless a considerable part of the saline matter consist of carbonates and sulphates, especially the former.

6. So large a proportion as a 4000th, probably even a considerably larger proportion, will be insufficient, if the salts in solution be in a great measure muriates.

7. It is, I conceive, right to add, that in all cases, even though the composition of the water seems to bring it within the conditions of safety now stated, an attentive examination should be made of the water, after it has been running for a few days through the pipes. For it is not improbable, that other circumstances, besides those hitherto ascertained, may regulate the preventive influence of the neutral salts.

8. When the water is judged to be of a kind which is likely to attack lead-pipes, or when it actually flows through them impregnated with lead, a remedy may be found, either in leaving the pipes full of the water and at rest for three or four months, or by substituting for the water a weak solution of phosphate of soda in the proportion of about a 25,000th part.

It may be mentioned, that the most convenient way to detect lead in water is, *first,* to examine what separates on exposure to the air by dissolving it in warm acetic acid, and testing the solution with sulphuretted-hydrogen, iodide of potassium, and bichromate of potash,—*then,* if this process fail, to concentrate the water to an eighth part, and again test any insoluble matter which separates,—and *lastly,* failing this procedure also, to evaporate the water to dryness, subject the residue along with charcoal to a red-heat, act on what remains with warm diluted nitric acid, and test the solution when filtered and neutralized with an alkali. It may admit of question, whether, in the event of lead being indicated in the last way only, the very minute quantity which may then be present can prove detrimental. But this is a topic which it is foreign to my present object to enter into.

* Conversely, it is probable, though not yet proved, that, if polished lead remain untarnished or nearly so for twenty-four hours in a glass of water, the water may be safely conducted through lead-pipes.

As connected closely with the subject of the preceding observations, I beg to append a few remarks on the nature of the compound of lead which is formed in the course of the action of water on the metal.

Where salts are present, whose acids are capable of forming insoluble compounds with oxide of lead, the deposit which gradually forms on the lead consists partly of these compounds. But where a substance is produced which floats loosely in the water, as in the action of distilled water; or where the water, from being clear when fresh drawn from a pipe, deposits a white precipitate on exposure to the atmosphere—as in the instance of the two spring-waters mentioned above —a compound of a different kind is produced. That which is formed in distilled water is the only variety produced readily in sufficient quantity for examination.

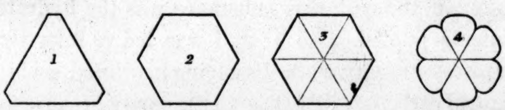
Guyton Morveau thought this substance was the hydrated oxide of lead. From my experiments, published in 1829, I was led to infer that it is carbonate of lead, for it effervesces strongly while dissolving in diluted acids; and it appeared to me to be the neutral carbonate ($PbO + CO^2$), partly because no other carbonate of lead was known at that time to chemists, and partly because, when deprived of hygrometric water by a temperature of 212°, its loss of weight, on being subsequently heated to redness, corresponded closely with the theory of its elements being united in the proportion of a single equivalent of acid and oxide. In 1834, however, Captain Yorke, who made some interesting experiments on this subject, —without being, for some time, aware of those previously conducted by me,— thought, in the first instance, that the substance formed in distilled water was the hydrated oxide: but he afterwards found reason for considering that it contained both carbonate and hydrated oxide of lead, although in proportions variable and not definite.

It appears improbable, that a substance which puts on invariably and entirely a crystalliform appearance, as this compound does when formed in distilled water, should be a mere mixture, of indefinite composition. Accordingly, I find that it is for the most part, and under certain conditions invariably, a regular definite compound of the carbonate and hydrate of the oxide of lead.

If lead be immersed in distilled water deprived of its gases by ebullition, and exposed to atmospheric air which has been freed of carbonic acid by solution of potash, the water soon becomes turbid and the lead tarnished, and there is slowly formed on the lead a crust of transparent microscopic crystals, presenting triangular and quadrangular facettes, and on the bottom of the vessel a whitish powder with a shade of leaden blue, and not crystalline. Both the crystals and powder are soluble, without effervescence, in nitric acid, and convertible into a yellow powder, with disengagement of much moisture, when they are heated to redness

after having been dried at 212°. This is evidently a hydrated oxide of lead. The powder becomes carbonated when left exposed to the air, even in the dry state.

When the action of the water takes place in the air, the product is more abundant, and seems to consist entirely of a mass of white, pearly, microscopic crystals. These acquire a pale gray tint, when removed from the water and allowed to dry spontaneously. When examined in water with a powerful compound microscope, they present the appearance of a congeries of thin tables. The primitive form of the table is probably the equilateral triangle, the crystals being therefore thin sections of a regular tetrahedre. Some crystals present the triangular form (1.), but with the angles always slightly truncated; others, by excessive truncation, have become hexagons (2); others present slender radiating lines, dividing the hexagon into six constituent equilateral triangles (3); and others of the latter construction assume the appearance of rosettes, probably by erosion of the angles of the hexagon (4).



When 28.62 grains had been dried at 180°, they lost only 0.01, on being heated to 250°; and no further change took place till the temperature rose to 350°, at which point moisture began to be slowly discharged. A low red heat expelled much carbonic acid and a considerable quantity of water.

An apparatus was constructed for transmitting the disengaged gas through fragments of chloride of calcium to absorb the moisture, as well as for collecting the dry gas over mercury. When the whole gas and water had been expelled by heat, air, previously deprived of carbonic acid gas and moisture by means of caustic potash, was passed through the tubes composing the apparatus, for the purpose of driving into the gas-jar the carbonic acid left in the tubes. The volume of carbonic acid was then ascertained by absorption with solution of potash, and properly corrected to the temperature of 60° and the barometric pressure of 30 inches. The results obtained with 24.20 grains of the substance formed in large quantity by the continuous action of distilled water for twenty months, were—

Oxide of lead, ...	21.035 grains.
Carbonic acid (5.588 cubic in.), . .	2.641 ...
Water, ...	0.535 ...
	24.211 grains.

These numbers correspond nearly with the theory, $3\,PbO + 2\,CO^2 + Aq$; that is, a compouud of three equivalents of oxide of lead, two of carbonic acid, and one of water,—or rather, a compound of two equivalents of carbonate of lead in union

with one equivalent of hydrated oxide of lead. The exact numbers by calculation, supposing the oxide correct, are 21.035 oxide, 2.656 acid, and 0.540 water. The slight deviation in my numerical results for the carbonic acid from the exact atomic numbers, probably depends on some of the protoxide of lead having absorbed oxygen from the atmospheric air passed through the tubes at the close, while heat was applied to the oxide, so that some of it became red oxide of lead.

This experiment was repeated with 27.965 grains, and the products were—

Oxide of lead, ...	24.275
Carbonic acid (6.33 cubic in.), . . .	2.992
Water, ...	0.650
	27.917

These numbers, like the last, approach closely to the theory $3PbO + 2CO^2 + Aq$, —the exact numbers, by calculation from the oxide, being 3.06 of carbonic, and 0.62 of water.

In a third trial with 23.935 grains, the results were—

Oxide of lead, ...	20.870
Carbonic acid (5.285 cubic in.), . . .	2.497
Water, ...	0.547
	23.914

Theory applies here exactly so far as regards the water, but the acid is somewhat deficient, the correct numbers, by calculation from the oxide, being 2.635 acid, and 0.540 water.

I have also examined the proportion of carbonic acid in other differently prepared specimens, by wrapping the powder in filtering paper, and introducing this into a jar filled with mercury and a little strong muriatic acid previously charged with carbonic acid gas. The results have been conformable with those stated above. When the water was freely exposed to the atmosphere, I have never found that the proportions differed more than a small fraction from the theoretical numbers just assigned.

The substance in question is therefore a definite compound of two lead salts. Other analogous examples have been for some time known to exist among the oxides and salts of this metal. Though of a brilliant whiteness while in water, it is rather gray when dry. It is permanent in the air; for a specimen exposed for many months gave the usual proportion of carbonic acid, when decomposed with muriatic acid. When suspended in water and treated with a stream of carbonic acid, the water of the hydrated oxide is displaced, and a neutral carbonate is formed, which is more dense, and of a pure white colour when dry.

On first ascertaining the nature of this substance, I imagined it was a new

body, not previously recognised by chemists. I have since found that MULDER conceives the common carbonate of lead, the white lead of commerce, to be of the same nature. He has recently discovered it to consist sometimes of two, and sometimes of three, equivalents of neutral carbonate, united with one of hydrated oxide; and he states that the whitest and finest varieties contain most carbonic acid. I find the white lead of this city, which is usually of fine quality, to be a compound of four equivalents of carbonate and one equivalent of hydrate. MULDER adds, that he could not succeed in displacing the whole water by means of carbonic acid. This may be so; but the compound formed by the action of distilled water on lead is not similarly constituted. When agitated for two hours in water with a brisk stream of carbonic acid gas, 27.93 grains of the dry product gave, by analysis,—

Oxide of lead, ...	23.44 grains.
Carbonic acid (9.27 cubic in.), . . .	4.38 ...
Water, ...	0.15 ...
	27.97 grains.

The carbonic acid obtained is only 0.06 of a grain short of what is required to neutralize the oxide.

XVII.—*On the Parasitic Vegetable Structures found growing in Living Animals.* By John Hughes Bennett, M.D., Edinburgh. (Communicated by Dr Graham.)

(Read 17th January and 7th February 1842.)

That the eggs of numerous parasitic animals may be deposited in the textures of living beings, and that these develop themselves in such textures, and draw thence their nourishment, has been long known. But that, under particular circumstances, certain cryptogamic plants are capable of germinating and fructifying in the living tissues of animals, and especially in man himself, is a discovery of recent date.

As these growths are not only interesting to the naturalist, but, inasmuch as they are connected with disease in animals, ought to arouse the attention of the pathologist, I was induced to make them a subject of observation, and have now the honour of laying the results before the Society.

The following are the objects of the present memoir.

1*st,* To confirm and extend the observations and experiments of M. Gruby concerning the mycodermatous vegetations found in the crusts of the disease named *Tinea favosa,* or *Porrigo lupinosa* of Bateman.

2*d,* To announce the occasional existence, and describe a plant found growing on the lining membrane or cheesy matter of tubercular cavities in the lungs of man.

3*d,* To describe the structure of a plant found growing on the skin of the gold-fish.

And, 4*th,* From a review of all the facts hitherto recorded in connexion with this subject, to draw certain conclusions respecting the pathological state which furnishes the conditions necessary for the growth of fungi in living animals.

I.

Observations on the Mycodermatous Vegetations constituting the crusts of the Tinea favosa, or Porrigo lupinosa of Bateman.

In the Comptes Rendus des Séances de l'Academie des Sciences for July and August 1841, there will be found abstracts of observations made by M. Gruby on the crusts of the disease named *Tinea favosa,* or *Porrigo lupinosa,* according to

Bateman. He shews, 1*st*, That this disease consists in the aggregation of millions of mycodermatous plants. They are formed of articulated filaments of a diameter from $\frac{1}{1000}$ to $\frac{1}{250}$ of a millimetre; they spring from an amorphous mass of which the periphery of each capsule of Tinea is composed, and give off towards its centre oblong or round homogeneous corpuscles, which are the reproductive spores. The longitudinal diameter of the sporules is from $\frac{1}{300}$ to $\frac{1}{100}$ of a millimetre, and the transverse is from $\frac{1}{300}$ to $\frac{1}{150}$. The cells of the tubes sometimes contain small round transparent molecules, of a diameter varying from $\frac{1}{10000}$ to $\frac{1}{1000}$ of a millimetre. 2*dly*, The seat of these vegetations is in the cells of the epidermis. The true skin is compressed, not destroyed; and the bulbs and roots of the hairs are only secondarily affected. 3*dly*, The disc of the capsule, which is not at the commencement perforated, opens by a small hole in the centre. This enlarges, and the plants push through it, so that, at a more advanced period, instead of there being a central depression in the capsule, there is a convexity, and its edges disappear. 4*thly*, He inoculated 30 phanerogamous plants, 24 silk-worms, 6 reptiles, 4 birds, and 8 mammifera, but only induced the disease once, and then in a plant. The human arm was inoculated five times, but, independent of a slight inflammation and suppuration, no effect was produced.

On reading the above observations last autumn, I examined the crusts on the head of a boy who laboured under the disease, and immediately detected the cylindrical and ramified appearances described by M. Gruby. With a view of determining the real nature of this affection, and observing the manner in which the fungi germinated, I was desirous of making a few observations on this case, and Dr Henderson, who had charge of it, obligingly consented to suspend for a time all active treatment.

Observation 1*st*. All the crusts were removed from the head by the application of poultices. In a few days the scalp was quite clean, presenting here and there anteriorly patches about the size of half-a-crown deprived of hair. In these bald portions of the scalp the skin looked somewhat injected and glossy on the surface; but there was no pain on pressure, no abrasion in the skin, or other symptom of inflammation or local lesion. The disease was now allowed to take its natural course, and I watched its development daily. In two days, minute pustules were observed to be thinly scattered over the surface, the contents of which, when examined under the microscope, were found to consist of normal pus. In two days more, the number of pustules had considerably increased, and those formerly observed had become larger. I surrounded several of the latter with a ring of ink, in order that there might be no difficulty in following the changes they underwent, and distinguishing them from others. In another day two of them broke, and the matter exuded formed a scab, which, under the microscope, was found to be composed of epidermic scales and irregular amorphous masses, without any trace of vegetable structure. In the interstices of these scabs,

the scalp was covered with a furfuraceous desquamation, consisting only, as shewn by the microscope, of epidermic scales. On the sixth day, the scabs were of a dirty yellow colour, but not of the peculiar tint or form of the porrigo crust. Only a few pustules remained, and the injected appearance of the skin was gone. On the tenth day, the head was covered with irregular agglomerated scabs, similar to those produced from impetigo. The separation of numerous epidermic scales, constituting a furfuraceous desquamation, also continued. On the twelfth day, I detected for the first time, at the posterior part of the scalp where the hair was most abundant, small bright yellow spots, the size of a pin's head, somewhat depressed below the surface. On removing one of these spots with the point of a lancet, and examining it by means of a biconvex lens of an inch focus, I found a smooth, cupped-shaped, bright yellow capsule, the diameter of which was about $\frac{1}{20}$ of an inch. Its margin was continuous with several epidermic scales, which it was necessary to cut or tear through before the capsule could be removed. Having done this, it was readily separated from the parts below, except where the hair which usually perforates these crusts connected it inferiorly with the dermis. On pulling this out, or cutting it through, the capsule could be removed entire, leaving behind it a reddened inflamed concave depression, corresponding to the convexity of its inferior surface. Its removal gave rise to the effusion of a thin greyish looking serum, which soon concreted on the surface. On placing this capsule in a drop of water, pressing it between two slips of glass, and examining it with a magnifying power of 300 diameters, it was found to be composed of an amorphous mass, in which were numerous long-jointed filamentous tubes. These were seen coming from the edge of the capsule, as M. Gruby has described. (Plate VI. fig. 3.) At this time there was no appearance of beaded filaments, composed of round or oval globules. These did not appear until three days later, at first isolated, and then in groups and chains. (Plate VI. figs. 5 and 6.) The further development of the plants, and of the disease, appeared to be exactly as M. Gruby has described it.

Observation 2d. In a boy of well marked scrofulous habit, labouring under the Porrigo lupinosa in its most characteristic form, the crusts over the two anterior thirds of the scalp, where it was for the most part bald, were numerous, round, and isolated, but matted together posteriorly where the hair was still abundant. When examined microscopically the mycodermatous vegetations were immediately detected as in the last case. All the crusts were removed by the application of poultices, and the head rendered perfectly smooth and clean. In three days, a furfuraceous desquamation of the cuticle appeared, which became more and more abundant until the eighth day, when the small bright yellow spots of the porrigo made their appearance, not having been preceded by the formation of any pustules. The crusts were removed several times in succession, and the disease again allowed to appear ; but in this case the appearance of the peculiar porrigo crusts was never preceded by that of pustules.

In several other cases which have come under my observation, I have satisfied myself that the formation of pustules is not essential to the disease, although they are often present. Hence the mistake of those pathologists who classified Porrigo lupinosa amongst the Pustulæ. M. GRUBY says that pustules are *never* present, which is equally erroneous, although they appear to be a secondary result, attributable to the irritation the disease produces in some individuals. On the other hand, I have never seen the disease produced without having been preceded by desquamation of the cuticle, an observation which appears to me of some importance, inasmuch as, if true, the disease ought to be classed amongst the *Squamæ*.

According to M. GRUBY, the plants grow in the substance of the epidermis. I have made observations to determine the correctness of this statement, and found that the whole inferior surface of the capsule is formed of epidermic scales, thickly matted together. These are lined by an amorphous, finely granulated matter, from which the plants appear to spring, and which unites the branches and sporules together *en masse*. Superiorly, however, the epidermic scales are not so dense; and I have always found them more or less broken up, and not continuous. The observations just described are here valuable, as indicating the probable mode in which these plants, or the sporules producing them, are deposited on the scalp. It will be seen that the appearance of the peculiar porrigo capsule was invariably preceded by a desquamation of the cuticle, that is, a separation or splitting up of the numerous external epidermic scales which constitute its outermost layer. Hence, it is more probable that the sporules or matters from which the vegetations are developed insinuate themselves between the crevices, and under the portion of epidermis thus partially separated, than that they spring up originally below, or in the thickness of the cuticle.

M. GRUBY accurately describes the mode in which the capsule is formed by the continual growth of the mycodermatous plants, but he says little regarding the manner in which the plants themselves are developed. According to my observations, as soon as the small yellow crust becomes visible, it consists of the outer capsule, formed by epidermic scales, with a layer of amorphous, very finely granulated matter within it, from which spring numerous jointed tubes. Sporules do not appear until later, varying from two to four days; and their presence in any quantity may be detected by the eye, from their presenting a whitish colour, as M. GRUBY pointed out. In order to examine the development of these vegetations microscopically, it is necessary to make a very thin section of the capsule, completely through, embracing the outer layer of epidermis, amorphous mass, and light friable matter found in the centre. It will then be found, on pressing this slightly between glasses, that the cylindrical tubes spring from the sides of the capsule, proceed inwards, give off branches which in turn terminate in round

or oval globules. Fig. 1, plate VI. represents a portion of such a section, and fig. 7 shews how these globules or sporules are given off in various ways. I have seen some oval bodies about twice the size of the others, and some round, both distinctly nucleated. (Fig. 6, *a.*) The long diameter of the former measured $\frac{1}{75}$ of a millimetre. The sporules agglomerated in masses are always more abundant, and highly developed in the centre of the crust. The cylindrical tubes, on the other hand, are more readily found near the external layer. I have occasionally seen swellings on the sides of the jointed tubes, but whether these are sporules, or the commencement of branches, remains still undetermined.

Remembering the ill success of M. GRUBY's inoculations, I thought it right to try whether the disease could be propagated in another part of the individual already affected; because, if not susceptible of extending in a person already predisposed, it was not likely to be caught by one in perfect health. I accordingly made a small puncture in the neck of the boy first spoken of, about an inch below the occipital protuberance, and an inch and a half from the large masses of crusts connected with the scalp. I introduced through this puncture, under the cuticle, some of the broken down yellowish-white friable matter found in the centre of the capsules, which consists principally of the sporules of the plant. The wound healed up in a few days without presenting any thing abnormal. I also inoculated my own arm in the same manner, but without any result. I repeated these inoculations twice on the boy and on myself with the matter of the pustules, instead of that of the crusts, but in every case without success.

It then occurred to me that, as the disease usually appeared in the hairy scalp, it might be more readily produced in that part of the integuments. I therefore had my own scalp inoculated in two places with the pus taken from one of the pustules. It excited inflammation, suppuration, and ulceration. The matter discharged formed hard scabs, which, however, in no way resembled those of the porrigo, or exhibited vegetations when examined with the microscope. After continuing three weeks, during which period one of the sores extended to the size of a shilling, and both ulcerations still spreading, they were destroyed by the frequent application of caustic. I subsequently had my head inoculated with the sporules of the mycodermata, but the wound healed up completely without producing any appreciable result.

I subsequently rubbed a mass of the white friable matter, constituted of the sporules, upon the arm, so as to separate several of the epidermic scales, and induce erythematous redness. Slight superficial abrasions were produced, which healed in a few days, without presenting any evidence of the mycodermata having germinated. I also sprinkled the sporules over an extensive accidental abrasion on the leg, which, however, healed up in the usual manner.

Thus, in none of these experiments, performed in various ways, on different portions of the surface, and frequently repeated so as to avoid fallacy, could I

succeed in causing the plant to germinate on parts different from those which originally produced it. In other words, I could not communicate the disease to other individuals, or from one part of the same individual to another, although it is generally conceived to be of a highly contagious nature.

I am not aware that this peculiar disease has ever been shewn to exist on any other animal than man, and we shall hereafter see, that whilst parasitic vegetable growths have been described as occurring on insects, fishes, reptiles, and birds, their occurrence in the inferior mammals has, with one exception, escaped notice. It is important, therefore, to mention, that I have observed crusts upon the face of a living common house-mouse, similar in every respect to those which constitute the Porrigo favosa in man. The crusts were of a more irregular form, prominent in the centre, not forming distinct capsules or perforated by a hair. They formed a prominent whitish friable mass on the left side of the face of the animal, about the size of a small bean. Examined microscopically, they presented the cylindrical tubes and sporules *en masse*, in every respect identical to those which grow on the scalp of man.

It has been noticed by every writer on the subject, that the odour of the crusts of Porrigo favosa is similar to that of mice, and this is so peculiar as not readily to be mistaken. It is singular, then, that the mycodermatous plant, constituting this disease, should be found growing on these animals. Whether the disease be peculiar to Man and the Rodentia? whether it be communicable from one to the other, or among the latter class of animals? are questions only to be answered by future researches.

II.

Description of a Cryptogamic Plant found growing in the sputa and lungs of a man who laboured under Pneumothorax.

In numerous microscopic examinations of tubercle, tuberculous sputa, and the lining membrane of tubercular cavities in the lungs of man, I had often observed long filaments, which were evidently the softened shreds of the cellular tissue constituting the natural texture of the lung. On some occasions, however, I observed fragments of tubes, somewhat larger, more or less matted together, which appeared distinctly jointed, and which led me to suppose that a vegetable structure must occasionally be developed in the matter of tubercle found in the lungs. I am now enabled to put the truth of this supposition beyond doubt, whilst circumstances render it highly probable, if not certain, that in the individual to whom I am about to refer, these fungi were developed before death.

In examining the sputa of a man in the Royal Infirmary, the most beautiful and regular vegetable structure was observed. The individual laboured under phthisis in its last stage, with pneumothorax. On simply placing a drop of the inspissated purulent-looking matter, discharged by expectoration, between two

slips of glass, and examining it with a magnifying power of 300 diameters, long tubes, jointed at regular intervals, and giving off several branches, could be seen. They varied in diameter from $\frac{1}{100}$ to $\frac{1}{200}$ of a millimetre, and appeared to spring, without any root, from an amorphous soft mass. Their edges were distinctly defined, and the joints composed of distinct partitions, the tubes being in that part constricted somewhat like certain kinds of bamboo. They were very transparent, and some management with the diaphragm of the instrument was necessary to shew them distinctly. They did not appear to contain granules or nuclei. (Plate VII. fig. 1.)

Interspersed amidst these tubes were numerous round and oval globules, often $\frac{1}{75}$ but generally $\frac{1}{100}$ of a millimetre in diameter, which here and there assumed the form of bead-like rows. (Fig. 5.) On one occasion I found a perfect branch of the jointed tubes connected with a bunch of these, but this was evidently accidental.

Both the jointed filaments and sporules were developed in great abundance on the sides of the spit-box containing the man's sputa, which, in this situation, was inspissated, and presented a yellowish coherent and viscous layer. Here they were often matted together, and presented the appearance drawn, Plate VII. fig. 2.

Two days afterwards the man died, and the left lung was found studded with cavities of different sizes, some of which communicated, by fistulous openings, with the cavity of the pleura. Several of the smaller cavities were partly filled with soft tuberculous matter, readily separable from the lining membrane. On examining this matter microscopically thirty-six hours after death, exactly the same appearances presented themselves as have been described. Numerous jointed transparent tubes, here matted together, there isolated, were readily observed, mingled with round or oval corpuscles, which, however, were larger and more developed. Some of these were of an oblong or truncated shape, and appeared to be separated joints of the tubes. (Fig. 6.)

I have no doubt that these vegetations existed in the man's lungs during life; *first*, Because they were apparent in sputa freshly expectorated, and, *secondly*, Because they could not have reached such a state of development, as has been described, in thirty-six hours. They continued to grow and develop themselves in the tubercular matter, after the removal of the lungs from the body, as well as in the matter discharged before death, by expectoration. They appeared to me somewhat analogous to the Penicilium glaucum of LINK, or those fungi so often found covering disorganized animal matter; although the form of the plant, and the mode in which the branches are given off, shew that they are not identical.

III.

On the Structure of a Cryptogamous Plant found growing on the Skin of the Gold-Fish, (Cyprinus auratus.)

For such notices as have already been published connected with the growth of vegetations on living fishes, I must refer to a subsequent part of this memoir. As in no case, however, are details entered into, I am ignorant whether the vegetations or confervæ alluded to are the same as those which I have myself personally examined.

Mr GOODSIR was the first who examined microscopically the vegetations found growing on the gold-fish. The fish he examined was observed to be in a languishing state for some time before death, and to be covered with a white efflorescence, of considerable length, which sprung principally from the dorsal fins and tail, and floated in the water. The animal was dead before being put into his possession. Some days afterwards, he kindly placed at my disposal some of these filaments, which I examined microscopically, and the following are the results.

Viewed with a power of 300 diameters, two very distinct structures were observed. One of these might be called Cellular, the other Non-cellular.

The cellular structure was composed of elongated cells, which varied in thickness $\frac{1}{100}$ to $\frac{1}{50}$ of a millimetre, presenting the appearance of long jointed tubes, which often extended twice across the field of the microscope. They were frequently branched, generally in a dichotomous manner, although sometimes three branches were given off from one joint. Some of the cells were empty, and appeared very transparent; others were full of granules, which varied in size from $\frac{1}{800}$ to $\frac{1}{150}$ of a millimetre in diameter. Every possible degree of variation existed in the quantity of the cellular contents, some being full of granules and opaque, others being partially so, and others again empty and very transparent. In most of the cells, a distinct nucleus existed, which appeared as a transparent vesicle about $\frac{1}{100}$ of a millimetre in diameter. Some contained two nuclei. (Plate VII. fig. 10.) The nuclei were generally (not always) placed at the proximal end of the cell, from which came off sometimes two other cells, more rarely three, giving a branched appearance to these vegetations. (Fig. 12.) On applying pressure, and by means of a little manipulation, the granular matter within any particular cell could readily be made to flow from one end to the other, or forced out by rupturing its walls. These jointed cellular tubes were often grouped together, forming a mesh-work, in which the cells filled with granules; and those which were empty could readily be distinguished from each other by their opaque and transparent appearance. (Plate VII. fig. 8.)

As regards the substance from which this jointed structure arose, it appeared to be an amorphous mass, composed of very minute granules almost identical with the matter found in the capsules of the Porrigo, and tubercular cavities formerly described. It appeared very abundant below the scales from whence the tubes

appeared to spring, and push through the crevices between them. No roots could be observed, and the cells appeared to come out directly from the above granular mass. I could not satisfy myself in what manner these filaments terminated, whether they bore sporules, or ended in bulbous extremities similar to those described by HANNOVER in the confervæ growing on the salamander. The specimen I examined was already so putrid, and the tubes broke so readily, that this point could not be determined.

Intermixed with these vegetations, were numerous long finer filaments, from $\frac{1}{800}$ to $\frac{1}{500}$ of a millimetre in diameter, and uniform in their size throughout. They were very long, sometimes curved, and sometimes matted together so as to form a mesh more or less dense. (Figs. 11 and 12.) Some of these filaments appeared broken or interrupted, although, on pressing the glasses, the interrupted portion moved simultaneously with the other. On increasing the magnifying power to 650 diameters, these portions were found connected by a very delicate sheath, which invested them externally. (Plate VII. fig. 13.)

It was some time before I could make out the origin of these filaments. I at length satisfied myself that they sprang from the sides of the cellular tubes.

IV.

Facts observed by various Authors connected with the growth of Parasitic Vegetables in Living Animals.

Before we can draw any conclusions regarding the origin or mode of growth of fungi in living animals, it will be necessary to inquire into what is at present known on this subject, and see how far the facts already detailed are analogous with the observations of others.

Parasitic vegetables have been found growing in numerous animals, and I shall arrange the facts respecting them according to the class of animals in which they have been found.

Mollusca.—LAURENT[1] has observed cryptogamous vegetations in the eggs of the *Limax agrestis*, which more or less impede the development of the embryo. He has noticed, 1. That the vegetations arise most often from the walls of the internal tunic of the egg, ramify in the albumen, and form in it a net-work, which is sometimes checked and compressed by a vigorous embryo, and sometimes they entwine the embryo in such a way that there is a struggle between the vegetable and animal development. 2. That the vegetable filaments may also be seen to arise from the body of a dead embryo, or of a non-developed vitellus. After having filled the albumen with their ramifications, the vegetations throw out new filaments, which pierce the internal tunic and shell, and prolong themselves from

[1] L'Institut, tom. vii. p. 229.

the egg placed in water, under the form of simple or ramified branches, which are extended to the surface and a little beyond the water. They terminate *en masse.*

VALENTIN[1] also saw confervæ in a state of active growth, for several days, upon the ova of (probably) *Limnius stagnalis*, during which period the embryo was in lively motion, and which did not die till later.[2]

Insects.—LEDERMÜLLER[3] noticed the fact, that on leaving dead flies in water for a certain time, plants spring from the surface of their bodies. Similar observations have been made by WRISBERG,[4] SPALLANZANI,[5] OTTO FRIEDERICH MÜLLER,[6] LYNGBYE,[7] GILL,[8] GÖTHE,[9] NEES VON ESENBECK,[10] and MEYEN.[11]

Parasitic vegetables have also been observed on living insects. On this subject KIRBY and SPENCE[12] justly remark, "that as insects often pass no small portion of their lives in a state of torpidity, in which they remain chiefly without motion, it will not seem strange, should any partial moisture accidentally accumulate upon them, that it affords a seed-plot for certain minute fungi to come up and grow in." Hence, probably, may be explained the phenomenon of the *worm plant*, described by PE'RE PARRENIN and REAUMUR,[13] and of the *vegetable fly* found in Dominica, described by NEWMAN.[14] To this circumstance, also, it seems most rational to attribute the growth on insects of certain species of *Clavaria*, as mentioned by HILL,[15] FOUGEREAUX DE BONDAROY,[16] BÜCHNER,[17] and WESTWOOD;[18] of certain species of *Isaria*, noticed by PERSOON[19] and SCHWEINITZ;[20] of the *Penicillium Fieberi*, figured to exist on the *Pentatoma prasina*, by CORDA,[21] and of the *Sphæria entomorhiza*, noticed by DICKSON, MADIANNA, and HALSEY,[22] and seen by them growing on the *vegetable wasp* of Guadaloupe.

1 Repertorium, vol. v. p. 44.

2 See also GRUITHUISEN, Nova Acta, vol. x. p. 445; who gives a description of, and figures confervæ growing from, a dead *Valvata branchiata*.

3 Mikroskopische Ergötzungen, 1760; pp. 1–90, tab. xlix. fig. 2.

4 Obs. de Animalculis Infusoriis Satura. Goettingæ, 1765; p. 31, fig. 9–2.

5 Opuscules de Physique Animale et Vegetale, tom. i. p. 157.

6 Neue Samml. d. Schriften der Königl. Danischen. Ges. d. Wiss. Copenhagen, 1788; iii. p. 13. And Nova Acta, vol. iv. p. 215.

7 Hydrophylotogia Danica, p. 79, tab. xxii. See also Flora Danica, tab. 896.

8 Technological Repository, vol. iv. p. 331.

9 Heften zur Morphologie, i. p. 292.

10 Nova Acta Physico-Medica, &c. 1831, vol. xv. Pars post. p. 375, tab. 79 and 80.

11 Idem, p. 381, and WIEGMANN's Archives, 1840, p. 62.

12 Entomology, vol. iv. p. 207.

13 Mem. de l'Acad. Roy. des Sciences, 1726, p. 426.

14 Philosophical Transactions, 1764, p. 271.

15 Idem, p. 272.

16 Mem. de l'Acad. Roy. 1769, p. 591.

17 Nova Acta, vol. iii. p. 437, tab. 7.

18 Annals of Nat. History, Nov. 1841, vol. viii. p. 217.

19 Synops. Meth. Fung. 687. g. 63, s. 12.

20 Annals of the Lyceum Nat. Hist. of New York, vol. i. pp. 125-6.

21 Icones Fungorum hucusque cognitorum, Pragæ, 1837, 1840.

22 Annals of Lyceum, Nat. Hist. Soc. New York, vol. i. p. 126.

On some occasions, it would appear, that the so called fungi observed on insects are, in point of fact, constituted of the stolen parts of flowers or plants. Thus BROWN[1] has determined apparent fungi on certain bees, to be composed of the stamina of orchidiæ, and detected the stamen of aristolochia in a beetle shewn to him by Mr M'LAY. SCHLECHTENDAHL and SIEBOLD[2] have recognised an apparent fungus formation on *Eucera Druriella, Zygæna lonicera, Leptura rufipes* and *pubescens*, to be the attached pollen of orchidiæ. BUSK[3] also observed confervæ on the body of a *Dytiscus marginalis*, which had been apparently derived from similar growths infesting some plants of *Valisneria spiralis*, contained in the glass vessel in which it was kept.

The disease in silk-worms, named *Muscardine*, which causes so great a mortality amongst these animals, is characterized by the appearance after death of a white eruption on the body of the animal, which eruption M. BASSI of Lodi[4] shewed to consist of numerous cryptogamic plants. These have received the name of *Botrytis Bassiana*. This fact has been confirmed by ARDOUIN,[5] JOHANNYS,[6] CREVELLI,[7] BONNAFOUS,[8] and TURPIN,[9] whose farther researches, experiments, and observations, have established its contagious nature, not only amongst silk-worms, but amongst insects generally.

Fishes.—In VALENTIN'S Repertorium, it is stated, that EHRENBERG has found *Chætophora (Tremella) meteorica* growing on the scales of the *Salmo eperlanus.*[10] Beyond this notice, however, I know nothing of the observations alluded to. VALENTIN remarks, that mouldiness, or colourless confervæ, occurring on the ova of fishes, constitutes a very powerful preventive to their development, and its progress is so rapid, that a single egg infected with it will in a very short time infest many hundreds, and thus destroy them. In the *Cyprinus nasus* also, when kept in narrow vessels, and the water not quite sweet, he observed the same fungus on all parts which might be abraded, as, for instance, the head and tail.

Mr J. T. COOPER has informed the Editor of the Microscopic Journal that he has frequently removed from the gills of gold-fish, kept in a cistern in his garden, a quantity of confervæ, the rapid growth of which over the whole surface of their bodies in every instance caused death.[11]

Reptiles.—VALENTIN[12] has seen colourless confervæ growing on the ova of

[1] KIRBY and SPENCE, Entomology, vol. iv. p. 208.
[2] Frorieps Notizen, No. 224–73.
[3] Microscopic Journal, vol. i. p. 149.
[4] Del Mal. del Segno calcinaccio o Muscardino, sec. ed. Milano, 1837.
[5] Annales des Sciences Nat. vol. viii. p. 229, 1837.
[6] Idem, vol. ii. p. 81, 1839.
[7] Linnæa, herausgegeben von Schlechtendahl, Halle, 8. 118-23.
[8] L'Institut, tom. vii. p. 154.
[9] Idem, tom. vii. pp. 199-200.
[10] Vol. 5. p. 44. See also Frorieps Notizen, No. 218, 314.
[11] Microscopic Journ. vol. i. p. 149.
[12] Repert. vol. v. p. 44.

Alytes obstetricans, similar to those which grow on the eggs of fishes. HANNOVER of Copenhagen[1] observed confervæ growing on the living salamander (*Triton punctatus*) when the tail was partly cut off, or the spinal cord divided so as not to occasion death. They sprung from the surface of the wound, and spread more or less over the integuments. Examined with the microscope, they were found to consist of membranous, simple, very seldom or never branched tubes, with corpuscular contents. The tubes varied in thickness, were conical at their extremities, but sometimes swoln and bulbous. There was often present a cellular formation, either springing from the walls of the tube, or from regular notches in the sides. Sometimes the tube was full, at others almost empty, the latter generally when the confervæ were ripe, and then they hung on the outer side of the tube.

STILLING[2] of Cassel saw similar confervæ on the toes of frogs, when the greater part of the inferior half of the spinal cord had been removed. From the first appearance of the efflorescence the animals, previously lively, became evidently weak, and made very slow movements. STILLING considers this structure to be of an animal nature. The granules contained in the cells possess movements, and he conceives them to be ova, from which small infusoria issue. He thinks the tubes are albuminous or fibrinous, and serve as a nidus in which the ova become developed. He states further, that the cellular structures which have been perceived in these tubes are Vorticellæ, and that he has seen them burst and discharge their contents.

I have often seen similar animalculæ to those described by STILLING in fluids which have become putrid by long contact with animal matter. They are often present without any trace of vegetation, so that the latter are by no means necessary for their existence. I think that he is in error in supposing that the granules are ova; that the dichotomous branches are composed of albumen or fibrin; or that the cellular structures are vorticellæ. The granules, tubes, and joints are, in my opinion, in every way analogous to those found in vegetable structures. That animalculæ may occur within them, as well as in the surrounding fluid, there can be no doubt, but their appearance is a subsequent phenomenon.

This opinion of mine is confirmed by more recent observations of HANNOVER.[3] According to him, the movements described by STILLING in the contents of the cell, are of a molecular nature. He denies that they are the eggs of infusoria, and points out that the larger worms described by STILLING, are ascarïdes from the intestines of the animal. In this memoir, also, he minutely details the mode in which the plants are developed.

[1] MÜLLER's Archives, 1839, p. 338.

[2] Idem. 1841, p. 279; and Lond. and Edinb. Month. Journ. of Med. Science, October 1841.

[3] MÜLLER's Archives, January 1842.

I think it more probable, as previously expressed, that the animalculæ alluded to by STILLING are infusoria rather than ascarides from the intestines.[1]

Birds.—OWEN,[2] on dissecting a flamingo, observed a green vegetable mould or mucor, growing on the lining membrane of tubercular cavities in the lungs, and on that of the smaller ramifications of the bronchial tubes. Thus, he remarks, *Entophyta* exist in animals as well as *Entozoa*.

EUDES DESLONGCHAMPS[3] noticed a similar appearance growing in an albuminous layer, which was effused on the membranous lining of the air-passages in an eider-duck. Examined with the microscope, it was found to consist of transparent inarticulate filaments, slightly or not at all ramified, forming an inextricable felt. There everywhere existed throughout this felted mass, a great number of small globular or ovoid vesicles, which were undoubtedly the sporules. Some of the filaments which were isolated were observed to support sporules with a capitulum, whilst others terminated in a flat margined disk, which appeared to be the mode in which those filaments terminated which had lost their sporules.

A vegetable mould has also been observed by ROUSSEAU and SERRURIER,[4] growing on a species of false membrane, effused between the intestines and vertebral column of a male parroquet. Its adhesion was so feeble, that on blowing upon it, it disappeared like the finest and lightest powder. They remark that pigeons (more particularly the hens) are very liable to be affected, if these animals inhabit cold and moist places, and in epochs of the rainy season.

Mammalia.—SERRURIER and ROUSSEAU merely mention having seen a vegetable mould in a hind *(Cervus Axis)*, but its nature, or on what part of the animal it grew, is not stated. With this exception, and that of the mouse alluded to, p. 282 of this memoir, parasitic vegetations amongst the mammalia have hitherto only been found growing in man.

SCHÖNLEIN of Berlin[5] was the first to recognise and figure a vegetable structure in the crusts formed on the scalp, in the disease named Porrigo lupinosa, by BATEMAN. REMAK,[6] however, has claimed the priority of this discovery.

[1] CARUS has figured and given a description of confervæ growing on a dead salamander. (Nova Acta, vol. ii. p. 493.) I may here observe, that similar confervæ may be produced at will, by allowing frogs or gold fish to remain, after death, some time in water without changing it. In a few days they become covered with a white efflorescence, which, examined microscopically, is found to consist of long transparent tubes, jointed at regular intervals. I have frequently examined confervæ thus produced: they are different from those which grow on these animals during life.

[2] Philosophical Magazine, 1833. Vol. ii. p. 71.

[3] Annales des Sci. Nat., Juin 1841, and Jameson's Journal, 6th October 1841.

[4] Comptes Rendus, tom. xiii. p. 18.

[5] Müller's Archives, 1839, p. 82.

[6] Valentin's Repertorium, 1841, vol. vi. p. 58. Med. Zeit., No. xvi. p. 73, 74.

This observation was repeated and confirmed by FUCHS and LANGENBECK[1] of Göttingen, who, it is stated, not only found vegetations in the crusts of the Porrigo lupinosa, but in the majority of skin diseases belonging to scrofulous affections. They have been seen also by TEXTOR.[2]

It is to GRUBY of Vienna,[3] however, that we are indebted for the most perfect description of these vegetations, and for new researches on this subject, the results of which have been condensed in the first section of this memoir.

LANGENBECK[4] observed a high degree of fungus development in the body of a man who died labouring under typhus. It extended from the amygdalæ, and upper part of the pharynx, through the œsophagus down to the cardia, and consisted of cellular non-granular filaments, with superposed nucleated (*gekernten*) sporules. On the intestinal ulcers of the ilium and cœcum, there also appeared to exist isolated filaments and sporules, which last were also present in the intestinal contents. It is not stated that these vegetations occurred during the life of the individual.

MEYNIER of Orleans[5] has put forth the opinion, that warts in man are true *gymnosporanges*, and that other human diseases are equally due to the development of different species of cryptogamous plants. This is the case in many of the dermatose affections, especially those of a scaly form. Certain tubercles appear to him similar to the *lycopodaciæ*, and in other cases it is the *uredines* which produce disease. The facts on which such opinions are founded, however, are unknown to us.

ESCHRICHT of Copenhagen[6] has stated that vegetations sometimes exist in the disease called aphtha. GRUBY[7] has also announced the discovery of another cryptogamous plant, distinct from that found in the porrigo, in a skin disease. No details of these observations, however, are given.

V.

Conclusions deduced from the facts which have been detailed.

Are we to consider, that these fungi draw their nourishment from the living animal tissue, and originate disease, or that they are deposited and grow in the inorganic products occasionally found in the textures, and are the results rather than the cause of morbid actions? In attempting to answer this difficult question, it is not my intention to discuss the disputed subject of equivocal generation.

[1] Comptes Rendus de la Polyclinique de Göttingen. Ann. Hannov. de M. Holscher. 1 cap. 1840. Fuchs, Traité des Maladies de la Peau, Göttingen 1840.

[2] Comptes Rendus, tom. xiii. p. 220. [3] Idem.

[4] Frorieps Notizen, No. 252, p. 145, 47, and Valentin's Repèrtorium, vol. v. p. 46.

[5] Comptes Rendus, vol. xiii. p. 311. [6] Jameson's Journal, October 1841.

[7] Comptes Rendus, vol. xiii. p. 388.

My purpose will be sufficiently answered by alluding to the well-known fact, that cryptogamic plants spring up on such portions of decayed animal or vegetable matter as present certain conditions necessary for their germination. I shall endeavour to shew that the fungi found growing in living animals never spring from the healthy textures, but from certain morbid products which appear to furnish the conditions essential for their sustenance. It is also probable that these morbid products are invariably of an albuminous nature, and that the constitutional disorder predisposing to their production, at least in the higher animals, is scrofula.

In man all the vegetations yet discovered have been found connected with the matter effused into the textures in scrofulous constitutions. The fungi found by myself, for instance, growing in the tuberculous cavities of the lungs, and those discovered by Schönlein, and described by Gruby, constituting scrofulous eruptions on the skin, grew on a finely granular amorphous mass, which presented no evidence of organization. Chemical researches have shewn that this form of tubercular matter is principally composed of albumen, which explains the large proportion of this animal principle present in the crust of the Porrigo or Tinea favosa, according to the analysis given by Alibert. The fungi found by M. Eudes Deslongchamps growing on the membranous lining of the air-passages in an eider-duck, sprung from an "albuminous layer"—"forming the soil on which they grew." The mould or mucor discovered by Owen growing in the lungs of the flamingo, occupied the same situation as those observed by myself in the lungs of man, viz. the lining membrane of tubercular cavities. The fungi found by MM. Rousseau and Serrurier in the parroquet, grew on a species of false membrane. What the nature of this membrane was, is not stated, but it is distinctly mentioned that the animal died of laryngeal and pulmonary phthisis. In pigeons, also, the same authors describe it as commonly induced by exposure to cold and moisture, circumstances well known to be the most common cause of scrofula and of tubercular depositions. According to the observations of Valentin, the parasitic confervæ found growing on fish are connected with a diseased state of the animal, and are induced by keeping them in narrow vessels and foul water. The gold-fish was evidently unhealthy which furnished the vegetations which I have myself described, and I have shewn that these were connected with a granular, inorganic, albuminous matter, identical with that found in the lungs of phthisical individuals, and in the crusts of Porrigo favosa. The salamanders and frogs in which confervæ grew, as described by Hannover and Stilling, were decidedly unhealthy, and induced by circumstances which must necessarily impair the vigour of animal life, and induce a state of cachexia. Vegetations attach themselves to, or grow on, insects generally when in a chrysalis state, that is, when the powers of life are sluggish or dormant. When seen in these animals during the most perfect period of their existence, they evidently laboured under disease and soon perished. This was distinctly observed in the vegetable wasp of Guadaloupe by

MADIANNA, and in the Dytiscus marginalis by BUSK. Lastly, the discovery of M. BASSI has demonstrated that vegetations occur in silk-worms affected with the muscardine, a disease which causes a great mortality among these animals; and the researches of M. ARDOUIN have shewn that these vegetations are formed at the expense of the adipose tissue. Whether tubercular matter was present in the worms is not stated; but we know that the disappearance of fat is one of the constant symptoms attendant upon imperfect nutrition.

It appears probable, therefore, 1*st*, that these vegetations always arise in living animals *previously* diseased; 2*d*, that their presence indicates great depression of the vital powers, and impairment of the nutritive functions of the economy; 3*d*, that the peculiar constitution or cachexia favourable to their growth is the tubercular or scrofulous in the mammalia, birds, and fishes, and most probably in reptiles and insects; and, 4*th*, that the therapeutic indications are, 1. to invigorate the system, and, 2. to apply locally, if possible, such applications as tend to destroy vegetable life.

NOTE.

On a cryptogamic Plant found growing in the sordes collected on the Teeth and Gums of individuals labouring under Typhus Fever.

Since the foregoing Memoir was read to the Society, I have found more than once a vegetable structure in the black deposit which collects on the Teeth and Gums of individuals in the last stage of Typhus Fever. They are similar to those which I have described as springing from tubercular matter, but not so large, the diameter of the tubes being from $\frac{1}{300}$ to $\frac{1}{150}$ of a millimetre. Their mode of development appears also to be similar; the partitions towards their extremities becoming more numerous, and these being terminated by a chain of oval sporules. Distinct mollecules from $\frac{1}{800}$ to $\frac{1}{300}$ of a millimetre in diameter could be distinguished in the cells, and in some of the elongated sporules.

Fig. 8.

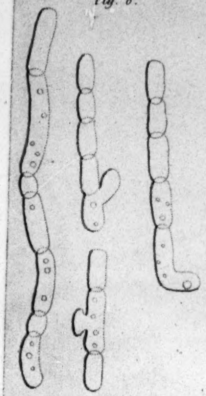


Fig. 1.

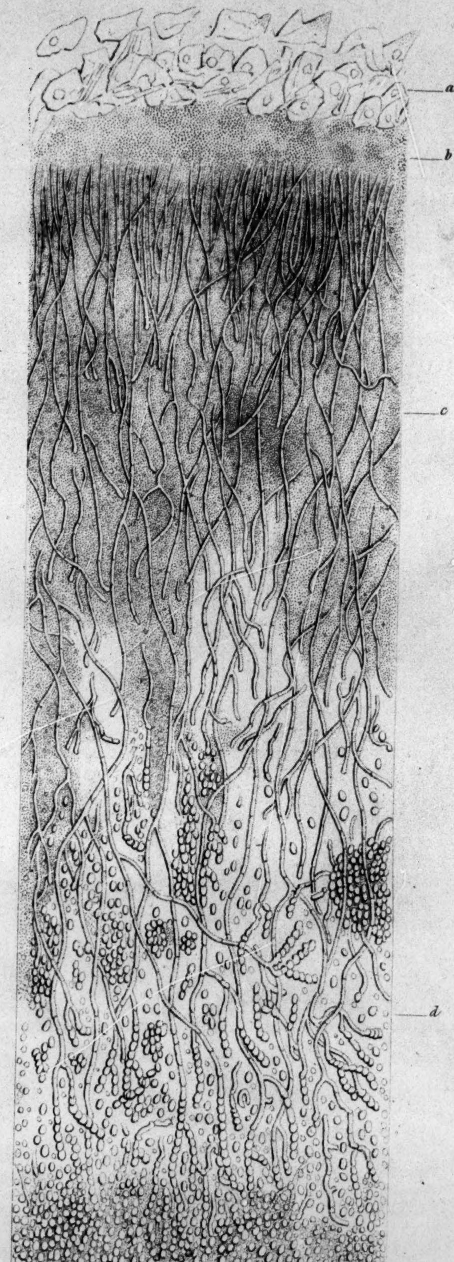


Fig. 2.

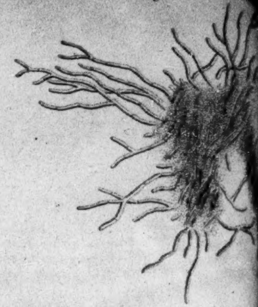


Fig. 4.

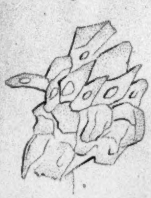


Fig. 3.

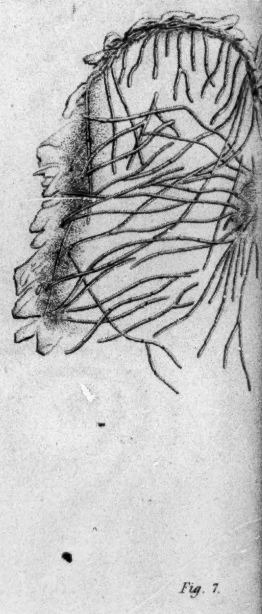
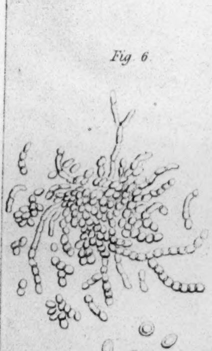


Fig. 5.

Fig. 7.

Fig. 6.



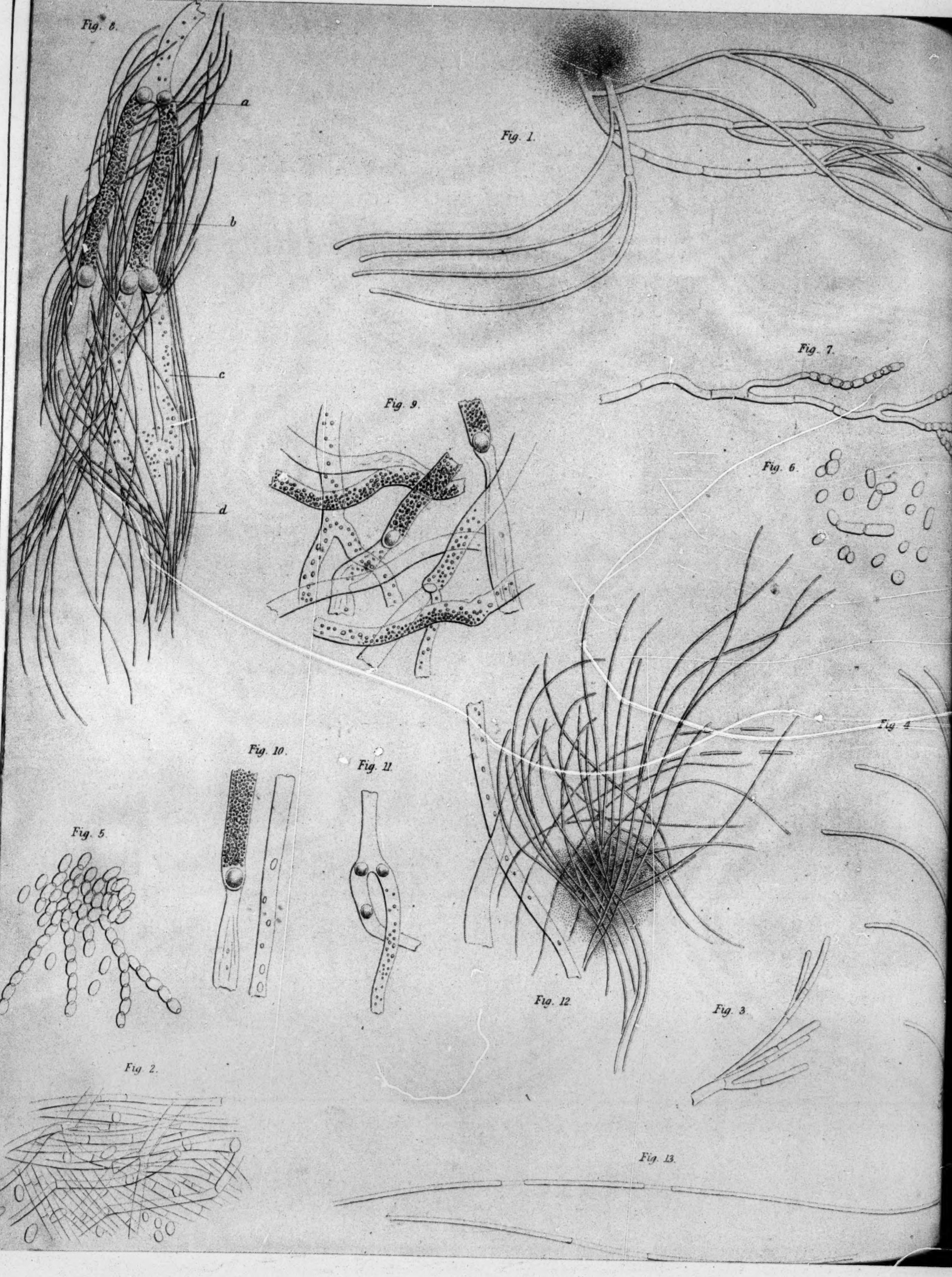


Fig. 8.
a
b
c
d
Fig. 1.
Fig. 7.
Fig. 9.
Fig. 6.
Fig. 4
Fig. 10.
Fig. 11.
Fig. 5.
Fig. 12.
Fig. 3.
Fig. 2.
Fig. 13.

EXPLANATION OF THE PLATES.

PLATE VI.

This Plate illustrates the mode of development of the mycodermatous vegetations growing in the capsules of the *Tinea favosa* or *Porrigo lupinosa* of Bateman.

Fig. 1. Appearance of the mycodermata in the Porrigo lupinosa, on making a thin section perpendicularly through the capsule. *a* Epidermic scales at the periphery of the capsule; *b* amorphous granular matter, rendered apparently of considerable width from being pressed between glasses; *c* the cylindrical jointed tubes matted together by the amorphous mass; *d* the cylindrical tubes and sporules separated by means of pressure. The former are seen branching and giving off sporules at their extremities. There are also seen numerous loose branches and sporules, isolated or grouped together. *e* Sporules *en masse* towards the centre of the capsule.

Fig. 2. Cylindrical tubes coming from the granular mass, found near the edge of the capsule.

Fig. 3. Similar tubes found at an early period of the development of the capsule. They are seen springing from the edge to which epidermic scales are attached.

Fig. 4. Mass of epidermic scales from the inferior surface of the capsule.

Figs. 5. and 6. A small portion of the whitish friable matter, consisting almost wholly of sporules, found in the centre of the capsule. *a* Larger, round, and oval sporules, having a distinct nucleus.

Fig. 7. Isolated branches, occasionally found floating loose in the field of the microscope, shewing the mode in which sporules and branches are given off from the cylindrical tubes.

Fig. 8. Similar branches, magnified 800 diameters, shewing the molecules occasionally found within the cells.

All the above figures, with the exception of fig. 8., *exhibit the structures as they appear when magnified* 300 *diameters.*

PLATE VII.

This plate gives the appearance of the vegetable structures found growing in the human lung, and on the skin of the gold-fish.

Fig. 1. A beautiful specimen of the plant found in the sputa before death, springing from the amorphous albuminous mass.

Fig. 2. Jointed tubes matted together, mixed with sporules, found in the soft cheesy matter in a tubercular cavity.

Figs. 3. and 4. Other specimens of the plant taken from the lung. In fig. 4. some sporules are seen accidentally attached.

Fig. 5. A mass of sporules, some arranged in bead-like rows.

Fig. 6. Sporules isolated of different forms.

Fig. 7. Branch of the plant giving off sporules.

Fig. 8. Portion of the plant found growing on the skin of the gold-fish. *a* Transparent vesicular nucleus; *b* cells full of granules; *c* cells more or less empty; *d* the non-cellular filaments.

Fig. 9. Portions of cellular tubes more or less matted together; some full of granules, others empty and collapsed.

Figs. 10. and 11. Other portions of the cellular tubes isolated. In fig. 11. two nuclei are seen in one cell.

Fig. 12. A mesh of the non-cellular filaments. *a* Amorphous, firmly granular, matter; *b* a filament apparently interrupted.

Fig. 13. Non-cellular filaments, magnified 650 diameters, shewing the delicate sheath which invests them.

All the above figures, with the exception of fig. 13., *exhibit the structures as they appear when magnified* 250 *diameters.*

XVIII.—*On the Ultimate Secreting Structure, and on the Laws of its Function.* By JOHN GOODSIR, M.W.S., Conservator of the Museum of the Royal College of Surgeons, Edinburgh.

(Read 21st March 1842.)

MALPIGHI was the first to announce that all secreting glands are essentially composed of tubes, with blind extremities. MÜLLER, by his laborious researches, has brought this department of the anatomy of glands to its present comparatively perfect condition. PURKINJE announced his hypothesis of the secreting function of the nucleated epithelium of the gland ducts, but made no statement to shew that he had verified it by observation. SCHWANN suggested that the epithelium of the mucous membranes might be the secreting organ of these surfaces. HENLE described minutely the epithelium cells which line the ducts of the principal glands and follicles, but did not prove that these are the secreting organs. The same anatomist has stated, that the terminal extremities of certain gland ducts are closed vesicles, within which the secretion is formed, and which contain nucleated cells. HENLE has not, therefore, verified the hypothesis of PURKINJE, although he is correct in stating that the terminal vesicles of certain gland ducts are closed. It will be shewn, in the course of this paper, that the secretion is not formed, as HENLE has asserted, in the closed vesicles, but in the nucleated cells themselves.

The discrepant observations of BOEHM and KRAUSE on the glands of PEYER, were in some measure reconciled by HENLE, who referred them to the same class of structures, as the closed vesicular extremities of the ducts of compound glands. Dr ALLEN THOMSON has made the important observation, that the primitive condition of the gastric and intestinal gland is a closed vesicle. WASMANN described the structure of the gastric glands in the pig; and his description will be fully explained by the observations and views contained in the present paper. HALLMAN has given a detailed account of the testicle of the ray, which closely resembles that of the *Squalus cornubicus*, as described in another part of this communication. He found the vesicles closed, but did not detect the mode of development of the spermatozoa, or the continual growth of the gland itself. None of the recent observations on the development of the spermatozoa have proved, that the vesicles, in which they are formed, are the epithelium cells of the ducts of the testicle. I am indebted to Dr ALLEN THOMSON for directing my attention to a notice in VALENTINE's Repertorium, 1841, of a Dissertation by ERDL, de Heli-

cis Algiræ vasis sanguiferis, 1840, in which he describes, in the kidney of that mollusk, cells, the nuclei of which pass out by the duct of the gland. It does not appear, however, that ERDL had discovered the uric acid within the cell.

I have now stated all that is known at present of this subject. No one, as far as I am aware, has proved that secretion takes place within the nucleated cell, or has pointed out the intimate nature of the changes which go on in a secreting organ, during the performance of its function. It is the object of the present communication to supply this deficiency in physiological science.

If the membrane, which lines the secreting portion of the internal surface of the ink-bag of *Loligo sagittata* (LAMARK) be carefully freed from adhering secretion by washing, it will be found to consist almost entirely of nucleated cells, of a dark brown or black colour. These cells are spherical or ovoidal, and measure from 1.000th to 2.000th of an inch in their longer diameters. Their nuclei consist of cells, grouped together in a mass. Between these composite nuclei, and the walls of their containing cells, is a fluid of a dark brown colour. This fluid resembles, in every respect, the secretion of the ink-bag itself. It renders each cell prominent and turgid, and is the cause of its dark colour.

The dilated terminal extremities of the ducts in the liver of *Helix aspersa* (MÜLLER) contain a mass of cells. If one of these cells be isolated, and examined, it presents a nucleus, consisting of one or more cells. Between the nucleus and the wall of the containing cell, is a fluid of an amber tint, and floating in this fluid are a few oil globules. This fluid differs in no respect from the bile, as found in the ducts of the gland. The cells measure from 1.000th to 2.000th of an inch.

If a portion of the ramified glandular organ, which opens into the fundus of the stomach of *Uraster rubens* (AGASSIZ) be examined, its internal surface is found to be lined with cells; between the nucleus of each of which, and the wall of the cell itself, a dark brown fluid is situated. The organ secretes a fluid, supposed to be of the nature of bile. The cells measure from 4.000th to 5.000th of an inch.

The dark brown ramified cæca of the same animal exhibit on their internal surfaces an arrangement of nucleated cells, the cavities of which contain a brown fluid. These cæca are also supposed to perform, or to assist in the performance of the function of the liver.

The liver of *Modiola vulgaris* (FLEMING) contains masses of spherical cells, measuring about 3.000th inch in diameter. Between the nucleus and the wall of each of these cells, a light brown fluid is situated, bearing a close resemblance to the bile in the gastro-hepatic pouches.

The nucleated cells, which are arranged around the gastro-hepatic pouches of the *Pecten opercularis*, are about 2.000th of an inch in diameter, irregular in shape, and distended, with a fluid resembling the bile.

The hepatic organ, which is situated in the loop of intestine of *Pirena Pru-*

num (FLEMING), consists of a mass of nucleated cells. These cells are collected in groups, in the interior of larger cells or vesicles. The nucleated cells measure about 3.000th of an inch, and are filled with a light brown bilious fluid.

The hepatic organ, situated in the midst of the reproductive apparatus, and in the loop of the intestine of *Phallusia vulgaris* (FORBES and GOODSIR), consists of a number of vesicles, and each vesicle contains a mass of nucleated cells. These cells measure about 2.000th of an inch, and contain a dark brown bilious fluid.

The hepatic organ, in the neighbourhood of the stomach, in each of the individuals of the compound mollusk, the *Alpidium-Ficus* (LINNÆUS), consists of nucleated cells, which measure 4.000th of an inch in diameter, and contain in their cavities a reddish brown fluid.

The liver of *Loligo sagittata* (LAMARK), contains a number of nucleated cells, ovoidal and kidney-shaped, ranging in diameter from 1.000th to 3.000th of an inch. These cells are distended with a brown bilious fluid.

The nucleated cells in the liver of *Aplysia punctata* (CUVIER), measure from 1.000th to 3.000th of an inch, and are full of a dark brown fluid.

The ultimate vesicular cæca of the liver of *Buccinum undatum*, contain ovoidal vesicles of various sizes. These vesicles contain more or less numerous nucleated cells. The cells are full of a dark brown fluid.

The hepatic cæca in the liver of *Patella vulgata*, contain vesicles about 1.000th of an inch in diameter. Each of these vesicles encloses a body which consists of a number of nucleated cells, full of a dark fluid resembling the bile.

The simple biliary apparatus, which surrounds the gastric portion of the intestinal tube of *Nereis*, contains nucleated cells, about 3.000th of an inch in diameter, full of a light brown fluid.

The hepatic cæca of *Carcinus Mœnas* contains cells full of a fluid of an ochrey colour, along with numerous oil globules. The cells measure about 2.000th of an inch.

The hepatic cæca of *Carabus catenulatus* (FABRICIUS) contain, attached to their internal surfaces, cells measuring about 1.000th of an inch in diameter. Between the nuclei, and the cell walls, a brown liquid containing numerous granules is situated.

The kidney of *Helix aspersa* (MÜLLER) is principally composed of numerous transparent vesicles. In the centre of each vesicle is situated a cell full of a dead white granular mass. This gland secretes pure uric acid.

The ultimate elements of the human liver are nucleated cells, measuring about 1.000th of an inch. Between the nucleus and the cell-wall is a light brown fluid, with one or two oil globules floating in it.

The vesicular cæca, in the testicle of *Squalus cornubicus*, contain nucleated cells which ultimately exhibit in their interior bundles of spermatozoa.

The generative cæca of *Echiurus vulgaris* (LAMARK) contain cells full of minute spermatozoa.

Aplysia punctata secretes from the edge and internal surface of its mantle a quantity of purple fluid. The secreting surface of the mantle consists of an arrangement of spherical nucleated cells, about 3.000th of an inch in diameter. These cells are distended with a dark purple matter.

The edge and internal surface of the mantle of *Janthina Fragilis* (LAMARK), the animal which supplied the Tyrian dye, secretes a deep bluish purple fluid. The secreting surface consists of a layer of nucleated cells, about 2.000th of an inch in diameter, distended, with a dark purple matter.

If an ultimate acinus of the mammary gland of the bitch be examined during lactation, it is seen to contain a mass of nucleated cells. These cells are generally ovoidal, rather transparent, and measure about 2.000th of an inch in the long diameter. Between the nucleus and the cell wall of each, a quantity of fluid is contained, and in this fluid float one, two, three, or more oil-like globules, exactly resembling those of the milk.

In addition to the series of examples already given, I might adduce many others to prove that secretion is a function of the nucleated cell. Some secretions, indeed, are so transparent and colourless, as to render ocular proof of their original formation within cells impossible ; and we do not yet possess chemical tests of sufficient delicacy for the detection of such minute quantities. The examples I have selected, however, shew that the most important and most striking secretions are formed in this manner. The proof of the universality of the fact, in reference to the glandular structures which produce colourless secretions, can only rest at present on the identity of the anatomical changes which occur in their cellular elements. This part of the proof I shall enter upon in another part of the paper.

The secretion within a primitive cell is always situated between the nucleus and the cell-wall. Now, as we know that the nucleus is the reproductive organ of the cell, that it is from it, as from a germinal spot, that new cells are formed, I am inclined to believe that it has nothing to do with the formation of the secretion. I believe that the cell-wall itself is the structure, by the organic action of which each cell becomes distended with its peculiar secretion, at the expense of the ordinary nutritive medium which surrounds it.

The ultimate secreting structure, then, is the primitive cell, endowed with a peculiar organic agency, according to the secretion it is destined to produce. I shall henceforward denominate it the primary secreting cell. It consists, like other primitive cells, of three parts,—the nucleus, the cell-wall, and the cavity. The nucleus is its generative organ, and may or may not, according to circumstances, become developed into young cells. The wall is the organ by the agency of which the cell performs the duty assigned to it. The cavity is the receptacle in which

the secretion is retained till the quantity has reached its proper limit, and till the period has arrived for its discharge.

Each primary secreting cell is endowed with its own peculiar property, according to the organ in which it is situated. In the liver it secretes bile—in the mamma, milk, &c.

The primary secreting cells of some glands have merely to separate from the nutritive medium a greater or less number of matters already existing in it. Other primary secreting cells are endowed with the more exalted property of elaborating from the nutritive medium matters which do not exist in it.

The discovery of the secreting agency of the primitive cell does not remove the principal mystery in which this function has always been involved. One cell secretes bile, another milk; yet the one cell does not differ more in structure from the other than the lining membrane of the duct of one gland from the lining membrane of the duct of another. The general fact, however, that the primitive cell is the ultimate secreting structure, is of great value in physiological science, inasmuch as it connects secretion with growth, as phenomena regulated by the same laws. The force, of whatever kind it may be, which enables one primary formative cell to produce nerve and another muscle, by an elaboration within itself of the common materials of nutrition, is identical with that force which enables one primary secreting cell to elaborate bile and another milk.

Instead of growth being a species of imbibing force, and secretion on the contrary a repulsive, the one centripetal, the other centrifugal, they are both centripetal. Even in their later stages the two processes, growth and secretion, do not differ. The primary formative cell, after becoming distended with its peculiar elaborated nutritive matter, in some instances changes its forms and arrangements according to certain laws, and then, after a longer or shorter period, dissolves and disappears in the inter-cellular space in which it is situated, its materials being taken up by the circulating system, if it be an internal, and being merely thrown off if it be an external cell. The primary secreting cell, again, after distention with its secretion, does not change its form so much as certain of the formative cells, but the subsequent stages are identical with those of the latter. It bursts or dissolves, and throws out its contents either into ducts or gland cavities (both of which, as I shall afterwards shew, are inter-cellular spaces), or from the free surface of the body.

The general fact of every secretion being formed within cells, explains a difficulty which has hitherto puzzled physiologists, viz., why a secretion should only be poured out on the free surface of a gland-duct or secreting membrane.

"Why," says Professor MÜLLER, "does not the mucus collect as readily between the coats of the intestine as exude from the inner surface? Why does not the bile permeate the walls of the biliary ducts, and escape on the surface of the liver, as readily as it forces its way outwards in the course of the ducts? Why

does the semen collect on the inner surface only of the tubuli semeniferi, and not on their exterior, in their interstices? The elimination of the secreted fluid on one side only of the secreting membrane, viz. on the interior of the canals, is one of the greatest enigmas in physiology." MÜLLER proceeds to explain this enigma by certain hypotheses; but the difficulty disappears, the mystery is removed, when we know that the secretion only exists in the interior of the ripe cells of the free surface of the ducts or membrane, and is poured out or eliminated simply by the bursting and solution of these superficial cells.

In the former part of this paper I have confined my observations to the structure and function of the ultimate secreting element, the primary secreting cell. I now proceed to state the laws which I have observed to regulate the original formation, the development, and the disappearance of the primary organ. This subject necessarily involves the description of the various minute arrangements of glands and other secreting structures. As the development of a subject so rich as this already has been, and still promises to be, would much exceed the limits of a single communication, I restrict myself at present to the announcement of the laws themselves, and to a statement of a few facts in illustration. I reserve for future communications, which I hope to have the honour of submitting to this Society, a detailed description of the minute changes which are constantly taking place in glands during the performance of their function.

If the testicle of *Squalus cornubicus* (GMELIN) be examined when the animal is in a state of sexual vigour, the following arrangements of structure present themselves.

The gland consists of a number of lobes separated, and at the same time connected by a web of filamentous tissue, in which ramify the principal bloodvessels.

The lobes, when freed from this tunic, present on their surface a number of vesicles. When the gland is dissected under water, and one of the lobes is raised out of its capsule, an extremely delicate duct is observed to pass from it into the substance of the capsule, to join the ducts of the other lobes.

When a section is made through one of the lobes, it becomes evident that the vesicles are situated principally on its exterior.

If a small portion be macerated in water for a few hours, and dissected with a couple of needles, there are observed attached to the delicate ducts which ramify through the lobe vesicles in all stages of development. These stages are the following:—1*st*, A single nucleated cell attached to the side of the duct, and protruding, as it were, its outer membrane.

2*d*, A cell containing a few young cells grouped in a mass within it; the parent cell presenting itself more prominently on the side of the duct.

3*d*, A cell attached by a pedicle to the duct, the pedicle being tubular, and communicating with the duct; the cell itself being pyriform, but closed and full of nucleated cells.

4th, Cells larger than the last, assuming more of a globular form, still closed, full of nucleated cells, and situated more towards the surface of the lobe.

5th, The full-sized vesicles already described as situated at the surface of the lobe. These vesicles are spherical, perfectly closed; that part of the wall of each which is attached to the hollow pedicle forms a diaphragm across the passage, so that the vesicle has no communication with the ducts of the gland. The contents of the vesicles are in various stages of development. Those least advanced are full of simple nucleated cells; in others, the included cells contain young cells in their interior, so that they appear granular under low powers; in others, the included cells have begun at a certain part of the vesicle to elongate into cylinders, with slightly rounded extremities. In others the cylindrical elongation has taken place in all the included cells, with the exception of a few, which still retain the rounded form, at a spot opposite to that part of the vesicle in which the change commenced, and at the same time it may be observed, that the cylindrical cells have become arranged in a spiral direction within the parent vesicle. *Lastly,* Vesicles exist in which all the cells are cylindrical, and are arranged within its cavity in a spiral direction.

The changes which occur in the included nucleated cells of the vesicle are highly interesting. After the nucleus of each has become developed into a mass of cells, the parent cell becomes, as has been stated, cylindrical. The change in the shape of the cell is contemporaneous with the appearance of a spiral arrangement of the included mass of cells. This spiral arrangement is also contemporaneous with an elongation of each cell in the mass, in the direction of the axis of the parent cell. When the elongation has reached its maximum, the original mass of included cells has assumed the appearance of a bunch of spirals, like cork-screws arranged one with another, spiral to spiral. In particular lights the cylindrical cell presents alternate spots of light and shade, but by management of the illumination, the included spiral filaments become evident; the light and shade is seen to arise from the alternate convexities and concavities of the spiral filaments, combined in a spiral bundle.

In vesicles more advanced, the walls of the cylindrical cells have become attenuated.

In other vesicles the diaphragms across their necks have dissolved or burst, the bundles of spiral filaments float along the ducts of the gland, or separate into individual spiral filaments. These filaments are completely developed spermatozoa, pointed and filamented at both extremities, thicker and spiral in the middle.

In the centre of the lobe where the smaller ducts meet to form the principal duct, there is a mass of grey gelatinous matter through which the ducts pass. This gelatinous matter consists of a number of cells lying between the converging ducts, and from their peculiar appearance not presenting the usual nuclei. I am inclined to believe that they are either vesicles which have never become developed

on account of the pressure of the surrounding parts, or that they are old vesicles in a state of atrophy after the expulsion of their contents.

Having now described the changes which are constantly taking place in the testicle of this shark when the organ is in a state of functional activity, I must defer till a future occasion an account of similar changes which occur in the parenchyma of an order of glands, of which the one already described may be considered as a type. I may state, however, that I have ascertained the following general facts in reference to glands of this order:—

1*st*, The glandular parenchyma is in a constant state of change, passing through stages of development, maturity, and atrophy.

2*d*, This state of change is contemporaneous with, and proportional to, the formation of the secretion, being rapid when the latter is profuse, and *vice versa*.

3*d*, There are not, as has hitherto been supposed, two vital processes going on at the same time in the gland, growth and secretion, but only one, viz. growth. The only difference between this kind of growth and that which occurs in other organs being, that a portion of the product is from the anatomical condition of the part thrown out of the system.

4*th*, The vital formative process which goes on in a gland, is regulated by the anatomical laws of other primitive cellular parts.

5*th*, An acinus is at first a single nucleated cell. From the nucleus of this cell others are produced. From these, again, others arise in the same manner. The parent cell, however, does not dissolve away, but remains as a covering to the whole mass, and is appended to the extremity of the duct. Its cavity, therefore, as a consequence of its mode of development, has no communication with the duct.

The original parent cell now begins to dissolve away, or to burst into the duct at a period when its contents have attained their full maturity. This period varies in different glands, according to a law or laws impressed upon each of them.

6*th*, In the gland there are a number of points from which acini are developed, as from so many centres. These I denominate the germinal spots of the gland.

7*th*, The secretion of a gland is not the product of the parent cell of the acinus, but of its included mass of cells. The parent cell or vesicle may be denominated the primary cell; its included nucleated cells, after they have become primary secreting cells, may be denominated secondary cells of the acinus.

8*th*, The matter which passes off by a duct of a gland may be, 1*st*, A true secretion, that is, matter formed in the primary secreting cell cavities; or, 2*d*, A mixture of a fluid formed in these cell cavities with the developed or undeveloped nuclei of the cells themselves; and, 3*d*, It may be a number of secondary cells passing out entire.

If a portion of the liver of *Carcinus Mænas* is carefully examined, it is observed that each of the follicles of which it consists presents the following struc-

ture :—The blind extremity of the follicle is slightly pointed, and contains in its interior a mass of perfectly transparent nucleated cells. From the blind extremity downwards, these cells appear in progressive states of development. At first they are mere primitive nucleated cells; further on they contain young cells; and beyond this they assume the characters of primary secreting cells, being distended with yellow bile, in which float oil globules, the oil in some instances occupying the whole cell. Near the attached extremity of the follicle an irregular passage exists in the midst of the cells, and allows the contents of the cells which bound it to pass on to the branches of the hepatic duct.

This arrangement of the secreting apparatus may be taken as the type of an order of glands, which consist of follicles more or less elongated. Growth in glands of this kind is regulated by the following laws :—

1st, Each follicle is virtually permanent, but actually in a constant state of development and growth.

2d, This growth is contemporaneous with the function of the gland, that function being merely a part of the growth, and a consequence of the circumstances under which it occurs.

3d, Each follicle possesses a germinal spot situated at its blind extremity.

4th, The vital action of some follicles is continuous, the germinal spot in each never ceasing to develope nucleated cells, which take on the action of, and become primary secreting cells, as they advance along the follicle. The action of other follicles is periodical.

5th, I have not been able to satisfy myself, but I am inclined to believe, that the wall of the follicle is also in a state of progressive growth, acquiring additions to its length at its blind extremity, and becoming absorbed at its attached extremity.* A progressive growth of this kind would account for the steady advance of its attached contents, and would also place the wall of the follicle in the same category with the primary vesicle or wall of the acinus in the vesicular glands.

6th, The primary secreting cells of the follicle are not always isolated. They are sometimes arranged in groups, and when they are so, each group is enclosed within its parent cell, the group of cells advancing in developement according to its position in the follicle, but never exceeding a particular size in each follicle.

I am inclined to believe, although of this I have not satisfied myself, that there is an order of glands, namely, those with very much elongated and anastomosing ducts, which do not possess germinal spots in particular situations, but in which these spots are diffused more uniformly over the whole internal surface of the ducts. I am the more inclined to believe this, from what I have observed in

* Mr Henry Goodsir, in a paper on the Development and Metamorphoses of *Caligus*, read in the Wernerian Society, April 1842, has stated that the wall of the elongated and convoluted follicle, which constitutes the ovary in that genus, grows from its blind to its free extremity, at the same rate as the eggs advance in development and position.

certain secreting membranes. Thus the membranes which secrete the purple of *Aplysia* and *Janthina* are not covered with a continuous layer of purple secreting cells, but over the whole surface, and at regular distances, there are spots, consisting of transparent, colourless nucleated cells, around which the neighbouring cells become coloured. Are these transparent cells the germinal spots of these secreting membranes? And may not the walls of the elongated tubes, and the surfaces of the laminæ within certain glands, have a similar arrangement of germinal spots?

We require renewed observations on the original development of glands in the embryo. From the information we possess, however, it appears that the process is identical in its nature with the growth of a gland during its state of functional activity.

The so-called blastema, which announces the approaching formation of a gland in the embryo, in some instances precedes, and is in other instances contemporaneous with, the conical blind protrusion of the membrane upon the surface of which the future gland is to pour its secretion.

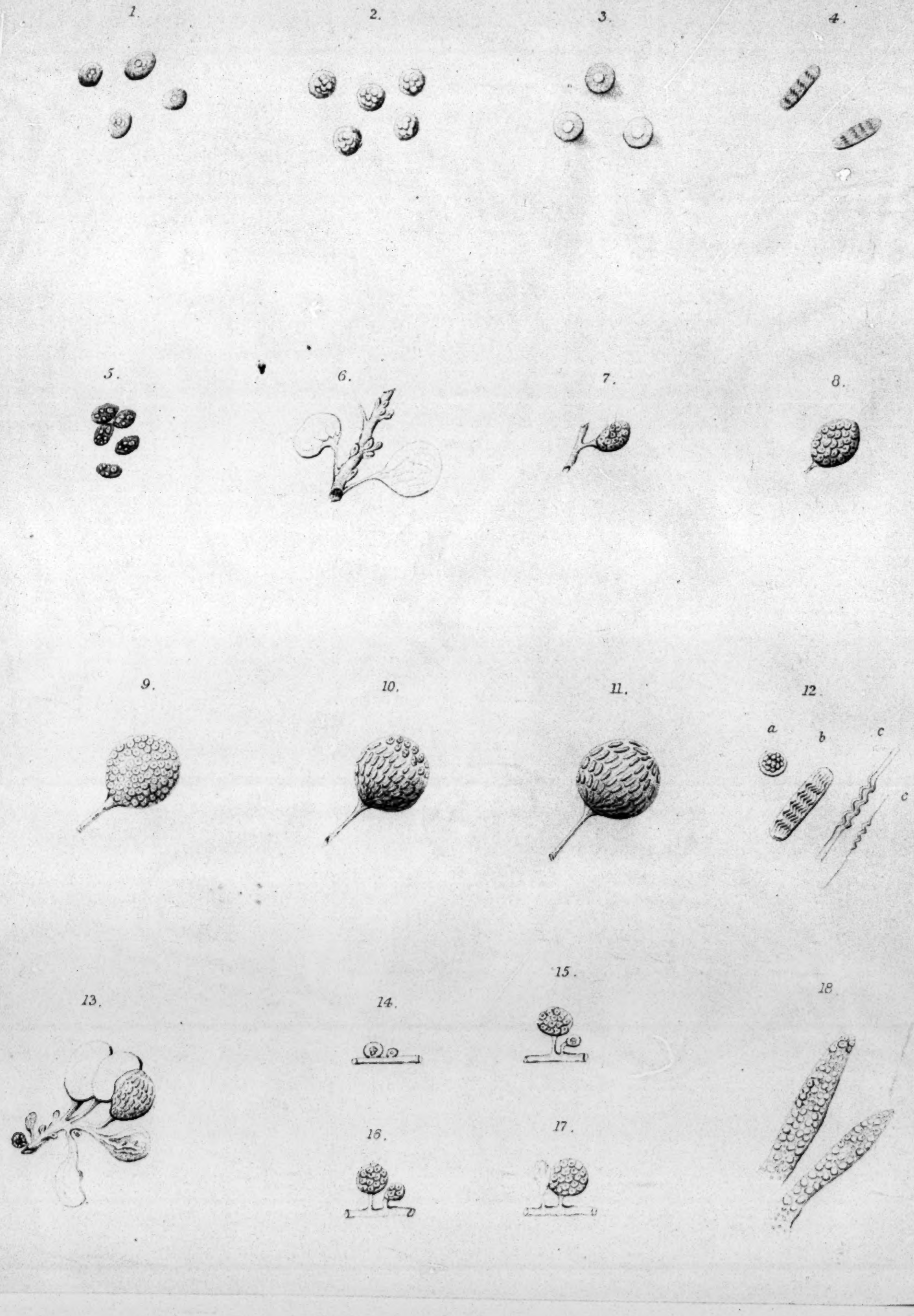
In certain instances it has been observed that the smaller branches of the duct are not formed by continued protrusion of the original blind sac, but are hollowed out independently in the substance of the blastema, and subsequently communicate with the ducts.

It appears to be highly probable, therefore, that a gland is originally a mass of nucleated cells, the progeny of one or more parent cells, mediate or immediate products of the yelk; that the membrane in connexion with the embryo gland may or may not, according to the case, send a portion of the membrane, in the form of a hollow cone, into the mass; but whether this happens or not, the extremities of the ducts are formed as closed vesicles, and then nucleated cells are formed within them, and are the parents of the epithelium cells of the perfect organ.

Dr Allen Thomson has ascertained that the follicles of the stomach and large intestine are originally closed vesicles. This would appear to shew that a nucleated cell is the original form of a follicle, and the source of the germinal spot which plays so important a part in its future actions.

The ducts of glands are therefore inter-cellular passages. This is an important consideration, inasmuch as it ranges them in the same category with the intercellular passages and secreting receptacles of vegetables.

I conclude, therefore, from the observations which I have made—1*st*, That all the true secretions are formed or selected by a vital action of the nucleated cell, and that they are first contained in the cavity of that cell; 2*d*, That growth and secretion are identical—the same vital process, under different circumstances.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EXPLANATION OF THE PLATE (VIII).

Fig. 1. Four secreting cells from the ink bag of *Loligo sagittata*.

Fig. 2. Five cells from the liver of *Patella vulgata*. In this instance the bile is contained in the cavities of the secondary cells, which constitute the nucleus of the primary cell.

Fig. 3. Three cells from the kidney of *Helix aspersa*. The contained secretion is dead white, and presents a chalky appearance.

Fig 4. Two cells from the vesicles of the testicle of *Squalus cornubicus*. The contained bundles of spermatozoa are developed from the nucleus,—each spermatozoon being a spiral cell.

Fig. 5. Five cells from the mamma of the bitch. In addition to their nuclei, these cells contain milk globules.

Fig. 6. A portion of duct from the testicle of *Squalus cornubicus*. A few nucleated cells, the primary or germinal cells of the future acini are attached to its walls.

Fig. 7. The primary cell of an acinus in a more advanced stage. The nucleus has produced a mass of of young cells. The pedicle appears to have been formed by the germinal cell carrying forward the wall of the duct. A diaphragm accordingly presents itself, across the neck of the pedicle.

Fig. 8. A primary cell in a more advanced stage.

Fig. 9. A primary cell still more advanced.

Fig. 10. Some of the secondary cells, products of the nucleus of the primary cell, are cylindrical, and are arranged in a spiral.

Fig. 11. The change into cylinders, and the spiral arrangement completed.

Fig. 12. *a*, One of the secondary cells; its nucleus a mass of young cells. *b*, A secondary cell elongated into a cylinder, each cell of its composite nucleus elongated into a spiral. *c*, The spiral cells, or spermatozoa, free.

Fig. 13. A bunch of acini, in various states of development, maturity, and atrophy.

The four following Figures are diagrams, arranged so as to illustrate the intimate nature of the changes which occur in vesicular glands when in a state of functional activity.

Fig. 14. A portion of gland duct with two acini. One of the acini is a simple primary cell: the other is in a state of development, its nucleus producing young cells.

Fig. 15. Both acini are advancing: the second has almost reached maturity.

Fig. 16. The second acinus is ready to pour out its contents, the first to take its place.

Fig. 17. The second acinus is in a state of atrophy, the first is ripe.

Fig. 18. Two follicles from the liver of *Carcinus mænas*. The colourless germinal spot is at the blind extremity of the follicle. The secreting cells become distended with bile and oil, as they recede from the germinal spot.

XIX.—*On the Quarantine Classification of Substances, with a view to the Prevention of Plague.* By JOHN DAVY, M.D., F.R.SS., L. & E., Inspector-General of Army Hospitals, L. R.

(Read 4th April 1842.)

To all those who have paid any attention to the subject of quarantine, it is well known that certain articles are held to be susceptible of conveying the contagion of plague ; that certain others, in regard to this property, are considered doubtful ; and that others are held to be unsusceptible of retaining and communicating it.

Are these distinctions accurate ? Are they founded on well-established facts ? On these questions I shall have the honour of submitting some remarks to the Society, with the hope of drawing attention to a subject of much importance, and hitherto, in a scientific point of view, strangely neglected.

Let us first consider the kind of articles which are held to be incapable of conveying the virus, or contagious matter of plague. They are chiefly the following :—

1. Different kinds of grain, flour, bread, starch ; fresh and dried fruits and conserves ; honey, oil, lard, cheese ; salted, dried, and smoked meats.

2. All kinds of wood and charcoal ; cordage, provided it is completely tarred ; rush and broom, and cordage made from them.

3. All the metals, with two or three exceptions, depending on the metal being newly wrought ; all minerals, fossils, earths, and salts.

4. Money and medals ; glass and pottery ; substances used in painting and dyeing ; and all articles solely composed of substances which have a place in the non-susceptible class.

It might be supposed, that good reasons could be assigned by those who have been concerned in framing this class of bodies,—including substances so numerous, so miscellaneous, and differing so widely in their nature and properties,—that it would be founded on extensive and careful experience, and the result of accurate and rigid induction.

The answers received to the inquiries I have made to endeavour to learn in what way, on what principles, this classification has been framed, have been altogether unsatisfactory, equally so in Malta and in Constantinople ; the former one of the oldest quarantine stations in Europe, the other where the system of quaran-

tine is now only in process of being introduced From all the information I have been able to collect, it would appear that no accurate method has been employed in determining the non-susceptibility of even *any one* article in regard to contagion, much less so many and so different. Persons officially employed, who have defended the classification, have referred to accumulated experience, without being able to mention any trials, any experiments, any precise observations, on which the inquiring mind can rest with satisfaction. The fact, I believe, is, that the classification, which is almost coeval with the establishment of lazarettos, was made during a time of panic, from vague considerations of the qualities of substances, in ignorant times, and by men unqualified, by the then imperfect state of knowledge, to arrive at accurate conclusions on a subject, as a matter of inquiry, of more than ordinary difficulty; and that their *dicta* gaining force with age, which has been called experience, have become laws, and venerable, and almost sacred as such.

In the absence of all precise experiments, it may be worth while to consider whether, taking into account the qualities of the substances pronounced to be non-susceptible, they are likely to be so in reality. Three of the more important ones may be selected on which to fix the attention, viz. metals, wood, and glass.

From what is known of the properties of either of these, it must be admitted, I apprehend, that they are totally destitute of all power, either of repelling the matter of contagion, or of destroying it, or, if it be adhesive, of preventing its adhesion. Glass and wood are incapable of effecting any alteration in the composition of animal substances; and the same remark applies to most of the metals. Glass and wood, it is well known, are the substances chiefly employed for the purpose of preserving and transmitting vaccine lymph. On wood or glass perfectly dried, or in glass tubes, in its liquid state, hermetically sealed, vaccine lymph has been preserved many months, and has been transmitted to distant countries. Reasoning from analogy, it seems highly probable, that using the same substances, and the same methods, other contagious matters, not excepting the contagious matter of the plague, might, with equal certainty, be preserved and transmitted.

It would be a superfluous labour and waste of time to offer remarks on all the different articles generally classed under the head of non-susceptible. Excepting a very few of a doubtful nature, as the alkalies, quicklime, and certain salts, they appear all to come under the same category in relation to the matter of contagion, as glass, or wood, or metal, being neither repellant of it, destructive, nor preventing its adhesion, that is to say, judging from analogy, in the absence of positive experience.

Let us now turn our attention to the class of substances called susceptible. They are principally the following:—

1. Wool, hair, and skins of different animals; feathers; animals, whether dead

or alive; silk, sponges, wrought coral; in brief, all animal substances, whether in a raw or manufactured state, not specified as belonging to either of the other classes.

2. Cotton and flax, and the vast number of fabrics made of these substances.

3. All the various articles of equipment; those commonly designated goods; furniture, saddlery; every thing belonging to bedding.

4. Candles, both wax and tallow.

5. Hot bread.

This class of objects, even more miscellaneous than the preceding, it can hardly be doubted, was formed at the same time, and in the same rude and inexact manner, and that it is as little deserving of confidence. What a vast number of carefully conducted trials would be requisite to determine the quality, in relation to susceptibility to convey contagion, of so many articles! And yet, I believe, it may confidently be stated, that not a single article of the many has yet been subjected to experiment, expressly for the purpose in question; and, even further, that those persons who are employed in carrying into effect the regulations of quarantine have not taken advantage of the experience unavoidably collected in lazarettos, especially in the instances of certain articles extensively exported from Egypt and the Levant, such as cotton and silk. Of each of these articles thousands of bales are annually passed through the lazarettos of Malta, Marseilles, and Trieste; and, during the last twenty years, often at times when plague prevailed in the countries from which they have been shipped. They are handled in the lazarettos by men whose duty it is to expose them to the air. No instance is on record of any individual so employed contracting plague. This is the result of careful inquiry made in the different lazarettos of the Mediterranean. Notwithstanding, cotton and silk are still retained in the list of susceptible articles, and are even commonly denounced as highly susceptible. Were reason followed, surely the contrary conclusion should be drawn; and, I apprehend, it would not be confined to these two substances, but would require to be extended to all the so-called susceptible articles, not one of them, that I am aware, having yet been proved to have been the medium of the communication of plague within the walls of a lazaretto.

Further, applying the same process of reasoning to the substances collected in this class that was used to the former, I apprehend the conclusion most warranted by analogy must be admitted to be, that the so-called susceptible articles are least entitled to be so considered, are, in truth, least fitted for preserving the matter of contagion in an unaltered active state.

All very porous, spongy, fleecy objects, all that are readily and very compressible, abound in atmospheric air, to the presence of which between their fibres and particles they owe the qualities indicated in the terms expressive of them. Now it is well known that the presence of atmospheric air, especially when con-

fined, is a great promoter of decay and decomposition, and that no animal substance in a moist state can be preserved free from putridity if subject to putrefaction, or defended from change and decomposition, unless atmospheric air be excluded. What should we say of an attempt to convey vaccine lymph to India in a fleece of wool, or in cotton, no precautions having been taken to expel and exclude hygrometrical moisture? It would appear preposterous. Is it not equally so to infer, *a priori*, that the matter of plague in its unchanged, its active state, can be brought to our shores from those of the Mediterranean in the same vehicles? And with different degrees of force, according to the quantity of atmospheric air which they may contain, the remark is applicable to almost all the articles brought together under the head of susceptible.

The distinction in the general classification between hot bread and cold bread, the one being placed in the list of susceptible articles, the other in that of the non-susceptible, seems very characteristic of the ignorant and unscientific manner in which the arrangement has been made, and as if other considerations, besides the mere qualities of bodies, may have weighed on the minds of those who framed the classification. It would have proved very inconvenient to have denounced bread as a susceptible article, as much so as grain; but not so, hot bread; it might have been considered a salutary measure, in relation to the general health, to prohibit its use. Viewed strictly, however, the rational conclusion is the reverse of that acted on, namely, that cold bread possibly may convey the matter of contagion from hand to hand; whilst the hot bread, in consequence of its heat, would, if it received it, render it inert. The experiments made by the late Dr HENRY went far to prove, that all the contagious matters which he submitted to trial were deprived of their peculiar power by a temperature several degrees below the boiling point of water.

After the remarks made on the classes of susceptible and non-susceptible articles, it will be almost sufficient to enumerate the articles which have been collected in the third class, or that of objects of a doubtful nature in relation to the conveying of contagion. They are, chiefly, coral in its rough state, elephants' teeth, horns and horn-shavings, drugs and spices of all kinds, coffee and sugar, rolled tobacco, roots and herbs for dyeing, vermilion, newly-wrought copper and its clippings, glutinous fruits.

The remarks made on the two preceding classes will apply generally to this. The larger number of articles included in it seems better fitted, reasoning analogically, to preserve the matter of contagion than to promote its decomposition; some, by their compactness of texture, excluding air, as ivory, horn, and coral; others, by their antiseptic qualities, as sugar, and the majority of drugs and spices, arresting change.

The classification I have commented on is that adopted in the quarantine system lately attempted to be introduced into Turkey under the superintendence

of a superior Council of Health, formed principally of delegates from the embassies of the different states of Europe residing in Constantinople, to the entire exclusion of Mahommedans, with the exception of one, the president of the board, a Turk of high official rank, appointed for form-sake, according to the usage of the Turkish Government. Generally, it accords with the classification in use in the adjoining countries, and being the last formed, it might be expected that it would approach nearest to perfection, and be least liable to objection.

In discussing the subject, I have taken for granted that plague is a contagious disease, and that it is propagated by a peculiar matter of contagion. If this be not admitted, of course the classification must be held to be superfluous, together with the whole system of quarantine, and all considerations as to its details must be useless. Admitting the contagion of plague, and that it is a fixed matter, according to the commonly received views of the contagionists, the same process of reasoning respecting it seems to be applicable, whether it be supposed to be of the nature of living germs, or of an animal poison, or a substance *sui generis*, altogether peculiar.

To engage, however, in the inquiry for practical purposes, with a view to the efficiency of the quarantine system, I need hardly remark, that a different procedure should be observed from that which I have followed, and that certain questions *in limine* ought, if possible, to be settled; as, whether the disease is essentially contagious or not; whether it is ever of local origin, and only occasionally contagious; or, whether it is ever any thing more than an epidemic disease. These are questions which have been long discussed, and, in the opinion of not a few, are still undecided. In Constantinople, the majority of the Frank physicians consider the plague strictly contagious; whilst, in Egypt, the greater number of them are convinced that it is not of a contagious character, or, at furthest, only rarely so, under peculiar circumstances.

In legislating with regard to quarantine, a great mistake appears to have been committed, namely, that of enacting regulations on the ground of suppositions, instead of on the firm basis of facts obtained by means of rigid inquiry and accurate research. The consequence has been what might be expected, a sanatory system of a very unsatisfactory kind, troublesome, expensive, and insecure, for so I believe it is considered by all unbiassed persons who have given it their careful attention. Were, indeed, the system in other respects perfect, the nature of the classification of substances that has passed in review must, of necessity, vitiate it, and render it, there is too much reason to believe, worse than useless.

The time, it may be expected, at least it is to be hoped, is not far distant, when a thorough investigation of the subject will be required, preliminary to a revision of the laws of quarantine, should the results of the inquiry be, that the plague is truly contagious, or is capable of becoming so. No investigation is more urgently required, whether we consider the interests of commerce, the security of

the public health, or, I believe I may add without exaggeration, the welfare of mankind at large, especially in the Mediterranean countries, and those communicating with them, now constituting the larger portion by far of the globe.

To those who have never been in quarantine, and have not witnessed the manner in which the distinctions involved in the classification, the subject of this paper, are made to operate, and how they tend to vitiate other parts of the sanatory system, a few examples, it has occurred to me, may perhaps be acceptable by way of illustration. The inconsistencies between the actual practices and those which seem to be warranted by rational views are so great, that the unexperienced in them, without giving attention to particular instances, can form but a faint idea of them, when viewed as it were abstractedly.

The few notices of such instances that I shall bring forward I shall give very briefly, selecting such as I can vouch for being correct, having come under my own observation.

1. A vessel arrives at Malta from a port in quarantine with that island, and is brought to an anchor in the quarantine harbour for the purpose of all communication with the shore being prevented for a limited time. Boats, however, immediately and during the time of probation, are allowed to come alongside, and bring fruit and certain provisions from the town. If brought in baskets, as they commonly are, the baskets as soon as emptied are returned, handed by those on board in quarantine to those in the boat in pratique, without compromising them, on account of the so-called non-susceptible material, wood, of which the baskets are made.

2. A trabaccalo (a large boat in use in the Adriatic) arrives at Corfu from Ancona, a town in pratique with the Ionian Islands, after having been under the necessity, from stress of weather, of putting into various ports of the mainland, some of them in Turkey in quarantine, in each of which the passenger and small crew landed, and got such articles of provisions as belong to the non-susceptible class, such as bread, wine in barrels, vegetables, &c., the passenger having taken care that contact with any native or susceptible article was avoided. He makes a declaration to this effect at the Corfu Sanità Office, and is immediately granted pratique, and permission to land.

3. In Constantinople, at the Sanità Office, the masters of all vessels, after arrival, are required to present themselves. They appear before a small window, with a projecting ledge of wood. In the same hour, twenty masters from different ports, some in quarantine, some not, may come in succession, and one after another rest their arms or place their hands on the ledge, those who are in pratique not thereby subjecting themselves to quarantine, because the ledge belongs to the class of non-susceptible substances.

4. In the Austrian lazaretto, near Orsova on the Danube, it is the common

practice of the officer who goes round to make an inventory of the effects of travellers in quarantine, to seat himself when doing so at the wooden table, and write where the traveller in quarantine, the instant before, may have been writing, and the probability is, that he will use the chair from which the traveller may have risen just before, without subjecting himself to quarantine, the table and chair being entirely of wood.

5. In passing the Porto di Ferro, a rapid of the Danube so called, a little below Orsova, where it is necessary for the passengers to land and walk a certain distance, they are accompanied by an armed guardiano, to prevent all communication on their part. He makes no objection to their touching stones or plants, and will willingly receive a glass of wine or a piece of bread from the boat; but is most cautious that no one of the party touch the rope by which the boat is tracked, that being a so-called susceptible article, and capable, it is supposed, of receiving and imparting contagion.

6. A gentleman, a native of the country, visits a friend in quarantine in the lazaretto just mentioned. They meet in the open air, in a little yard belonging to the traveller's apartment, in which, on a line, some of his clothes are suspended for depuration, according to the rules of the lazaretto. The tassel of the cap of the visitor comes in contact with one of them; with the end of his stick he casts his cap off his head; but this does not save him from the penalty he dreaded. The guardiano, who witnessed the contact, places him instantly in quarantine, and for the same period as his friend, with whom he is asserted to have communicated by the contact of susceptible articles. He might, unquestioned, and with impunity, have taken his friend's stick with him into the adjoining town, or have drank out of the glass used by his friend, or have touched twenty other things in common, provided they did not belong to the list of susceptible articles.

7. When cases of plague were in the lazaretto of Constantinople last summer. after visiting them, one of the official authorities present, on taking leave, requested of the medical man in attendance on the plague patients a little leaf-tobacco to make a cigar. The medical man, in rigid quarantine, placed the tobacco on a stone and withdrew, and the official man in pratique instantly took it up for use; and, to a question as to the propriety of so doing, replied it was quite in rule and safe, leaf-tobacco not being a susceptible article.

8. Money at this time, when plague was actually in the lazaretto, imported from Alexandria, was not allowed to be received from those in quarantine till it had been passed through water. A perforated iron ladle received the money, in which it was dipt in a bucket of water, and instantly delivered to the person requiring payment in pratique. The immersion might have been for two or three seconds.

After witnessing this, I applied animal matters, minute quantities hardly perceptible, to polished steel and glass, immersed them in water for a quarter of

a minute, and then examined them with a microscope. Traces of the animal matter in each instance were distinctly perceptible; in two, indeed, it was not perceptibly diminished by the immersion. The two were the cerumen of the ear, and that soiling matter from the hand, or any other part of the surface of the body, which, as is well known, dulls by contact any polished surface. Those of which traces only remained were saliva dried, the finest pellicle, and a stain of blood. The difference noticed between these animal matters, as regards their appearance after immersion, no doubt depends on the nature of the particular substances. Saliva and blood both contain matter soluble in water, which will be removed by its action; whilst the epithelium fragments in the one, and the fibrin in the other, will remain, being insoluble. In the other two instances of the cerumen of the ear and the soiling matter, the excretion from the skin, the material of each being in most part insoluble in water, it could not be expected that immersion for so short a time should have any visible effect. I have found, indeed, that glass dulled by having been applied to the upper eyelid, or by being held between the thumb and fore-finger, or pressed against other parts of the body, may be immersed in water several hours, even days, without losing its peculiar appearance when placed under the microscope, an appearance it seems to owe to sebaceous or oily matter mixed with epithelium scales in constant process of excretion and separation.

These few instances, which might be easily multiplied, may suffice to illustrate the danger lurking under the classification commented on, and how, if it be based erroneously, as I have endeavoured to show, it undermines altogether, and renders, as before said, worse than useless the other parts of the system of quarantine as at present followed.

Murrayfield House, near Edinburgh,
March 31. 1842.

XX.—*On the Theoretical Investigation of the Absolute Intensity of Interfering Light.* *By* The Rev. P. Kelland, *A.M., F.R.SS., Lond. and Edin., F.C.P.S., Professor of Mathematics in the University of Edinburgh, late Fellow and Tutor of Queen's College, Cambridge.*

(Read 4th April 1842.)

The subject of the following Memoir has been already touched on in a communication which I made to the Cambridge Philosophical Society, and which appears in the seventh volume of their Transactions, Art. IX. For the better understanding of what follows, it is desirable to state briefly the contents of that paper. Its object is to estimate the quantity of light which is received on a screen of unlimited dimensions, after passing through a certain aperture, or suffering reflection at two mirrors, as in the case of the interference experiment, so as to exhibit the appearance of bands. The question to be answered is this:—Does interference restore light or increase it, as it seems to do in the case of a grating placed before a lens, whereby, instead of one, a number of luminous images are produced? Or does it destroy light, as it seems to do in the case of interference by the reflection of two portions of a ray at different surfaces inclined to one another, where, instead of an uniformly bright field, at least near its centre, the eye is presented with a series of dark bands? The subject is treated by taking the ordinary expression for the intensity of light at any point of a screen placed in the focus of a lens on which the light falls, and, by integration, determining the whole intensity, or the total quantity of light which falls on the screen. The conclusions deduced from this process are,—1. That when an aperture is placed before the lens, the total intensity of the light which passes to the screen varies as the magnitude of the aperture. 2. That when a grating is placed before it, the total intensity of the light received on the screen is to the quantity which falls on the grating, as the space left open by the wires to the whole space on which the light falls. 3. That when light falls on two mirrors, inclined by a small angle to each other, the whole quantity of light received on the screen is equal to the quantities which would be received from each of the mirrors separately. These results all concur in the establishing of the following answer to our question. *The effect of interference*, whether produced by diffraction or admixture, *is to displace, but neither to destroy nor to produce light.* The total quantity of light which

is received on the screen is exactly the same as if there were no interference at all, but it is differently distributed.

These conclusions are deduced from the expressions for the intensity of the light at *any* point of the screen given by the Astronomer Royal in his Tracts. They depend, therefore, on HUYGHENS's principle, and are proper for the examination of the truth of that principle, or of its more exact statement. It is to this matter, which was only casually touched on in the Memoir referred to, that I wish to direct attention at present.

The principle, as stated by Mr AIRY, is this: "The effect of any wave in disturbing any given point, may be found by taking the front of the wave at any given time, dividing it into an indefinite number of small parts, considering the agitation of each of these small parts as the cause of a small wave, which will disturb the given point, and finding, by summation or integration, the aggregate of all the disturbances of the given point, produced by the small waves coming from all points of the great wave." Here it is evident that there is introduced as the coefficient of agitation, *the area* of the front of the wave, or at least of a portion of it, so that we must divide again by some area, or the product of some two lines, in order to bring the agitation at one point to depend simply on the agitation at another. It will be the object of the following pages to ascertain what that divisor must be. This will be effected by conceiving that it is some *constant* quantity, which we are led to do from the results of our former investigations, and then from the expression for the total quantity of light received on the screen to deduce its value. We proceed, then, to solve the following problems.

PROB. I.—*A series of plane waves passes through a parallelogram, and is transmitted to a screen by means of a lens, the focus of which is on its surface; to find the total quantity of light which is received on the screen.*

Let $2e$, $2f$ be the length and breadth of the parallelogram, p, q the co-ordinates of a point in the screen, measured from the focus parallel respectively to e and f, b the distance of the lens from the screen. Then, if a be the coefficient of vibration, or a^2 the intensity or quantity of light on a unit of surface of the incident wave; D the divisor in question; the intensity at the point whose co-ordinates are p and q is

$$16\frac{a^2}{D^2}e^2f^2\left(\frac{b\lambda}{2\pi qf}\sin\frac{2\pi qf}{b\lambda}\right)^2\left(\frac{b\lambda}{2\pi pe}\sin\frac{2\pi pe}{b\lambda}\right)^2.$$ AIRY's *Tracts*, p. 324.

Hence the total intensity, or the quantity of light on the screen is

$$64\frac{a^2e^2f^2}{D^2}\int_0^\infty dq\left(\frac{b\lambda\sin\frac{2\pi qf}{b\lambda}}{2\pi qf}\right)^2\int_0^\infty\left(\frac{b\lambda\sin\frac{2\pi pe}{b\lambda}}{2\pi pe}\right)^2dp.$$

Let $\frac{2\pi pe}{b\lambda}=x, \frac{2\pi qf}{b\lambda}=y, \therefore\ dp=\frac{b\lambda}{2\pi e}dx$, &c. And the total quantity of

$$\text{light} = \frac{64\, a^2 e^2 f^2 b^2 \lambda^2}{4\pi^2 e f D^2}\int_0^\infty \left(\frac{\sin y}{y}\right)^2 dy \int_0^\infty \left(\frac{\sin x}{x}\right)^2 dx.$$

$$\text{Now, } \int_0^\infty \frac{a\cos mx\, dx}{a^2+x^2} = \frac{\pi}{2} e^{-ma},$$

$$\therefore \int_0^\infty \frac{\sin^2 x}{x^2}dx = \int_0^\infty \frac{1}{2}\,\frac{1-\cos 2x}{x^2}dx,$$

$$=\frac{\pi}{4}\left(\frac{e^{o}-e^{-2a}}{a}\right); \ a \text{ being equal to } 0.$$

$$=\frac{\pi}{4}\ \frac{2a+\&c.}{a}\ (a=0)$$

$$=\frac{\pi}{2}$$

$$\therefore \quad \text{total quantity of light} = \frac{4\, e f b^2 \lambda^2 a^2}{D^2}$$

$$= \text{area of parallelogram} \times \frac{b^2\lambda^2 a^2}{D^2}.$$

Now, it *ought* to be, area of parallelogram $\times\ a^2$;

$$\therefore \qquad D = b\lambda.$$

PROB. II.—*A series of equal parallelograms are placed before a lens, to find the whole quantity of light received on a screen placed perpendicular to the axis of the lens at its focus.*

Let e, $2f$ be the breadth and length of one of the openings, g the breadth of one of the opaque sides of one of the parallelograms; p, q the co-ordinates of a point on the screen, measured from the focus of the lens, q being parallel to the sides of the parallelograms, b the focal length of the lens, or the perpendicular distance between the screen and the lens; m the number of openings.

The intensity of the light at the point p, q is given by AIRY (*Tracts*, p. 328) as

$$\frac{4\,a^2e^2f^2}{D^2}\left(\frac{\lambda b}{2\pi qf}\sin\frac{2\pi qf}{\lambda b}\right)^2\left(\frac{\lambda b}{\pi pe}\sin\frac{\pi pe}{\lambda b}\right)^2\left(\frac{\sin\frac{p(e+g)\pi}{\lambda b}m}{\sin\frac{p(e+g)\pi}{\lambda b}}\right)^2,$$

$\frac{a}{D}$ being introduced as the coefficient of vibration, and the divisor which we seek to determine respectively.

The whole quantity of light received on the screen is the integral of this expression with respect to p and q, each between the limits of $+\ \infty$ and $-\ \infty$. Call

it u, and let

$$\frac{\pi p e}{\lambda b} = x, \quad \frac{2\pi q f}{\lambda b} = y, \quad 1 + \frac{g}{e} = r.$$

$$\therefore \quad u = \frac{8 a^2 e f \lambda^2 b^2}{\pi^2 D^2} \int_o^\infty dx \int_o^\infty dy \left(\frac{\sin y}{y}\right)^2 \cdot \left(\frac{\sin x}{x}\right)^2 \cdot \left(\frac{\sin r m x}{\sin r x}\right)^2$$

Now, $$\int_o^\infty dy \left(\frac{\sin y}{y}\right)^2 = \frac{\pi}{2} \quad \text{(last Prob.)}$$

$$\therefore \quad u = \frac{4 a^2 e f \lambda^2 b^2}{\pi D^2} \int_o^\infty dx \left(\frac{\sin x}{x}\right)^2 \cdot \left(\frac{\sin r m x}{\sin r x}\right)^2.$$

Now, it was proved in my Memoir in the Transactions of the Cambridge Philosophical Society, vol. vii. p. 163, that the value of the integral

$$\int_o^\infty dx \left(\frac{\sin x}{x}\right)^2 \left(\frac{\sin r m x}{\sin r x}\right)^2 \text{ is } \frac{\pi}{2}.$$

$$\therefore \quad u = \frac{2 a^2 e f \lambda^2 b^2}{D^2} = \frac{\lambda^2 b^2}{D^2} \cdot a^2 \times \text{area of the aperture left uncovered.}$$

Now, it ought to be $a^2 \times$ same aperture.

$$\therefore \quad D = b\lambda.$$

PROB. III.—*Let every thing remain as in the last Problem, except that the aperture is an isosceles triangle.*

The vibration at the point whose co-ordinates are p, q, is

$$\frac{a}{D}\iint dx\, dy \sin \frac{2\pi}{\lambda}\left(v t - B + \frac{p x}{b} + \frac{q y}{b}\right);$$

the limits being $y = -x \tan \alpha$, $y = x \tan \alpha$; $x = 0$, $x = c \cos \alpha$: where α is the half of the angle included by the equal sides of the triangle. Integrating with respect to y, we get

$$\frac{\lambda b}{2\pi q}\frac{a}{D}\int dx \left\{\cos \frac{2\pi}{\lambda}\left(v t - B + \frac{p x}{b} - \frac{q x \tan \alpha}{b}\right) - \cos \frac{2\pi}{\lambda}\left(v t - B + \frac{p x}{b} + \frac{q x \tan \alpha}{b}\right)\right\}.$$

If M, N denote the coefficients respectively of

$$\cos \frac{2\pi}{\lambda}(v t - B) \text{ and } \sin \frac{2\pi}{\lambda}(v t - B) \text{ in this integral,}$$

$$M = \frac{\lambda b}{2\pi q} \cdot \frac{\lambda b a}{2\pi (p - q \tan \alpha) D} \sin \frac{2\pi c}{\lambda b}(p \cos \alpha - q \sin \alpha)$$
$$- \frac{\lambda b}{2\pi q} \cdot \frac{\lambda b a}{2\pi (p + q \tan \alpha) D} \sin \frac{2\pi c}{\lambda b}(p \cos \alpha + q \sin \alpha)$$

$$N = \frac{\lambda b}{2\pi q} \cdot \frac{\lambda b a}{2\pi D} \cdot \frac{-1 + \cos \frac{2\pi c}{\lambda b}(p \cos \alpha - q \sin \alpha)}{p - q \tan \alpha}$$
$$+ \frac{\lambda b}{2\pi q} \cdot \frac{\lambda b a}{2\pi D} \cdot \frac{1 - \cos \frac{2\pi c}{\lambda b}(p \cos \alpha + q \sin \alpha)}{p + q \tan \alpha}.$$

By developing the sines and cosines, these expressions become,

$$M = \frac{\lambda^2 b^2 a}{2\pi^2 D\,(p^2 - q^2 \tan^2 a)} \left\{ \tan a \sin\left(\frac{2\pi}{\lambda b} pc \cos a\right) \cos\left(\frac{2\pi}{\lambda b} qc \sin a\right) \right.$$

$$\left. - \frac{p}{q} \cos . \left(\frac{2\pi}{\lambda b} pc \cos a\right) \sin\left(\frac{2\pi}{\lambda b} qc \sin a\right) \right\}.$$

$$N = \frac{\lambda^2 b^2 a}{2\pi^2 D\,(p^2 - q^2 \tan^2 a)} \left\{ -\tan a + \tan a \cos\left(\frac{2\pi}{\lambda b} pc \cos a\right) \cos\left(\frac{2\pi}{\lambda b} qc \sin a\right), \right.$$

$$\left. + \frac{p}{q} \sin\left(\frac{2\pi}{\lambda b} pc \cos a\right) \sin\left(\frac{2\pi}{\lambda b} qc \sin a\right) \right\}$$

Abbreviate $\frac{2\pi}{\lambda b} p c \cos a$ by x, and $\frac{2\pi}{\lambda b} q c \sin a$ by y; then $\frac{p}{q} = \frac{x}{y} \tan a$;

and $p^2 - q^2 \tan^2 a = \dfrac{x^2 - y^2}{\left(\frac{2\pi c \cos a}{\lambda b}\right)^2}$.

$\therefore$ $M^2 + N^2$, or the intensity at the point whose co-ordinates are p, q, is

$$\frac{4 a^2 c^4}{D^2 (x^2 - y^2)^2} \sin^2 a \cos^2 a \left\{ 1 - 2\cos x \cos y - \frac{2x}{y} \sin x \sin y + \cos^2 y + \frac{x^2}{y^2} \sin^2 y \right\}.$$

If then u = total intensity, we have

$$u = 4 \int_0^\infty \int_0^\infty dp\, dq\, (M^2 + N^2)$$

$$= \frac{4 a^2 c^2 \lambda^2 b^2 \sin a \cos a}{\pi^2 D^2} \int_0^\infty \int_0^\infty \frac{dx\, dy}{(x^2 - y^2)^2} \left\{ 1 - 2\cos x \cos y - \frac{2x}{y} \sin x \sin y + \cos^2 y + \frac{x^2}{y^2} \sin^2 y \right\}.$$

We proceed now to find the value of this integral. To effect this we assume the following as proved, $\int_0^\infty \frac{a \cos qx\, dx}{a^2 + x^2} = \frac{\pi}{2} e^{-qa}$ . . . . (1.)

Dividing by a, and then differentiating with respect to a, we get,

$$\int_0^\infty \frac{\cos qx\, dx}{(a^2 + x^2)^2} = \frac{\pi}{4} \frac{qa + 1}{a^3} e^{-qa} \quad . \; . \; . \; . \; . \; . \; . \; . \quad (2.)$$

Differentiating this with respect to q

$$\int_0^\infty \frac{x \sin qx\, dx}{(a^2 + x^2)^2} = \frac{\pi q}{4a} e^{-qa} \quad . \; . \; . \; . \; . \; . \; . \; . \; . \quad (3.)$$

Now $1 + \cos^2 y + \frac{x^2}{y^2} \sin^2 y = 2 + \frac{x^2 - y^2}{y^2} \sin^2 y$.

Put $a = \sqrt{-1} . y$, $q = 0$ in (2) $\therefore \int_0^\infty \frac{dx}{(x^2 - y^2)^2} = -\frac{\pi}{4 y^3 \sqrt{-1}}$.

Put $a = \sqrt{-1}\, y$, $q = 0$ in (1) $\therefore \int_0^\infty \frac{dx}{y^2 (x^2 - y^2)} = \frac{\pi}{2 y^3 \sqrt{-1}}$.

Put $a=\sqrt{-1}\,y, q=1$ in (2) $\therefore \int_o^\infty \frac{\cos x\,dx}{(x^2-y^2)^2} = -\frac{\pi}{4y^3\sqrt{-1}}(\sqrt{-1}\,y+1)\overline{e}^{y\sqrt{-1}}$

Put $a=\sqrt{-1}\,y, q=1$ in (3) $\therefore \int_o^\infty \frac{x\sin x\,dx}{y(x^2-y^2)^2} = \frac{\pi}{4y^2\sqrt{-1}}\overline{e}^{y\sqrt{-1}}$

so that we get

$$u = \frac{4a^2c^2\lambda^2b^2\sin\alpha\cos\alpha}{\pi D^2\sqrt{-1}}\int_o^\infty dy\left\{-\frac{1}{2y^3}+\frac{1}{2y^3}\sin^2 y+\frac{e^{-y\sqrt{-1}}}{2y^3}(\sqrt{-1}\,y+1)\cos y\right.$$
$$\left.-\frac{1}{2y^2}e^{-y\sqrt{-1}}\sin y\right\}$$
$$=\frac{2a^2c^2\lambda^2b^2\sin\alpha\cos\alpha}{\pi D^2\sqrt{-1}}\int_o^\infty dy\left\{-\frac{\cos^2 y}{y^3}+\frac{1}{y^3}(\sqrt{-1}\,y+1)\cos y(\cos y-\sqrt{-1}\sin y)\right.$$
$$\left.-\frac{1}{y^2}\sin y(\cos y-\sqrt{-1}\sin y)\right\}$$
$$=\frac{2a^2c^2\lambda^2b^2\sin\alpha\cos\alpha}{\pi D^2}\int_o^\infty dy\left\{\frac{1}{y^2}-\frac{\sin y\cos y}{y^3}\right\}.$$

From (1), by putting $q=0$, $a=0$, we get $\int_o^\infty \frac{dx}{x^2}=\frac{\pi}{2a}$, $a=0\ldots\therefore\int_o^\infty\frac{dy}{y^2}=\frac{\pi}{2a}$.

Also $\int_o^\infty \frac{\sin y\cos y\,dy}{y^3}=\frac{1}{2}\int_o^\infty\frac{\sin 2y}{y^3}dy=2\int_o^\infty\frac{\sin x}{x^3}dx,$

where $x=2y$ $\quad\therefore\int_o^\infty\frac{\sin y\cos y}{y^3}dy=\frac{\pi}{2}\frac{e^{-a}}{a}$ from (3.), q being equal to 1, and a to 0.

Hence $\int_o^\infty\left(\frac{dy}{y^2}-\frac{dy\sin y\cos y}{y^3}\right)=\frac{\pi}{2}\left(\frac{1}{a}-\frac{e^{-a}}{a}\right)=\frac{\pi}{2}$ since $a=0$.

$$\therefore u=\frac{a^2c^2\lambda^2b^2\sin\alpha\cos\alpha}{D^2}=\frac{\lambda^2b^2}{D^2}\times a^2\times c^2\sin\alpha\cos\alpha$$
$$=\frac{\lambda^2b^2}{D^2}\times a^2\times\text{area of aperture.}$$

Now it ought to be $a^2\times$ area of aperture

$$\therefore D=\lambda b.$$

Remark.—The intensity at the point p, q is expressed by

$$\frac{A}{(x^2-y^2)^2}\left\{(\cos x-\cos y)^2+\left(\sin x-\frac{x}{y}\sin y\right)^2\right\}.$$

But this is not the problem as it is most frequently presented to us. We must therefore solve another case.

CASE 2. When the centre of gravity of the triangle falls in the line with the focus of the lens.

Here our limits are $y=-\left(x+\frac{2\,c\cos\alpha}{3}\right)\tan\alpha,\ y=\left(x+\frac{2\,c\cos\alpha}{3}\right)\tan\alpha;$

$$x=-\frac{2\,c\,\cos\alpha}{3},\ x=\frac{c\,\cos\alpha}{3}.$$

Integrating with respect to y we get

$$\frac{\lambda\,b\,a}{2\,\pi\,q\,\mathrm{D}}\int dx\left\{\cos\frac{2\,\pi}{\lambda}\left(\overline{v\,t-\mathrm{B}+\frac{p\,x}{b}-\frac{q}{b}\tan\alpha\,x+\frac{2\,c}{3}\cos\alpha}\right)\right.$$

$$\left.-\cos\frac{2\,\pi}{\lambda}\left(\overline{v\,t-\mathrm{B}+\frac{p\,x}{b}+\frac{q}{b}\tan\alpha\,x+\frac{2\,c}{3}\cos\alpha}\right)\right\}$$

$$\therefore\ \mathrm{M}=\frac{\lambda\,b}{2\,\pi\,q\,\mathrm{D}}\cdot\frac{\lambda\,b\,a}{2\,\pi\,(p-q\tan\alpha)}\left\{\sin\frac{2\,\pi\,c}{\lambda\,b}\left(\frac{p\cos\alpha}{3}-q\sin\alpha\right)+\sin\frac{2\,\pi\,c}{\lambda\,b}\,\frac{2\,p\cos\alpha}{3}\right\}$$

$$-\frac{\lambda\,b}{2\,\pi\,q\,\mathrm{D}}\cdot\frac{\lambda\,b\,a}{2\,\pi\,(p+q\tan\alpha)}\left\{\sin\frac{2\,\pi\,c}{\lambda\,b}\left(\frac{p\cos\alpha}{3}+q\sin\alpha\right)+\sin\frac{2\,\pi\,c}{\lambda\,b}\,\frac{2\,p\cos\alpha}{3}\right\}$$

$$\mathrm{N}=\frac{\lambda\,b}{2\,\pi\,q\,\mathrm{D}}\cdot\frac{\lambda\,b\,a}{2\,\pi\,(p-q\tan\alpha)}\left\{\cos\frac{2\,\pi\,c}{\lambda\,b}\left(\frac{p\cos\alpha}{3}-q\sin\alpha\right)-\cos\frac{2\,\pi\,c}{\lambda\,b}\cdot\frac{2\,p\cos\alpha}{3}\right\}$$

$$-\frac{\lambda\,b}{2\,\pi\,q\,\mathrm{D}}\cdot\frac{\lambda\,b\,a}{2\,\pi\,(p+q\tan\alpha)}\left\{\cos\frac{2\,\pi\,c}{\lambda\,b}\left(\frac{p\cos\alpha}{3}+q\sin\alpha\right)-\cos\frac{2\,\pi\,c}{\lambda\,b}\,\frac{2\,p\cos\alpha}{3}\right\}.$$

If we adopt the notation previously used, this gives

$$\mathrm{M}=\frac{\lambda\,b\,a\,c\cos\alpha}{2\,\pi\,q\,\mathrm{D}}\left\{\frac{\sin\left(\frac{x}{3}-y\right)+\sin\frac{2x}{3}}{x-y}-\frac{\sin\left(\frac{x}{3}+y\right)+\sin\frac{2x}{3}}{x+y}\right\}$$

$$=\frac{2\,a\,c^2\sin\alpha\cos\alpha}{(x^2-y^2)\,y\,\mathrm{D}}\left\{y\sin\frac{x}{3}\cos y-x\cos\frac{x}{3}\sin y+y\sin\frac{2x}{3}\right\}$$

$$\mathrm{N}=\frac{2\,a\,c^2\sin\alpha\cos\alpha}{(x^2-y^2)\,y\,\mathrm{D}}\left\{y\cos\frac{x}{3}\cos y+x\sin\frac{x}{3}\sin y-y\cos\frac{2x}{3}\right\}$$

$$\therefore\ \mathrm{M}^2+\mathrm{N}^2=\frac{4\,a^2\,c^4\sin^2\alpha\cos^2\alpha}{\mathrm{D}^2\,(x^2-y^2)^2}\left\{1+\cos^2 y-2\cos x\cos y-\frac{2x}{y}\sin x\sin y+\frac{x^2}{y^2}\sin^2 y\right\}$$

$$=\frac{4\,a^2\,c^4\sin^2\alpha\cos^2\alpha}{\mathrm{D}^2\,(x^2-y^2)^2}\left\{(\cos x-\cos y)^2+(\sin x-\frac{x}{y}\sin y)^2\right\}.$$

As this expression is precisely the same as that in the last case, it is unnecessary to integrate it.

PROB. IV. *Every thing the same, except that the aperture is a circle concentric with the lens.*

The vibration at the point M, whose distance from the focus of the lens is ρ, due to an element of the front of the wave at P, whose distance from the centre is r,

and angular distance from a plane passing through the axis of the lens and the point M is θ, is

$$a\, r\, d\, r\, d\,\theta \sin \frac{2\pi}{\lambda}(v\,t-\mathrm{PM}).$$

Now $\quad \mathrm{P\,M}^2=\varrho^2+b^2-2\,r\,\varrho\cos\theta;$

$\therefore \quad \mathrm{P\,M}=\mathrm{B}-\frac{r\varrho}{b}\cos\theta$ nearly; and the vibration becomes

$$\frac{a\,r}{\mathrm{D}}\,d\,r\,d\,\theta\sin\frac{2\pi}{\lambda}\left(v\,t-\mathrm{B}+\frac{r\varrho}{b}\cos\theta\right).$$

Let $\frac{a}{\mathrm{D}}\int_0^c\int_0^{2\pi} r\,d\,r\,d\,\theta\sin\left(\frac{2\pi}{\lambda}\frac{r\varrho}{b}\cos\theta\right)=\mathrm{M}$

$$\frac{a}{\mathrm{D}}\int_0^c\int_0^{2\pi} r\,d\,r\,d\,\theta\cos\left(\frac{2\pi}{\lambda}\frac{r\varrho}{b}\cos\theta\right)=\mathrm{N},$$

then the intensity at the point M is $\mathrm{M}^2+\mathrm{N}^2$.

Now $\mathrm{M}=\frac{a}{\mathrm{D}}\int_0^{2\pi} d\,\theta\left\{-\frac{c\,\lambda\,b}{2\,\pi\,\varrho\cos\theta}\cos\phi+\left(\frac{\lambda\,b}{2\,\pi\varrho\cos\theta}\right)^2\sin\phi\right\}$

where $\quad \phi=\frac{2\,\pi\,\varrho\,c}{\lambda\,b}\cos\theta.$

$$\therefore \quad \mathrm{M}=\frac{a\,c^2}{\mathrm{D}}\int_0^{2\pi} d\,\theta\left\{-\frac{\cos\phi}{\phi}+\frac{\sin\phi}{\phi^2}\right\}$$

$$=\frac{a\,c^2}{\mathrm{D}}\int_0^{2\pi} d\,\theta\left\{-\frac{1-\frac{\phi^2}{1.2}+\frac{\phi^4}{1.2.3.4}-\&\text{c.}}{\phi}+\frac{\phi-\frac{\phi^5}{1.2.3}+\&\text{c.}}{\phi^2}\right\}$$

$$=\frac{a\,c^2}{\mathrm{D}}\int_0^{2\pi} d\,\theta\,\Sigma(-1)^r\left(\frac{\phi^{2r+1}}{\underline{/2\,r+2}}-\frac{\phi^{2r+1}}{\underline{/2\,r+3}}\right)$$

$$=\frac{a\,c^2}{\mathrm{D}}\int_0^{2\pi} d\,\theta\,\Sigma\frac{(-1)^r}{\underline{/2\,r+3}}(2\,r+2)\,\phi^{2r+1};$$

where $\quad \underline{/2\,r}$ is an abbreviation for $1\,.\,2\,.\,3\ldots 2\,r$.

Now $\quad \int_0^{2\pi} d\,\theta\,(\cos\theta)^{2r+1}=0 \quad \therefore$ every term of the above expression is zero; and consequently $\mathrm{M}=0$.

Again, $\mathrm{N}=\frac{a}{\mathrm{D}}\int_0^{2\pi} d\,\theta\left\{\frac{\lambda\,b\,c}{2\,\pi\,\varrho\cos\theta}\sin\phi+\left(\frac{\lambda\,b}{2\,\pi\,\varrho\cos\theta}\right)^2(\cos\phi-1)\right\}.$

Denote $\frac{2\,\pi\varrho\,c}{\lambda\,b}$ by m.

$$\therefore \quad \mathrm{N}=\frac{a\,c^2}{\mathrm{D}}\int_0^{2\pi} d\,\theta\left\{\frac{\sin(m\cos\theta)}{m\cos\theta}-\frac{1-\cos(m\cos\theta)}{(m\cos\theta)^2}\right\}$$

$$=\frac{a c^2}{\mathrm{D}}\int_0^{2\pi} d\theta\left\{1-\frac{m^2\cos^2\theta}{\underline{|3}}+\frac{m^4\cos^4\theta}{\underline{|5}}-\&c.-\frac{1}{\underline{|2}}+\frac{m^2\cos^2\theta}{\underline{|4}}-\&c.\right\}$$

$$=\frac{a c^2}{\mathrm{D}}\int_0^{2\pi} d\theta\left\{1-\frac{1}{\underline{|2}}-m^2\cos^2\theta\left(\frac{1}{\underline{|3}}-\frac{1}{\underline{|4}}\right)+m^4\cos^4\theta\left(\frac{1}{\underline{|5}}-\frac{1}{\underline{|6}}\right)-\&c.\right\}$$

Now $$\int_0^{2\pi} d\theta\cos^{2r}\theta=\frac{(2r-1)(2r-3)\dots.1}{2r\ \ (2r-2)\dots.2}\cdot 2\pi;$$

$$\therefore\quad \mathrm{N}=\frac{2\pi a c^2}{\mathrm{D}}\left\{1-\frac{1}{\underline{|2}}-\frac{1}{2}\left(\frac{1}{\underline{|3}}-\frac{1}{\underline{|4}}\right)m^2+\frac{1\,.\,3}{2\,.\,4}\left(\frac{1}{\underline{|5}}-\frac{1}{\underline{|6}}\right)m^4-\&c.\right\}.$$

The coefficient of m^{2r} between brackets is

$$(-1)^r\cdot\frac{1.3\dots(2r-1)}{2.4\dots 2r}\cdot\left(\frac{1}{\underline{|2r+1}}-\frac{1}{\underline{|2r+2}}\right) \text{ which is equal to}$$

$$(-1)^r\cdot\frac{1.3\dots(2r-1)}{2.4\dots 2r}\cdot\frac{1}{\underline{|2r}}\cdot\frac{1}{2r+2}$$

$$=(-1)^r\cdot\frac{1.3\dots(2r-1)}{2^r\ \ \underline{|r}}\cdot\frac{2.4\dots 2r}{2^r\,1.2\dots r}\cdot\frac{1}{\underline{|2r}}\cdot\frac{1}{2r+2}$$

$$=(-1)^r\frac{1}{2^{2r}\,(\underline{|r})^2\,(2r+2)}$$

$$\therefore\quad \mathrm{N}=\frac{\pi a c^2}{\mathrm{D}}\left\{1-\frac{m^2}{2\,.\,2^2}+\frac{m^4}{3\,.\,2^4\,(\underline{|2})^2}\ \&c.\ \right\}$$

Let $\frac{m^2}{2}=m'$

$$\therefore\quad \mathrm{N}=\frac{\pi a c^2}{\mathrm{D}}\left\{1-\frac{m'}{2}+\frac{m'^2}{3(\underline{|2})^2}\ \&c.\ -\ \frac{m'^3}{4(\underline{|3})^2}+\&c.\,(-1)^r\frac{m'^r}{(r+1)\,(\underline{|r})^2}\ \&c.\ \right\}.$$

By squaring $\frac{\mathrm{N}}{\pi a c^2}$, we obtain, coefficient of m'^r in $\left(\frac{\mathrm{N D}}{\pi\ a\ c^2}\right)^2=$

$$(-1)^r\left\{1\cdot\frac{1}{(r+1)\,(\underline{|r})^2}+\frac{1}{2}\cdot\frac{1}{(\underline{|1})^2\,r\,(\underline{|r-1})^2}+\frac{1}{3\,(\underline{|2})^2}\cdot\frac{1}{(r-1)\,(\underline{|r-2})^2}+\ \&c.\text{ to } r+1 \text{ terms}\right\}$$

$$=\frac{(-1)^r}{(\underline{|r+1})^2}\left\{1.\frac{r+1}{1}+\frac{r}{2}\cdot\frac{(r+1)^2}{(\underline{|1})^2}+\frac{r-1}{3}\cdot\frac{\overline{r\,(r+1)}|^2}{(\underline{|2})^2}\ +\ \&c.\right\}$$

$$=\frac{(-1)^r}{(\underline{|r+1})^2}\left\{1.\frac{r+1}{1}+\frac{r+1}{1}\cdot\frac{(r+1)r}{1\,.\,2}+\frac{(r+1)r}{1\,.\,2}\cdot\frac{(r+1)(r)(r-1)}{1\,.\,2\,.\,3}\ +\ \&c.\right\}$$

$=\frac{(-1)^r}{(\underline{|r+1})^2}$ × coefficient of the products two and two of the consecutive terms of the expansion of $(1+x)^{r+1}$, *i. e.* 1st coefficient × 2^d + 2^d × 3^d + &c.

$$= \frac{(-1)^r}{(\underline{|r+1})^2} \times \text{coefficient of } x^r \text{ in the expansion of } (1+x)^{2r+2}$$

$$= \frac{(-1)^r}{(\underline{|r+1})^2} \cdot \frac{(2r+2)(2r+1)\ldots\ldots(r+3)}{1.2\ldots\ldots\ldots\ldots r}$$

$$= \frac{(-1)^r}{(\underline{|r+1})^2} \quad \frac{\underline{|2r+2}}{\underline{|r}\,\underline{|r+2}}$$

$$= \frac{(-1)^r}{(\underline{|r+1})^2} \quad 2^{r+1} \quad \frac{\big(1.3.5\ldots(2r+1)\big)\underline{|r+1}}{\underline{|r}\,\underline{|r+2}}$$

$$= (-1)^r \quad 2^{2r+2} \quad \frac{1.3.5\ldots(2r+1)}{2.4.6\ldots(2r+2)} \cdot \frac{1}{\underline{|r}\,\underline{|r+2}}$$

$$\therefore \quad \left(\frac{\mathrm{N\,D}}{\pi\, a\, c^2}\right)^2 = 4\,\Sigma\,(-1)^r \frac{1.3\ldots(2r+1)}{2.4\ldots(2r+2)} \quad \frac{m^{2r}}{\underline{|r}\,\underline{|r+2}}.$$

Now, *the total intensity* $= \int_o^\infty 2\pi\,\varrho\, d\varrho\, \mathrm{N}^2$

$$= \left(\frac{\lambda b}{2\pi c}\right)^2 2\pi \int_o^\infty \mathrm{N}^2\, m\, dm$$

$$= \left(\frac{\lambda b}{2\pi c}\right)^2 2\pi \left(\frac{\pi a c^2}{\mathrm{D}}\right)^2 4\int_o^\infty m\, dm\, \Sigma\,(-1)^r \frac{1.3\ldots(2r+1)}{2.4\ldots(2r+2)} \frac{m^{2r}}{\underline{|r}\,\underline{|r+2}}$$

$$= \frac{2\pi a^2 c^2 \lambda^2 b^2}{\mathrm{D}^2} \int_o^\infty \Sigma\,(-1)^r \frac{1.3\ldots(2r+1)}{2.4\ldots(2r+2)} \frac{m^{2r+1}}{\underline{|r}\,\underline{|r+2}}\, dm$$

$$= \frac{\pi a^2 c^2 \lambda^2 b^2}{\mathrm{D}^2} \Sigma\,(-1)^r \frac{1.3\ldots(2r+1)}{2.4\ldots(2r+2)} \quad \frac{m^{2r+2}}{\underline{|r+1}\;\underline{|r+2}},$$

the smallest value of r being 0.

Now $\quad e^{-\sqrt{m}\,y\,z} = \Sigma\,(-1)^r \frac{(\sqrt{m}\,y\,z)^r}{\underline{|r}}$

$$e^{-\sqrt{m}\frac{z}{y}} = \Sigma\,(-1)^r \frac{\left(\sqrt{m}\,\frac{z}{y}\right)^r}{\underline{|r}}$$

beginning with $r = 0$.

Hence that part of $y\, e^{-\sqrt{m}\,y\,z}\; e^{-\sqrt{m}\frac{z}{y}}$, which does not contain y is

$$-\left\{ z\sqrt{m} + \frac{z^3 m^{\frac{3}{2}}}{\underline{|1}\,\underline{|2}} + \frac{z^5 m^{\frac{5}{2}}}{\underline{|2}\,\underline{|3}} + \ldots \right\}$$

or, that part of $\dfrac{y\, e^{-\sqrt{m}\,\left(y+\frac{1}{y}\right)z}}{z\sqrt{m}}$ which does not contain y is $-\Sigma \dfrac{z^{2r+2}m^{r+1}}{\underline{/r+1}\ \underline{/r+2}}$

beginning with $r=-1$.

also $$\frac{1}{\sqrt{1+\frac{m}{z^2}}}=1-\frac{1}{2}\frac{m}{z^2}+\ldots\ldots(-1)^{r+1}\frac{1.3\ldots(2r+1)}{2.4\ldots(2r+2)}\frac{m^{r+1}}{z^{2r+2}}+\&c.$$

$$=\Sigma(-1)^{r+1}\frac{1.3\ldots(2r+1)}{2.4\ldots(2r+2)}\frac{m^{r+1}}{z^{2r+2}}$$

beginning with $r=-1$.

$\therefore$ that part of the product $\dfrac{ye^{-\sqrt{m}\,z\left(y+\frac{1}{y}\right)}}{z\sqrt{m}\sqrt{1+\frac{m}{z^2}}}$

which contains neither y nor z, is

$\Sigma(-1)^r\dfrac{1.3\ldots(2r+1)}{2.4\ldots(2r+2)}\dfrac{m^{2r+2}}{\underline{/r+1}\underline{/r+2}}$, beginning with $r=-1$, or (since the first term is -1); that part of the product which does not contain y or z in

$\dfrac{ye^{-\sqrt{m}\,z\left(y+\frac{1}{y}\right)}}{z\sqrt{m}\sqrt{1+\frac{m}{z^2}}}+1$ is $\Sigma(-1)^r\dfrac{1.3\ldots(2r+1)}{2.4\ldots(2r+2)}\dfrac{m^{2r+2}}{\underline{/r+1}\ \underline{/r+2}}$

beginning with $r=0$.

But this is precisely one of the factors of the expression for the total intensity.

$$\therefore\quad \text{total intensity} = \frac{\pi a^2 c^2 \lambda^2 b^2}{D^2}\times$$

$$\left(\text{that term which contains neither } y \text{ nor } z \text{ in } 1+\frac{y\,e^{-\sqrt{m}\,z\left(y+\frac{1}{y}\right)}}{z\sqrt{m}\sqrt{1+\frac{m}{z^2}}}\right).$$

Now, the actual value of m is infinity, since the integral has to be taken between the limits 0 and ∞, and has been expanded in such a form as to vanish for the former limit. But in order that the expression which we have just determined may also vanish when $m=0$, which it must do if it be the proper formula for expansion, we must add to it some function which does not contain a term independent of y or z. We may add *any* such term we please. For our present object it is not necessary to add any at all; but it may be thought fit to do so.

We observe, then, that $\dfrac{-y}{2\sqrt{m}\sqrt{1+\frac{m}{z^2}}}$, which always involves y, will suffice.

$$\therefore \quad \text{total intensity} = \frac{\pi a^2 c^2 \lambda^2 b^2}{D^2} \times$$

(that term which contains neither y nor z in $1 + \dfrac{y\left(e^{-\sqrt{m}\, z\left(y+\frac{1}{y}\right)} - 1\right)}{z\sqrt{m}\sqrt{1+\frac{m}{z^2}}}$, when $m = \infty$).

But $\dfrac{y\left(e^{-\sqrt{m}\, z\left(y+\frac{1}{y}\right)} - 1\right)}{z\sqrt{m}\sqrt{1+\frac{m}{z^2}}} = 0$, whatever be y or z, when $m = \infty$. Hence the part of it which does not contain y or z is 0 also.

$$\therefore \quad \text{total intensity} = \frac{\pi a^2 c^2 \lambda^2 b^2}{D^2} = \frac{\lambda^2 b^2}{D^2}\, a^2 \times \text{area of aperture.}$$

Hence we obtain $D = \lambda\, b$.

PROB. V.—*A series of plane waves are reflected at each of two equal mirrors inclined by an angle to each other, and are brought by a lens to a screen; to find the total intensity of light at the screen.*

Let x, y, z, be the co-ordinates of a point in the front of the wave; p, q, those of the point in the screen; the origin being that point of the screen which lies in the centre of the line joining the foci, whose distance is $2f$, the axis of x being in this line, and the plane of $x\,y$ the screen.

$$\therefore \; \mathrm{PM}^2 = (p-x)^2 + (q-y)^2 + z^2 = (p+f-\overline{x+f})^2 + (q-y)^2 + z^2$$
$$= (p+f)^2 + q^2 - 2\,(\overline{p+f}\;\overline{x+f} + q\,y) + b^2.$$

$$\therefore \quad \mathrm{PM} = \mathrm{B} - \frac{(p+f)\;(x+f) + q\,y}{b} \text{ nearly, for the upper mirror.}$$

Similarly $\mathrm{QM} = \mathrm{B} + \dfrac{(p-f)\;(x'+f) - q\,y}{b}$ for the second, x' being measured *downwards* in this case.

$\therefore$ the whole vibration due to the two mirrors is

$$\frac{a}{D}\iint dx\, dy \sin\frac{2\pi}{\lambda}\left(vt. - B + \frac{(p+f)\;(x+f) + q\,y}{b}\right)$$
$$+ \frac{a}{D}\iint dx'\, dy \sin\frac{2\pi}{\lambda}\left(vt. - B - \frac{(p-f)\;(x'-f) - q\,y}{b}\right)$$

Let $2\,l$ be the length of the mirrors, g their breadth, measured perpendicular to the axis of z; then the limits are $x=g$, $x=0$, $x'=g$, $x'=0$, $y=l$, $y=-l$.

Integrating for y we obtain,

$$\text{vibration} = \frac{a\,b\,\lambda}{2\pi\,q\,\mathrm{D}}\int_o^g d\,x\left\{\cos\frac{2\pi}{\lambda}\left(\mathrm{vt.} - \mathrm{B} + \frac{(p+x)\,(x+f)-q\,l}{b}\right)\right.$$

$$-\cos\frac{2\pi}{\lambda}\left(\mathrm{vt.} - \mathrm{B} + \frac{(p+f)\,(x+f)+q\,l}{b}\right) + \cos\frac{2\pi}{\lambda}\left(\mathrm{vt.} - \mathrm{B} - \frac{(p-f)\,(x+f)+q\,l}{b}\right)$$

$$\left.-\cos\frac{2\pi}{\lambda}\left(\mathrm{vt.} - \mathrm{B} - \frac{(p-f)\,(x+f)-q\,l}{b}\right)\right\}.$$

Integrating for x, it gives

$$\frac{a\,b^2\,\lambda^2}{4\,\pi^2\,\mathrm{D}\,q}\left\{\frac{1}{p+f}\left[\sin\frac{2\,\pi}{\lambda}\left(v\,t - \mathrm{B} + \frac{(p+f)\,(g+f)-ql}{b}\right)\right.\right.$$

$$-\sin\frac{2\,\pi}{\lambda}\left(v\,t - \mathrm{B} + \frac{(p+f)\,f+ql}{b}\right) - \sin\frac{2\,\pi}{\lambda}\left(v\,t - \mathrm{B} + \frac{(p+f)\,(g+f)+ql}{b}\right)$$

$$\left.+\sin\frac{2\,\pi}{\lambda}\left(v\,t - \mathrm{B} + \frac{(p+f)\,f+ql}{b}\right)\right] + \frac{1}{p-f}\left[\sin\frac{2\,\pi}{\lambda}\left(v\,t - \mathrm{B} - \frac{(p-f)\,f+ql}{b}\right)\right.$$

$$-\sin\frac{2\,\pi}{\lambda}\left(v\,t - \mathrm{B} - \frac{(p-f)\,(g+f)+ql}{b}\right) - \sin\frac{2\,\pi}{\lambda}\left(v\,t - \mathrm{B} - \frac{(p-f)\,f-ql}{b}\right)$$

$$\left.\left.+\sin\frac{2\,\pi}{\lambda}\left(v\,t\,\mathrm{B} - \frac{(p-f)\,(g-f)-ql}{b}\right)\right]\right\}$$

$$= \frac{a\,b^2\,\lambda^2}{2\,\pi^2\,\mathrm{D}\,q}\cdot\sin\frac{2\,\pi\,q\,l}{\lambda\,b}\left\{\frac{1}{p+f}\left[-\cos\frac{2\,\pi}{\lambda}\left(v\,t - \mathrm{B} + \frac{(p+f)\,(g+f)}{b}\right)\right.\right.$$

$$\left.+\cos\frac{2\,\pi}{\lambda}\left(v\,t - \mathrm{B} + \frac{(p+f)\,f}{b}\right)\right] + \frac{1}{p-f}\left[-\cos\frac{2\,\pi}{\lambda}\left(v\,t - \mathrm{B} - \frac{(p-f)\,f}{b}\right)\right.$$

$$\left.+\cos\frac{2\,\pi}{\lambda}\left(v\,t - \mathrm{B} - \frac{(p-f)\,(g+f)}{b}\right)\right]$$

$$= \frac{a\,b^2\,\lambda^2}{\pi^2\,\mathrm{D}\,q}\sin\frac{2\,\pi\,q\,l}{\lambda\,b}\left\{\frac{\sin\frac{\pi}{\lambda}\frac{(p+f)\,g}{b}\sin\frac{2\,\pi}{\lambda}\left(\mathrm{vt.} - \mathrm{B} + \frac{(p+f)\,(g+2f)}{2\,b}\right)}{p+f}\right.$$

$$\left.+\frac{\sin\frac{\pi}{\lambda}\frac{(p-f)\,g}{b}\sin\frac{2\,\pi}{\lambda}\left(\mathrm{vt.} - \mathrm{B} - \frac{(p-f)(g+2f)}{2\,b}\right)}{p-f}\right\}$$

$$\therefore\ \text{intensity} = \left(\frac{a\,b^2\,\lambda^2}{\pi^2\,\mathrm{D}q}\sin\frac{2\,\pi\,q\,l}{\lambda\,b}\right)^2\left\{\frac{\sin\frac{\pi}{\lambda}\frac{(p+f)\,g}{b}\cos\frac{\pi}{\lambda}\frac{p\,(g+2f)}{b}}{p+f}\right.$$

$$\left.+\frac{\sin\frac{\pi}{\lambda}\frac{(p-f)\,g}{b}\cos\frac{\pi}{\lambda}\frac{p\,(g+2f)}{b}}{p-f}\right\}^2 + \left\{\frac{\sin\frac{\pi}{\lambda}\frac{(p+f)\,g}{b}\sin\frac{\pi}{\lambda}\frac{p\,(g+2f)}{b}}{p+f}\right.$$

$$\left.-\frac{\sin\frac{\pi}{\lambda}\frac{(p-f)\,g}{b}\sin\frac{\pi}{\lambda}\frac{p\,(g+2f)}{2\,b}}{p-f}\right\}^2$$

$$= \left(\frac{a\,b^2\,\lambda^2}{\pi^2 \mathrm{D}} \quad \frac{\sin\dfrac{2\pi q l}{\lambda b}}{q}\right)^2 \left\{ \frac{\sin^2 \dfrac{\pi(p+f)g}{\lambda b}}{(p+f)^2} + \frac{\sin^2 \dfrac{\pi(p-f)g}{\lambda b}}{(p-f)^2} \right.$$

$$\left. - \frac{2\sin\dfrac{\pi}{\lambda}\dfrac{(p-f)g}{b}\sin\dfrac{\pi}{\lambda}\dfrac{(p+f)g}{b}\cos\dfrac{2\pi}{\lambda}\dfrac{p(g+2f)}{b}}{p^2-f^2} \right\}$$

Now the total intensity is the double integral of this expression between the limits ∞ and $-\infty$ of p and q.

But $$\int_o^\infty \left(\frac{\sin x}{x}\right)^2 = \frac{\pi}{2};\ \text{let } x = \frac{2\pi q l}{\lambda b}$$

$$\therefore \quad \int_{-\infty}^{\infty} \left(\frac{\sin\dfrac{2\pi q l}{\lambda b}}{\dfrac{2\pi q l}{\lambda b}}\right)^2 \frac{2\pi l}{\lambda b}\, d q = \pi$$

and total intensity $$= \frac{2\pi^2 l}{\lambda b} \frac{a^2 b^4 \lambda^4}{\pi^4 \mathrm{D}^2} \int_{-\infty}^{\infty} d p \left\{ \frac{\sin^2\dfrac{\pi(p+f)g}{\lambda b}}{(p+f)^2} + \frac{\sin^2\dfrac{\pi(p-f)g}{\lambda b}}{(p-f)^2} \right.$$

$$\left. - \frac{2\sin\dfrac{\pi(p+f)g}{\lambda b}\sin\dfrac{\pi(p-f)g}{\lambda b}\cos\dfrac{2\pi p(g+2f)}{\lambda b}}{p^2-f^2} \right\}.$$

Now $$\int_{-\infty}^{\infty} \phi(p\pm f)\, d p = \int_{-\infty}^{\infty} \phi(p\pm f)\, d(p\pm f)$$

$$= 2\int_o^\infty \phi(p\pm f)\, d(p\pm f)$$

$$\therefore \quad \int_{-\infty}^{\infty} d p \frac{\sin^2\dfrac{\pi(p+f)g}{\lambda b}}{(p+f)^2} = \frac{\pi^2 g}{\lambda b}$$

$$\int_{-\infty}^{\infty} d p \frac{\sin^2\dfrac{\pi(p-f)g}{\lambda b}}{(p-f)^2} = \frac{\pi^2 g}{\lambda b};$$

$\therefore$ total intensity $$= \frac{4 b^2 \lambda^2 a^2 l g}{\mathrm{D}^2} -$$

$$4\frac{a^2 b^3 \lambda^3 l}{\pi^2 \mathrm{D}^2} \int_{-\infty}^{\infty} d p \frac{\sin\dfrac{\pi(p+f)g}{\lambda b}\sin\dfrac{\pi(p-f)g}{\lambda b}\cos\dfrac{2\pi p(g+2f)}{\lambda b}}{p^2-f^2}$$

and it remains only that we find the value of this integral.

We have $$4\sin\frac{\pi(p+f)g}{\lambda b}\sin\frac{\pi(p-f)g}{\lambda b}\cos\frac{2\pi p(g+2f)}{\lambda b}$$

$$= 2\cos\frac{2\pi f g}{\lambda b}\cos\frac{2\pi p(g+2f)}{\lambda b} - \cos\frac{4\pi p f}{\lambda b} - \cos\frac{4\pi p(g+f)}{\lambda b}$$

Now, by formula (1.) Prob. III, we can obtain the value of each of the integrals of these expressions by writing $f\sqrt{-1}$ for a, and putting for q its corresponding value: thus,

$$\int_{-\infty}^{\infty} \frac{\cos \dfrac{2\pi p (g+2f)}{\lambda b}}{p^2-f^2} dp = 2\int_{o}^{\infty} \frac{\cos q p}{p^2-f^2} dp = \frac{\pi e^{-\frac{2\pi (g+2f) f\sqrt{-1}}{\lambda b}}}{f\sqrt{-1}}$$

$$\int_{-\infty}^{\infty} \frac{\cos \dfrac{4\pi p f}{\lambda b}}{p^2-f^2} dp = \frac{\pi e^{-\frac{4\pi f^2\sqrt{-1}}{\lambda b}}}{f\sqrt{-1}}$$

$$\int_{-\infty}^{\infty} \frac{\cos \dfrac{4\pi p (g+f)}{\lambda b}}{p^2-f^2} dp = \frac{\pi e^{-\frac{4\pi (g+f) f\sqrt{-1}}{\lambda b}}}{f\sqrt{-1}}$$

$$\therefore \int_{-\infty}^{\infty} \frac{4 \sin \dfrac{\pi (p+f) g}{\lambda b} \sin \dfrac{\pi (p-f) g}{\lambda b} \cos \dfrac{2\pi p (g+2f)}{\lambda b} dp}{p^2-f^2}$$

$$= \frac{\pi}{f\sqrt{-1}} \left\{ 2 \cos \frac{2\pi f g}{\lambda b} e^{-\frac{2\pi (g+2f) f\sqrt{-1}}{\lambda b}} - e^{-\frac{4\pi f^2\sqrt{-1}}{\lambda b}} - e^{-\frac{4\pi (g+f) f\sqrt{-1}}{\lambda b}} \right\}$$

$$= \frac{\pi}{f\sqrt{-1}} \left\{ 2 \cos \frac{2\pi f g}{\lambda b} \left(\cos \frac{2\pi (g+2f) f}{\lambda b} - \sqrt{-1} \sin \frac{2\pi (g+2f) f}{\lambda b} \right.\right.$$

$$\left.\left. - \cos \frac{4\pi f^2}{\lambda b} + \sqrt{-1} \sin \frac{4\pi f^2}{\lambda b} - \cos \frac{4\pi (g+f) f}{\lambda b} + \sqrt{-1} \sin \frac{4\pi (g+f) f}{\lambda b} \right\}$$

$$= \frac{\pi}{f\sqrt{-1}} \left\{ \cos \frac{4\pi (g+f) f}{\lambda b} + \cos \frac{4\pi f^2}{\lambda b} - \sqrt{-1} \sin \frac{4\pi (g+f) f}{\lambda b} \right.$$

$$- \sqrt{-1} \sin \frac{4\pi f^2}{\lambda b} - \cos \frac{4\pi f^2}{\lambda b} + \sqrt{-1} \sin \frac{4\pi f^2}{\lambda b} - \cos \frac{4\pi (g+f) f}{\lambda b}$$

$$\left. + \sqrt{-1} \sin \frac{4\pi (g+f) f}{\lambda b} \right\} = 0.$$

Hence the expression for the total intensity is reduced to $\dfrac{4b^2 \lambda^2 a^2 l g}{D^2} = \dfrac{b^2 \lambda^2 a^2}{D^2} \times$ sum of areas of the mirrors estimated perpendicularly to the line which bisects the angle between; or which is the same thing,

$$\text{total intensity} = \frac{b^2 \lambda^2 a^2}{D^2} \times \text{effective aperture.}$$

But it ought to be, $a^2 \times$ effective aperture

$$\therefore \qquad D^2 = b^2 \lambda^2 \text{ or } D = b\lambda.$$

Conclusion.—It appears that in all the Problems, the result is one and the same, that the divisor is $b\lambda$. Hence, we enunciate Huyghens' principle as follows:—*The vibration at a given point, caused by a given wave, is found by taking the front of the wave, dividing it into an indefinite number of small parts, considering the agitation of each of these parts as the origin of a wave whose maximum of vibration, on reaching the point, is equal to the quotient of that at the disturbing point, divided by the product of the length of the wave, and the perpendicular from the disturbed point on the front of the wave.*

XXI.—*Analysis of Caporcianite and Phakolite, two new Minerals of the Zeolite Family.* By THOMAS ANDERSON, M.D. Communicated by Dr CHRISTISON.

(Read, April 18. 1842).

THE minerals of the zeolite family have for many years attracted the especial attention of men of science, and the class has been rapidly extended in proportion to the progress made in its study in a crystallographic as well as chemical point of view. The first characteristic difference, originally observed long since by CRONSTEDT, and by him considered to be the distinguishing mark of one single mineral species, which he designated Zeolite,—namely, the property of swelling out by heat previous to fusion,—has since been found to belong to a great number of other combinations. These, although materially different from each other in crystallographic form, have proved to be closely allied in chemical constitution, in so far as they consist, without exception, of a silicate of an alkali or alkaline earth, in combination with a silicate of alumina and water. It is evident, then, that the relation of the silicic acid to the base, in both terms, as well as the quantity of water, is capable of considerable variation, so that the general mineralogical formula which should embrace all the members of the zeolite family would be

$$u\, r\, S^{v} + x\, A\, S^{y} + z\, A q.$$

Where r represents the monatomic alkaline or earthy basis, and the terms u, v, x, y, and z, are capable of varying within certain limits.

The minerals Caporcianite and Phakolite form two new members of the above general formula. Their analysis was conducted in the following manner:—

The finely pulverized mineral was dried for several days over sulphuric acid in an exsiccator, at the ordinary temperature of the atmosphere. A certain quantity of the dry powder was then weighed in a small tube retort, and heated to moderate redness for the space of half an hour. The water thus driven off was absorbed in a counterpoised tube of chloride of calcium and weighed. Another portion of the dry powder was then dissolved in hydrochloric acid, and evaporated to dryness for the separation of the silicic acid. The dry mass was then moistened with hydrochloric acid, digested for several hours, and dissolved in water, and the silicic acid filtered off. The purity of the silicic acid was then tested by solution in a boiling solution of carbonate of soda; the undissolved matter, which

consisted chiefly of silicate of lime, reproduced by the strong drying necessary for the separation of the silicic acid, was then heated to redness with carbonate of soda; and alumina and lime were precipitated respectively by ammonia and oxalate ammonia. The precipitates thus obtained, weighed and subtracted from the first weight, gave that of the pure silicic acid. The solution, after the filtration of the silicic acid, was precipitated by caustic ammonia; the precipitate, after being filtered, washed, dried, and weighed, was dissolved in hydrochloric acid, and the silicic acid left undissolved was weighed; to the filtered solution potassa was added in sufficient quantity to redissolve the alumina at first precipitated. By this means iron and magnesia were left undissolved, which were again precipitated from a solution in hydrochloric acid, the first by succinate, and the second by phosphate, of soda. The weights of the silicic acid, peroxide of iron, and magnesia, contained in the phosphate, being subtracted from the first weight of the ammoniacal precipitate, gave that of the pure alumina. The solution filtered from the ammoniacal precipitate was then treated with a solution of oxalate of ammonia; and the precipitate of oxalate of lime, after filtration and washing, was heated to strong redness, and treated several times in succession with a solution of carbonate of ammonia at a gentle heat as long as it continued to gain weight; and the lime was then weighed in the state of carbonate. The solution which was left after the separation of the oxalate of lime, was then evaporated to dryness in a counterpoised platinum crucible, and the ammoniacal salts driven off by a moderate heat; after which a higher temperature was given for the purpose of melting the remaining salts. These, which consisted of chloride of potassium, chloride of sodium, and magnesia, were weighed together. By solution in water the magnesia remained undissolved, and was filtered off, washed and weighed; to the solution, chloride of platinum and spirit were added, when the double chloride of platinum and potassium fell, which was collected on a weighed filter, and from which the quantity of chloride of potassium, and thence that of the potassa, were determined. By subtraction of the weights of magnesia and chloride of potassium from the first weight, that of the chloride of sodium was obtained from which the soda was reckoned.

Caporcianite.

This mineral was kindly presented to me for analysis by Professor Berzelius. It was first observed by Dr Paolo Savi at Caporciani, in the valley of the Cæcino, where it occurs in a copper mine worked by two Englishmen of the names of Hall and Sloane, and has been described by its discoverer in his *Memorie per servire allo studio della costituzione fisica della Toscana, parte* 2da, § 53.

Caporcianite conducts itself before the blowpipe in a manner perfectly similar to the other zeolites, in so far as its fusibility and relation to the fluxes are

concerned; but it differs from them in this much, that, previous to melting, it swells out only to a very inconsiderable degree; for it melts almost at the same instant that the swelling manifests itself.

The analysis yielded the following results:—

Silicic acid,	52.8	oxygen contained	27.43	8.
Alumina,	21.7	....	10.15	10.18—3.
Peroxide of iron,	0.1	....	0.03	
Lime,	11.3	....	3.23	3.65—1.
Magnesia,	0.4	....	0.15	
Potassa,	1.1	....	0.22	
Soda,	0.2	....	0.05	
Water,	13.1	....	11.64	3.
	100.7			

If we here express by r the monatomic bases, then the quantities of oxygen in r, A, S, and Aq are to each other as 1 : 3 : 8 : 3, which evidently determine the mineralogical formula to be $r\,S^2 + 3\,A\,S^2 + 3\,A\,q$. This when transformed to the chemical formula, becomes $\dot{r}^3\,\dddot{S}i^2 + 3\,\dddot{A}l\,\dddot{S}i^2 + 9\,\dot{H}$.

It thus appears that Caporcianite stands chemically in near relation with the minerals, Analcime, Ledererite, Potash-Harmotome, Chabasie, and Levyne, from which it is separated merely by the difference in the quantity of water which it contains. All these minerals consist of a bisilicate of the first as well as of the second term; and the quantity of oxygen in the alumina is in all of them three times that contained in the monatomic basis. The formulæ of these minerals are as follows:—

Analcime, Ledererite,	$r\,S^2 + 3\,A\,S^2 + 2\,A\,q$	$r = N.$ / $r = C.N.$
Caporcianite,	$r\,S^2 + 3\,A\,S^2 + 3\,A\,q$	$r = C.$
Potash-Harmotome,	$r\,S^2 + 3\,A\,S^2 + 5\,A\,q$	$r = K.C.$
Chabasie, Levyne,	$r\,S^2 + 3\,A\,S^2 + 6\,A\,q$	$r = C.N.$ / $r = C.K.N.$

The formula $r\,S^2 + 3\,A\,S^2$ is thus, then, known to exist in no less than four different combinations with water, namely, with 2, 3, 5, and 6 atoms, the second of which results from the foregoing analysis.

Phakolite.

This mineral occurs in small crystals in the Bohemian Mittelgebirge, and was from crystallographic investigation believed to be nearly related to Chabasie. But the following analysis shews that this supposition is not confirmed by its chemical constitution.

Phakolite, which, in its relations before the blowpipe, agrees in all respects with the other zeolites, was analyzed after the foregoing method, with this exception, that the quantity of water was determined simply by the loss of weight sustained at a red heat. The composition was found to be as follows:—

Silicic acid, .	45.628	oxygen contained 23.708.	
Alumina, .	19.480	9.077	9.221.
Peroxide of iron,	0.431	0.144	
Lime, . .	13.304	3.737	4.442.
Magnesia, .	0.143	0.053	
Potassa, . .	1.314	0.222	
Soda, . .	1.684	0.430	
Water, . .	17.976	15.982.	
	99.960		

This constitution has little resemblance to that of chabasie; for the quantities of oxygen in r, A S and A q, are to each other in chabasie, whose mineralogical formula is $r\,S^2 + 3\,A\,S^2 + 6\,A\,q$, as 1 : 3 : 8 : 6, whereas those quantities in phakolite are in the relation of 1 : 2 : 5 : $3\frac{1}{2}$. If we assume that the quantity of water has come out too high, which is generally the case when it is determined by the simple loss of weight at a red heat, then the constitution of phakolite would be represented by the mineralogical formula $r\,S^3 + 2\,A\,S + 3\,A\,q$, which transformed to the chemical, is $3\,\dot{r}\,\dddot{Si} + 2\,\dddot{Al}\,\dddot{Si} + 9\,\dot{H}$.

It appears, then, that phakolite belongs to that class of minerals which in the first term contain a tersilicate, and in the second, a simple silicate of the base, along with water. The minerals belonging to this class at present made out are:—

Gigantolite, . .	$r\,S^3 + A\,S + A\,q$	$r = fe, mg$, K.N.
Harringtonite, .	$r\,S^3 + A\,S + 2\,A\,q$	$r =$ C.N.
Mesotype, .		$r =$ N.C.
Lehuntite, . .	$r\,S^3 + A\,S + 3\,A\,q$	$r =$ (N.)C.
Phakolite, . .	$r\,S^3 + 2\,A\,S + 3\,A\,q$	$r =$ (C.)K.N.
Mezolite, . .	$r\,S^3 + 3\,A\,S + 3\,A\,q$	$r =$ N + 2 C.
Scolezite, . .		$r =$ C.
Pyrargillite, . .	$r\,S^3 + 3\,A\,S + 4\,A\,q$	$r = fe, mg$, K.N.
Antrimolite, . .	$r\,S^3 + 5\,A\,S + 5\,A\,q$	$r =$ C.(K.)

From this table it will be seen that phakolite forms a middle term between lehuntite and mezolite, and differs from them only in the second or alumina term, which in the three minerals stand to each other in the ration of 1, 2, and 3, while the quantities of silicate of the monatomic bases and water are the same in all three.

XXII.—*On the Property belonging to Charcoal and Plumbago, in fine Plates and Particles, of Transmitting Light.* By JOHN DAVY, M.D., F.R.S. L. & E., Inspector-General of Army Hospitals, L. R.

(Read 9th January 1843.)

I AM not aware that this property has yet been known to belong to these substances; they are commonly considered and spoken of as opaque, without any qualification.

It was in examining the charcoal of the pith of the elder, that I was first led to entertain doubt of the accuracy of the current opinion.

The pith of the elder consists of polyhedral cells, commonly pentagonal, of from about $\frac{1}{300}$th to $\frac{1}{400}$th inch in diameter, formed of woody matter of extraordinary fineness, as may be inferred from their transparency when seen under the microscope, and their great lightness. They are unaltered in form, when converted into charcoal. The charcoal obtained (that which I examined was from a shoot of this year gathered in December) was brilliant, as might be expected, from its consisting of plates, and very soft and brittle; in other respects, in mass, it was nowise peculiar, having the ordinary colour and opacity of charcoal. When broken up, however, and seen with a high magnifying power, the detached plates were found to be transparent in different degrees (allowing lines drawn on the glass-support to be seen under them), and of different shades of brown—passing into black on one hand, and into almost white on the other, especially as seen by reflected light. In general appearance they were not unlike mica viewed with the naked eye. No pores were visible in them; but in some there were foramina, circular, or oval, varying in diameter from about $\frac{1}{16,000}$th of an inch to $\frac{1}{4000}$th. The plates themselves varied in size from about $\frac{1}{800}$th to $\frac{1}{1000}$th of an inch, estimating their width, and selecting the most entire. So thin were they, that, under a glass magnifying 800 diameters, the most transparent had no apparent thickness; the darker, less transparent, may have had a thickness of from about $\frac{1}{20,000}$th to $\frac{1}{30,000}$th of an inch, judging from one, the edge of which, when floating in water, was so inclined as to offer a tolerable view of it.

That these plates consisted of charcoal, and were not composed of foreign adventitious matter, such as silica, potassa, or carbonate of lime, I satisfied myself by a few simple experiments. They were unaltered in the dilute mineral acids, took fire when heated, and left hardly a perceptible ash: they deflagrated with chlorate of potash, like common charcoal, and, in brief, did not appear to possess any chemical qualities connected with their attenuated state, different from those

of common charcoal. I may add, they were found in mass to conduct electricity. I may observe, further, in confirmation of what has just been stated, that I could detect no material difference in the charcoal, as regards the translucency of its plates, after it had been subjected to boiling in distilled water and in dilute muriatic acid; nor, in a specimen prepared from the pith of the elder similarly treated, and boiled also in alcohol previously to charring, nor after the charcoal had been ignited a second time; if any difference existed, it was in favour of the purified portions.

Inferring that the charcoal of the pith of the elder owes its transparency under the microscope to the thinness of its plates, I expected to find the same property exhibited by charcoal generally, in a finely divided state; and the trials I have made of different specimens have not disappointed me. The notice of a few examples may suffice.

The pith of the annual shoot of the sycamore, is very similar in structure to that of the elder; and the plates of which it consists, when reduced to charcoal, exhibit, under the microscope, a similar appearance, though not quite so distinct.

The pith of the rush is formed of radiating fibres, $\frac{1}{4000}$th inch in diameter and under, five or six of which commonly proceed from a common centre, and which are occasionally connected by a membrane or plate of extreme thinness. When charred, the fibres, by transmitted light, appear of a brownish hue, and some of them allow lines on the glass-support to be seen obscurely. The plates, by reflected light, appear almost white; by transmitted light brown or grey; through them, lines on the glass may be seen distinctly.

The charcoal of cotton shews the fibre of this substance in a very clear manner, varying in diameter from $\frac{1}{4000}$th to $\frac{1}{2000}$th inch, flattened, ribbon-like, and twisted at intervals.* The finer fibres, and the flat part of the larger, free from torsion, exhibit a certain degree of transparency under the microscope, although they do not allow lines to be seen through them, unless they have been wasted in a certain degree in the fire during the process of carbonization; the larger fibres, especially when twisted, are almost black, and nearly, if not quite, opaque.

The charcoal of linen-thread, and flax, equally well shew the form of the fibre of this substance, which is so characteristic. Smaller than the fibre of the cotton, varying in thickness from $\frac{1}{8000}$th to $\frac{1}{4000}$th of an inch, cylindrical, without any twist, it appears to be more dense than the fibre of the cotton, and is in a less degree translucent,—indeed, it is difficult to find a fibre of the charcoal made from the finest cambric, that transmits light with tolerable distinctness.

* Mr Bauer, in the account he has given of the microscopical appearance of cotton, appended to the "History of the Cotton Manufacture in Great Britain," by E. Baines jun., Esq., describes the twisted appearance of the fibres as being often owing to the junction and torsion of two fibres: this I have never witnessed, and I am induced in consequence to question the correctness of the observation; Mr Bauer may have been deceived by using a microscope of indifferent construction.

It occurred to me as probable, that the very thin and delicate flower-leaves of plants might yield charcoal of marked transparency. The petals, probably from being more heterogeneous in composition than the substances before mentioned, contract more when subjected to heat, and are more changed, than they are in form. This may be in a great measure prevented, by confining them in the process of carbonization between two surfaces of platina or silver-foil. Charcoal thus obtained from the petal of the pansy, adhering to the foil, was sufficiently translucent to allow the metallic splendour of the platinum to be seen beneath it. Examined with a high magnifying power, it displayed no pores; it appeared of a bright brown colour, where most translucent, evidently the effect of transmitted light reflected from the metal.

I shall make mention of only one other variety of vegetable charcoal, that made from oak-wood. This, when reduced by trituration to the most subtle powder, spread on glass, and held between the eye and a bright light, obscures the light in a certain degree, like soot, the matter of smoke from flame, and imparts to it a brownish red hue; and under the microscope, like soot, each minute particle appears to transmit a reddish light: the smallest particles barely within the limit of distinct vision, using a glass with a magnifying power of nearly four hundred diameters and a strong light, have a peculiar splendour, not unlike that of the dust of the diamond.*

As most animal substances enter into fusion in the process of carbonization, it is not easy to obtain animal charcoal, fit for trial, of the kind under consideration. However, such as I have tried, has exhibited pretty distinctly the same property as that from vegetables, as regards the translucency of its minute parts. I may mention particularly that from gold-beaters' skin, and that from silk.

The charcoal of gold-beaters' skin displays with great clearness the texture of this substance, provided it is prepared with care, so as to prevent the running together of its parts in fusion under the action of heat—which may be effected by charring it, spread on glass or metallic foil. The structure it exhibits under a high magnifying power, is, as it were, of two layers, very like that of gold leaf; one composed of fibres forming an irregular open net-work, almost, if not quite opaque;—the other, of a close tissue, either apparently homogeneous, or consisting of extremely minute fibres, transmitting a brownish light, about equal in strength to the green light transmitted by the finer tissue of the gold-leaf, the coarser fibre of which reflects yellow light.†

* It may be remarked that this powder, by reflected light under the microscope, appears white, but by transmitted, highly coloured; and when not in exact focus, almost black. This applies to the finest powder, viz. that just within the limits of distinct vision, using a lens magnifying about 400 diameters, as well as to powder somewhat coarser.

† Some parts of gold leaf, which, under a feeble illumination, reflect yellow light, with a stronger transmit green.

Silk, even when charred, compressed between pieces of silver-foil, in consequence of fusion, loses entirely its fibrous structure, affording a hard, brittle, charcoal.* A single fibre of silk, however, may be preserved in its filamentous form, if charred with a graduated heat on a plate of glass; when it appears as a glassy thread, expanded at intervals into globules, of different degrees of transparency, and of different shades of brown, according to their thinness.

Plumbago, in a very attenuated state, like charcoal, appears to be translucent. The powder of it, rubbed on glass so as to render dim its surface, like smoke, imparts to light transmitted through it a certain colour, a brownish hue; and seen with a high magnifying power, exhibits much the same appearance as the fine powder of the charcoal of oak-wood already mentioned; its minute particles transmit a reddish light, and are very brilliant when seen with a strong light. The streak of plumbago on glass, when very light, exhibits the same colour, and admits of a line underneath it being pretty distinctly seen. The specimens I have examined have been a foliated kind, which occurs in small quantity, disseminated through the dolomite rock of Ceylon, associated with ceylanite,—the common plumbago of commerce, sold in the state of powder,—and two pieces of the foliated kind from Cumberland. The Ceylon specimen appears to be very pure; it admits of extension under pressure, or, in other words, of a certain degree of malleability,† and, also, of having its minute fragments united by pressure, as it were by a process of welding.

Coke and anthracite, reduced to a very fine powder, I find, in regard to the transmission of light, resemble the powder of charcoal of oak-wood; the minute particles have the same brilliant appearance under the microscope. Two specimens of the former were tried, one obtained from bituminous coal, the other from compact lignite; these substances themselves differ very little from the coke they yield in the degree of transparency of their minute particles. One specimen only of anthracite was made the subject of experiment. Strong illumination was required to shew the translucency of its minute particles; many of the minute fragments, having flat surfaces, reflected white light.

* So very different in appearance is the charcoal of cotton, linen, and silk under the microscope, that the admixture of either in a fabric is more easily recognised after charring than before, especially in the instance of a mixed fabric of silk and linen that has been in use,—the coarser fibres of both being of nearly the same diameter, and, after wearing, the jointed appearance of the fibre of linen becoming very indistinct. The process of charring, I may add, may probably be employed with advantage in examining the minute structure of many of the lower vegetables, such as the byssi, confervæ, and others of the cryptogamia; one species that I have thus tried (*Byssus globosa*) displays its structure in a very distinct manner, composed of beaded fibres of about $\frac{1}{1200}$ inch in diameter.

† It is right to remark, that I first heard of this property belonging to the purer forms of plumbago from Professor JAMESON. Is it not owing to this quality that plumbago exhibits a metallic lustre when rubbed? The compact kind, when broken by main force, is without this lustre, is of a dull opaque black, not unlike fractured basalt, but on the slightest friction it acquires the lustre of a metal.

I shall offer, before concluding, a few remarks on carbon, considered in its varieties. How paradoxical are these! The diamond—most remote in its character from a metal, perfectly transparent—a non-conductor of electricity—placed at the head of the class of gems, and resembling not a little, in its general character, those oriental ones, of which a metallic oxide, alumine, is the chief constituent part. What a contrast is there between it and charcoal, a conductor of electricity, possessed of peculiar properties, especially in regard to absorption, differing in this respect from almost every other substance, excepting, indeed, the hydrate of alumine—a resemblance the more remarkable, considering their similarity in their crystalline state! And, farther, what a contrast is there between both these substances and plumbago, which possesses the perfect metallic lustre, is sectile, slightly malleable, admits of incorporation, as it were, by welding, and, in brief, has very much the character of a metal!

To what the marked differences of these substances are owing remains to be ascertained. It may be to the presence of minute portions of foreign matters, which have hitherto escaped detection, although diligently sought after. The difference in their specific gravities is in favour of this view.* Or, it may be, that

* Whilst the diamond is comparatively of the high specific gravity 3.5, I find that of charcoal, coke, and anthracite (making allowance for the ash yielded by the latter) is only about 1.5. This is the result of some trials made with considerable care. The method employed was briefly the following. In the instance of the charcoal, whilst hot from the crucible in which it had been prepared, it was weighed in air; then, with distilled water, it was subjected to the air-pump, till it sunk and ceased to give out any air, when it was weighed in water; after which it was dried, ignited, and again weighed in air.

A piece of charcoal of the oak, weighing 12 grs., thus treated, appeared to be of sp. gr. 1.519 at 53° F.; a piece of charcoal of deal, weighing 5.67 grs., of sp. gr. 1.54, and reduced to powder of the sp. gr. 1.45.

In the instances of anthracite and coke, the same method was used, omitting the weighing in air the second time, after the weighing in water, as they yielded nothing soluble in water, and ascertaining the quantity of ash, or foreign fixed matter, which each afforded on incineration.

A portion of a specimen of anthracite, for which I was indebted to Professor Jameson, weighing 65.2 grs., appeared to be of the sp. gr. 1.57; it contained 4.7 per cent. of ash. A portion of coke, weighing 18.02 grs., obtained from bituminous coal, appeared to be of the sp. gr. 1.70; it contained 6.8 per cent. of ash. The ash of the anthracite consisted chiefly of silica, with a little alumina, coloured light red by peroxide of iron; the ash of the coke, of silica, with only a trace of alumine and peroxide of iron, of the latter not sufficient to colour it; both were without lime or alkali. The difficulty of extracting the whole of the air from the anthracite, charcoal, and coke, was considerable, especially from the coke. After three days' exposure to the action of the air-pump, the effect was produced on the anthracite; in about the same time on the charcoal; but not in less than eleven on the coke; it floated eight days,—and this notwithstanding that the pump was frequently worked—the mercury in the gauge standing steadily, after the first day, at .25 inch, and although the total quantity of air to be disengaged was equal only to the volume of 4.48 grs. of water. In one instance, the charcoal was boiled in distilled water, after it had ceased to give out air under the exhausted reservoir; but without effect in increasing its specific gravity.

The

they are connected solely with difference of mechanical arrangement. The transparency of the minute portions of charcoal and plumbago, assimilating them to the diamond, may be considered as not unfavourable to this latter view, which is, I believe, the one now commonly adopted, especially taking into account the difference of magnitude between the plates and particles described, possessing the property of translucency, and the atoms, or ultimate particles, the subject of chemical action, and probably of crystalline aggregation. A piece of goldbeater's skin, an inch square, before it was reduced to charcoal, weighed .36 grain, and, when reduced, only .03 grain; and yet a portion of this, not exceeding in size a single red particle of the blood of man, might be seen under the microscope to consist of several parts: a cylindrical piece of the charcoal of the pith of the elder, .4 of an inch long, and .2 of an inch in its transverse diameter, weighed only $\frac{1}{50}$th of a grain, and yet it was composed of a vast number of plates of large dimensions, microscopically considered, one of them being capable of covering more than a hundred of the smaller particles which abound in and seem to form the basis of chyle.

The translucency of charcoal or carbon, in a very finely divided state, and its effect on light, may help to account for the colour of flame and of luminous bodies seen through the medium of smoke, and also for the colour (different tints of brown) exhibited by fluids in which carbonaceous matter is suspended, as when sulphuric acid is heated with alcohol; or charcoal in impalpable powder, such as lamp-black, is mixed with a solution of gum. In the former instance, it is not improbable that the brown colour of the fluid may be owing chiefly to particles of carbon suspended in it, so small as to be invisible even when the eye is aided by the highest power of the microscope. This conjecture is in accordance with the fact, and indeed was suggested by it, that the coloured fluid, which, seen by

The specific gravity of plumbago is stated to range between 1.987 and 2.456. A specimen of the compact kind, from Borrowdale, I found of sp. gr. 2.264, and after the exhaustion of adhering air, by the air-pump, 2.316. A specimen of the foliated kind, I found of the sp. gr. 2.22, and after having been subjected to the action of the air-pump, 2.26. The former yielded, on incineration, 11.48 per cent. of ash, retaining the form of the mass, of a light ochre yellow, which was found to consist chiefly of silica, with a little peroxide of iron, and a very little alumine, with a trace of lime and magnesia. The latter being incinerated with great difficulty, was deflagrated in red hot nitre; it yielded 4 per cent. of ash, which was found to consist chiefly of silica and peroxide of iron, with a little lime and magnesia. The ash was in the form of a powder of fine scales. If the iron and silicon exist in plumbago uncombined with oxygen, their presence may account for the specific gravity of the mineral exceeding that of charcoal and anthracite; but, if combined with oxygen, then it must be admitted that the carbon in plumbago is in a denser state than in charcoal. The circumstance that plumbago is slightly magnetic is in favour of the first idea, and also the fact, as I have ascertained, that it yields air (which it may be presumed is hydrogen) when acted on by dilute sulphuric acid previously purged of air by the air-pump, and after which iron may be detected in solution.

the naked eye, is apparently a perfect solution, under the microscope, with a high power, exhibits particles of carbon, many of them so small as to be barely within the limits of distinct vision, and what these are to a lower power, others may be to the power which brings these into view.

It may be conjectured that other substances, at present considered as opaque, if examined in the same manner as that which I have applied to charcoal and plumbago, may also be found to be translucent. Hitherto, I have succeeded only with one, viz. iodine. When it is viewed in a very finely divided state, as in the smallest crystals in which it can be obtained, and these allowed to be attenuated by evaporation, and so reduced to extreme thinness, it appears by transmitted light of a bright purple colour, very like the colour of iodine in its gaseous state.

EDINBURGH, *December* 21. 1842.

XXIII.—*On the Growth of Grilse and Salmon.* *By* Mr Andrew Young, *Invershin, Sutherlandshire. In a Letter addressed to* James Wilson, *Esq., F.R.S.E. Communicated by* Mr Wilson.

(Read 9th January 1843.)

The history of the habits and development of the salmon has been for ages a subject of dispute, even among men of science and experience. The general theory which prevailed regarding its earlier stage of existence was, that the spawn deposited in the autumn or beginning of winter, produced by development the smolts which were seen descending to the sea in the course of the next ensuing spring.

In regard to its after state, some supposed that the smolts which descended the rivers in spring or early summer returned as grilses that same season. Many doubted this theory, and maintained that it was impossible, or very improbable, that they should return so speedily in that greatly enlarged condition; their only argument, however, being founded on the unlikelihood of such a rapid growth.

Others, again, have maintained the opinion that the grilse is a distinct species of fish, closely allied to, but not identical with, the true salmon, and that in the so-called condition of grilse it has attained to its ultimate state. In fact, this has been asserted not only by naturalists, viewing the subject somewhat vaguely as one with which they had no great acquaintance, but also by practical, that is, professional fishermen, whose opinions, from their more enlarged experience, were supposed to carry greater weight. But till recently all these various opinions were uncertain and unsatisfactory, in so far as they were founded upon supposition, without proof or trustworthy experiment.

But in regard to the first, or, as I may call it, fresh-water state of the fry of salmon, Mr John Shaw of Drumlanrig, several years ago commenced, and some time ago concluded, a series of most sagacious and well known practical experiments, which settled that part of the subject. I lately visited Mr Shaw's experimental ponds, and carefully examined his various specimens, from the ova to the smolts, and I am perfectly convinced of the accuracy of the results, and of the correctness of the general conclusions which he has drawn. This I believe would have been my opinion, judging simply from Mr Shaw's explanations; but I am happy to say (and think it my duty so to do), that my own more reeent experiments, undertaken upon similar principles, though not for such a length of time, have led to the same results. I have as yet gone no further than to a period corresponding to Mr Shaw's first season; but this decidedly confirms Mr Shaw's principal discovery, by confuting the idea entertained by our ancestors, that the fry became smolts during the first spring after the ova were spawned. All my confined specimens are still in the state of parr, exactly as described by Mr Shaw, with this difference, that their dimensions are somewhat greater; and I therefore

anticipate that a larger number will become smolts in about a twelvemonth after their first spring, than was the case with those observed in Dumfriesshire. This may possibly arise from their being with me so much nearer the salt-water, of which I find the influence on salmon, in all its stages, to be most remarkable. I conceive, however, that the question of the true history of this important species, from the egg to the smolt, has been set at rest. To connect that early state with the grilse, and the latter link with the adult salmon, is therefore the object of the present communication. My long continued superintendence and control of some of the finest and most productive salmon fisheries in Scotland, and the peculiar position I occupy, as residing almost within a few yards of the cruives upon the noted river Shin, afford me advantages which I trust I have not altogether neglected.

I have now made many experiments on these fish, taking up the subject where it was necessarily left off by Mr Shaw; and I find, that, notwithstanding the slow growth of the parr in fresh water, such is the influence of the sea, as a more enlarged and salubrious sphere of life, that the very smolts which descend into it from the rivers in spring, ascend into the fresh waters in the course of the immediate summer as grilses, varying in size in proportion to the length of their stay in salt water.

Thus, in the months of April and May of the year 1837, I marked a great number of the descending smolts by making a peculiar perforation in the caudal fin with a pair of small nipping irons constructed for the purpose; and in the months of June and July I caught a considerable number on their return to the rivers, all in the state of grilse, and varying from 3 lb. to 8 lb., according to the time which had elapsed since their first departure from the fresh water, or, in other words, the length of their sojourn in the sea.

Again, in April and May of the present season (1842), I marked a number of descending smolts, by clipping off the dead or adipose fin upon the back, and carefully replacing them in the river. In the course of June and July following, I caught them returning up the river, bearing my peculiar mark, and agreeing with those of 1837, both in respect to size, and the relation which that size bore to the lapse of time. Two of these smolts, as marked last spring, and which I caught again as grilse, I have transmitted to the care of Mr Goodsir, College of Surgeons, in whose hands they may be seen for the satisfaction of the curious or the sceptical.

I have no doubt that many who argue on supposition, not on facts, may ask, how, when salmon from the ovum to the smolt are so slow of growth, their advance from the smolt to the grilse should be so rapid? In regard to this, I can only state the fact as I have repeatedly ascertained it; and it is not the less a fact, although some of the final causes which produce it may be uncertain or obscure. My own opinion is, that it is owing to their change of domicile from

fresh to salt; and in proof of this I may refer to the following fact, that, with the exception of the early state of parr, in which the growth is admitted to be slow, salmon actually never do grow in fresh water at all, either as grilse, or in the adult state. All their growth in these two most important later stages takes place during their sojourn in the sea. Not only is this the case, but I have also ascertained that they actually decrease in dimensions after entering the river, and that the higher they ascend the more they deteriorate, both in weight and quality. In corroboration of this I may refer to the extensive fisheries of the Duke of Sutherland, where the fish of each station of the same river are kept distinct from those of another station, and where we have had ample proof that salmon habitually decrease in weight in proportion to their time and distance from the sea.

I have also instituted another series of experiments for the purpose of shewing the relationship of the grilse and adult salmon, and connecting, as it were, these two final stages with each other. In the spring of 1841 I marked a number of spawned grilses soon after the termination of the spawning season. I took a net and coble, and fished the rivers for the purpose; and all the spawned grilse of 4 lb. weight were marked, by putting a peculiarly twisted piece of wire through the back fin. They were immediately thrown into the river, and of course disappeared, making their way downwards with the other spawned fishes to the sea. In the course of the next summer we again caught several of these fish which we had thus marked with wire as 4 lb. grilse, grown, in the short period of four or five months, into beautiful full-formed salmon, ranging from 9 lb. to 14 lb. in weight, the difference still depending on the length of their sojourn in the sea.

Again, in January 1842, I repeated the same process of marking 4 lb. grilses which had spawned, and were therefore about to seek the sea; but, instead of placing the wire in the back fin, I this year put it in the upper lobe of the tail or caudal fin. On their return from the sea we caught them, salmon as before. Two of these fish, marked as grilse on the 29th of January, and weighing at that time 4 lb., were caught as salmon on the 4th and 14th of July, weighing respectively 8 lb. and 9 lb. They were transmitted, at your request, to the care of Mr Goodsir of the College of Surgeons, for preservation by that gentleman, in whose hands they may now be seen, in confirmation of my statements on the subject of the salmon's growth.*

I may here state, that the motive which first induced me to mark both grilse and salmon was the desire to ascertain whether the fish of a river were a breed peculiar to that river alone; that is, whether the same individuals, after descending to the sea, returned to their original spawning grounds, or whether, as many supposed, the main body returning shorewards from their feeding grounds in distant parts of the ocean, and progressing southwards along the coasts of Scotland, were thrown into, or encouraged to enter, estuaries and rivers by various accidental circumstances, and thus, that the numbers obtained in these estuaries and

* The specimens above referred to are now in the Museum of the Royal Society.

rivers depended mainly on wind and weather being suitable to their upward entrance at the time of their nearing the mouths of the fresher waters.

To settle this point I commenced, in 1836, to catch and mark all the spawned fish I could obtain in the course of the winter months during their sojourn in the rivers. We frequently fished all the pools of a river with net and coble; and, as soon as we drew the fish ashore, we made a peculiar perforation in their tails with the nipping-irons, and threw them back into the water. In the course of the following fishing season we caught great numbers of them after their return from the sea, each in its own river with its distinctive mark.

We have also another proof of the fact, that the different breeds or races of salmon continue to revisit their native streams. You are aware that the river Shin falls into the Oykel at Invershin, and that the conjoined waters of these rivers, with the Carron and other streams, form the estuary of the Oykel, which flows into the more open sea beyond or eastwards of the bar, below the Gizzen Brigs. Now, were the salmon which enter the mouth of the estuary at the bar thrown in merely by accident or chance, we should expect to find the salmon of all the various rivers which form the estuary of the same average weight, for, if it were a mere matter of chance, then a mixture of small and great would occur indifferently in each of the interior streams. But the reverse of this is the case. The salmon in the Shin will average from 17 lb. to 18 lb. in weight, while those of the Oykel scarcely attain an average of half that weight. I am therefore quite satisfied, as well by having marked spawned fish descending to the sea, and caught them ascending the same river, and bearing that river's mark, as by a long continued general observation of the weight, size, and even something of the form, that every river has its own breed, and that that breed continues, till captured and killed, to return from year to year into its native stream.

I may now mention that I commenced marking grilses, with a view to ascertain that they became salmon, so far back as 1837, and have continued to do so ever since, though never two years with the same mark. The result in respect to growth in each of these earlier years corresponded exactly with what I have given in more detail regarding the years 1841 and 1842.

The following lists are extracted from the note-book in which I have carefully recorded the peculiar marks, the periods of marking, the weight at each period, the time of the recapture, and the increased weight.

List of Smolts marked in the River, and recaptured as Grilse, on their first ascent from the Sea.

Period of Marking.	Period of Recapture.	Weight when Retaken.
1842, April and May.	1842, June 28.	4 lb.
... ...	... July 15.	5 lb.
... ...	... ... 15.	5 lb.
... ...	... ... 25.	7 lb.
... ...	... ... 25.	5 lb.
... ...	... ... 30.	$3\frac{1}{2}$ lb.

List of Grilse marked after having Spawned, and recaptured as Salmon, on their second ascent from the Sea.

Period of Marking.	Period of Recapture.	Weight when Marked.	Weight when Retaken.
1841, February 18.	1841, June 23.	4 lb.	9 lb.
... ... 18.	... ... 25.	4 lb.	11 lb.
... ... 18.	... ... 25.	4 lb.	9 lb.
... ... 18.	... ... 25.	4 lb.	10 lb.
... ... 18.	... July 27.	4 lb.	13 lb.
... ... 18.	... ... 28.	4 lb.	10 lb.
... March 4.	... ... 1.	4 lb.	12 lb.
... ... 4.	... ... 1.	4 lb.	14 lb.
... ... 4.	... ... 27.	4 lb.	12 lb.
1842, January 29.	1842, July 4.	4 lb.	8 lb.
... ... 29.	... ... 14.	4 lb.	9 lb.
... ... 29.	... ... 14.	4 lb.	8 lb.
... March 8.	... ... 23.	4 lb.	9 lb.
... January 29.	... ... 29.	4 lb.	11 lb.
... March 8.	... Aug. 4.	4 lb.	10 lb.
... January 29.	... ... 11.	4 lb.	12 lb.

In the year 1841, we marked a few spawned salmon along with the grilse. We marked both kinds in the back fin, but the salmon with copper wire, and the grilse with brass wire. I perceive by my lists, that a salmon marked on the 4th of March, returned and was captured on the 10th of July, with an increase of 6 lb., having grown in that time from 12 to 18 lb. In 1842, however, we marked no salmon; all were grilse of 4 lb., and were marked in the caudal fin. In the course of both seasons we caught far more marked grilse returning with the form and attributes of perfect salmon than are recorded in my lists. In many specimens the wires had been torn from the fins, either by the action of the nets or other casualties; and although I could myself recognise distinctly that they were the fish I had marked, I kept no note of them. All those recorded in my lists returned, and were captured with the twisted wires complete,—the same as the specimens transmitted for your examination.

I think that the preceding facts, viewed in connection with Mr Shaw's observations, entitle us to say that we are now well acquainted with the history and habits of the salmon, and its rate of growth from the ovum to the adult state. The young are hatched during a period which admits of considerable range, according to the temperature of the season, and the character of special places. Their growth in the early state of parr is extremely slow, and the silvery aspect of the smolt is not assumed till at least the lapse of a year from the time of hatching. The great mass of smolts descend to the sea during the months of April and May, but not the whole, because the varying range of the spawning season carries with it a somewhat corresponding range in the assumption of the

first change, and the migration of the smolts towards the sea. They return as grilse during the summer and autumn of the same season, their increase of size depending on the length of time which has elapsed between their departure from and return towards the river water. Such of these grilse as escape the snares of the fishermen and other enemies, spawn that same season in common with the salmon, and after spawning, both the one and the other redescend into the sea in the course of the winter or ensuing spring. They all return again to the rivers sooner or later, according as they had previously left it, after having spawned, early or late. The grilse have now become salmon by the time of their second ascent from the sea. All these changes and conditions I have verified by special experiment, as well as by general observation, from the egg to the smolt (the parr inclusive), from the smolt to the grilse, from the grilse to the salmon; and I am perfectly satisfied with the accuracy of the conclusions come to, and which I have here endeavoured to explain.

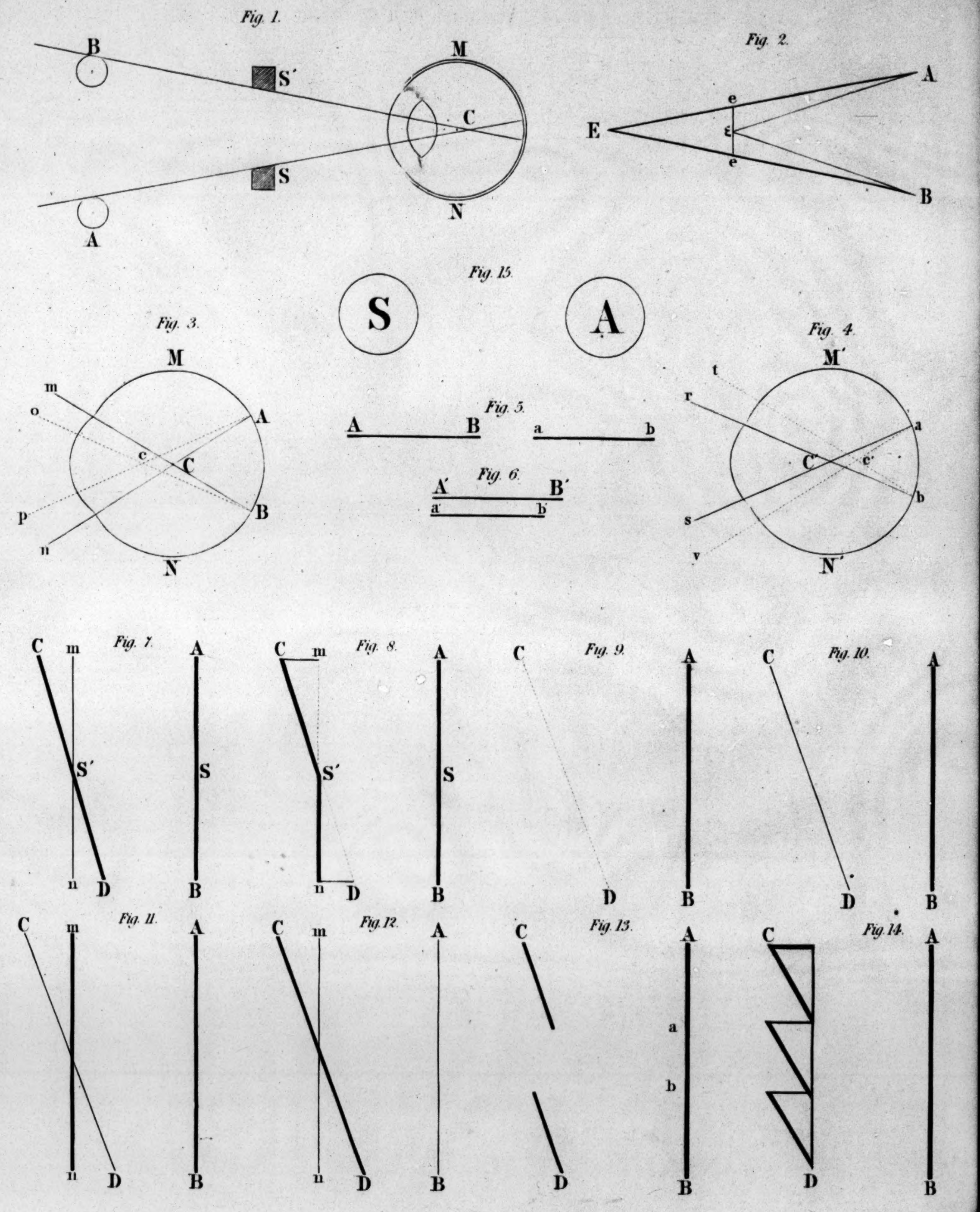
Fig. 1.
B
S′
M
C
S
A
N
Fig. 2.
A
e
E
ε
e
B
Fig. 15.
S
A
Fig. 3.
M
m
o
A
c
C
p
B
n
N
Fig. 4.
M′
t
r
a
C′
c′
b
s
v
N′
Fig. 5.
A
B
a
b
Fig. 6.
A′
B′
a′
b′
Fig. 7.
C
m
A
S′
S
n
D
B
Fig. 8.
C
m
A
S′
S
n
D
B
Fig. 9.
C
A
D
B
Fig. 10.
C
A
D
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Fig. 11.
C
m
A
n
D
B
Fig. 12.
C
m
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Fig. 13.
C
A
a
b
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Fig. 14.
C
A
D
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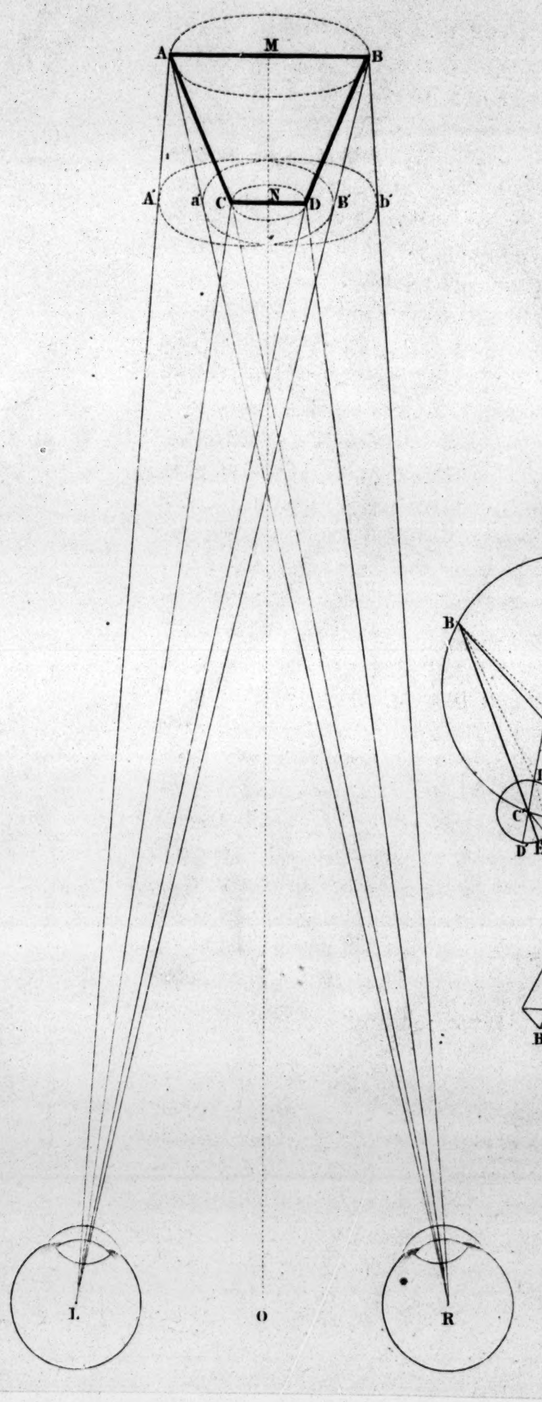
Fig. 16.



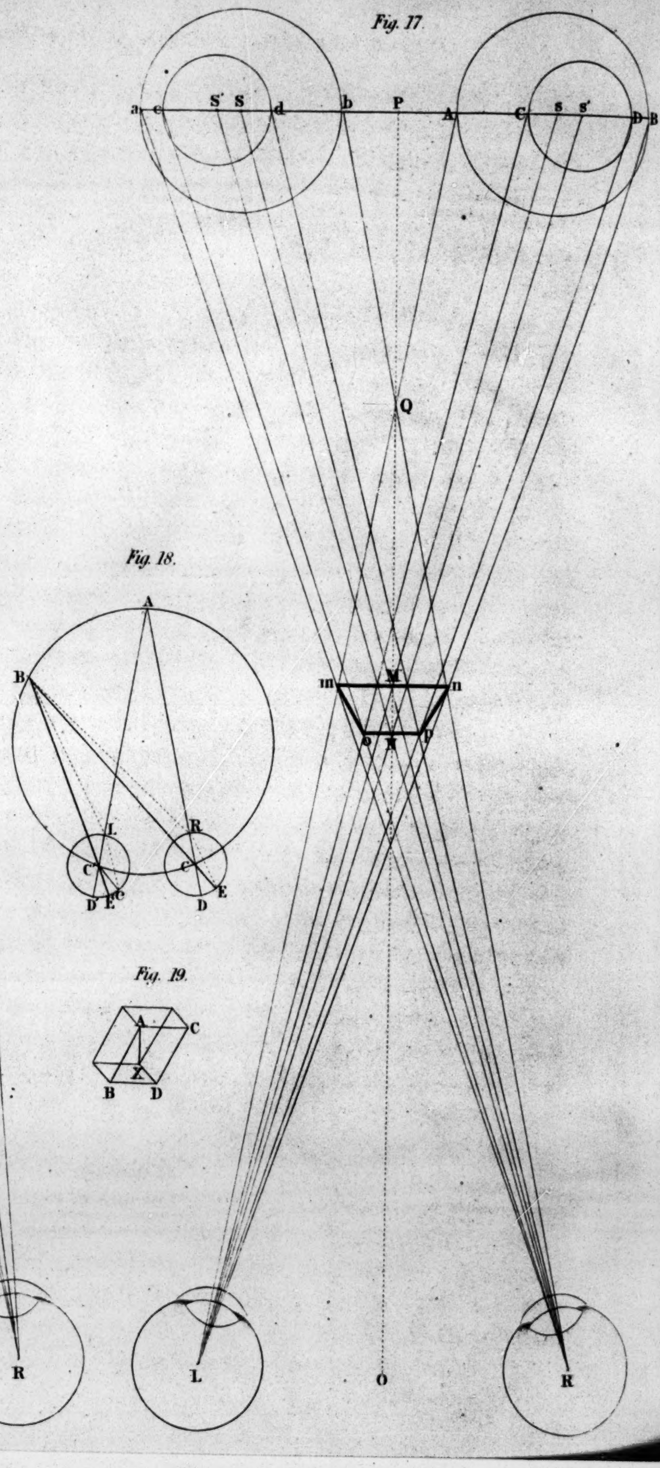


Fig. 17.

Fig. 18.

Fig. 19.

XXIV.—*On the Law of Visible Position in Single and Binocular Vision, and on the representation of Solid Figures by the union of dissimilar Plane Pictures on the Retina.* *By* SIR DAVID BREWSTER, *K. H. D. C. L., F. R. S., and V. P. R. S. Edin.*

(Read 23d Jan. and 6th Feb. 1843.)

IN the course of an examination of Bishop BERKELEY'S "New Theory of Vision," the foundation of the Ideal Philosophy, I have found it necessary to repeat many old experiments, and to make many new ones, in reference to the functions of the eye as an optical instrument. I had imagined that many points in the physiology of vision were irrevocably fixed, and placed beyond the reach of controversy; but though this supposition may still be true in the estimation of that very limited class of philosophers who have really studied the subject, yet it is mortifying to find that the laws of vision, as established by experiment and observation, are as little understood as they were in the days of LOCKE and BERKELEY. Metaphysicians and physiologists have combined their efforts in substituting unfounded speculation for physical truth; and even substantial discoveries have been prematurely placed in opposition to opinions of which they are the necessary result.

In prosecuting this subject, my attention has been particularly fixed upon the interesting paper of my distinguished friend Professor WHEATSTONE, "*On some remarkable and hitherto unobserved phenomena of binocular vision.*" * It is impossible to over-estimate the importance of this paper, or to admire too highly the value and beauty of the leading discovery which it describes, namely, the perception of an object of three dimensions by the union of the two dissimilar pictures formed on the retinæ:—but, in seeking an explanation of this curious phenomenon, and in applying it to explain phenomena previously known, Mr WHEATSTONE has adduced experimental results, and drawn conclusions which stand in direct opposition to what was best established in our previous knowledge. Before entering, however, upon this branch of the subject, I must first explain the law of visible direction, and the phenomena of ocular parallaxes.

1. *On the Law of Visible Direction in Monocular Vision.*

Several philosophers had hazarded the opinion, that every external visible point is seen in the direction of a line passing from its picture on the retina through the centre of the eye considered as a sphere; while others maintained that every such point was seen in the direction of the refracted ray by which its image was formed.

* Phil. Trans. 1838, p. 371.

The celebrated D'ALEMBERT, in his *Doutes sur differents questions d'Optique*, maintains that the action of light upon the retina is conformable to the laws of mechanics; and he adds, that it is difficult to conceive how an object could be seen in any other direction than that of a line perpendicular to the curvature of the retina at the point where it is really excited. He then investigates, mathematically, how the apparent magnitudes of objects would be affected, on the two suppositions that the line of visible direction coincides either with the refracted ray, or with a line perpendicular to the retina at the point of excitement. On the *first* of these suppositions, he finds that the apparent magnitude of small objects would be increased about $\frac{1}{13}$th, and on the *second* supposition, a little more than $\frac{1}{3}$, or $\frac{6823}{18267}$. This last result is, as D'ALEMBERT justly remarks, so contrary to experience, that we cannot suppose vision to be thus performed, however natural the supposition may appear. "In the direction of what line, then," he adds, "do we perceive objects, or visible points, which are not placed in the optical axis? This is a point which it appears very difficult to determine exactly and rigorously. As experience, however, proves that objects of small extent, which are within the range of our eyes, do not appear sensibly greater than they are in reality, it follows, that the visible point which sends a ray to the cornea, is seen sensibly in its place, and consequently in the direction of a line joining the point itself and its image on the retina. But why," D'ALEMBERT adds, "is this the case? It is a fact which I will not undertake to explain."*

When we consider the data from which D'ALEMBERT has deduced the preceding results, it is not easy to account for his having abandoned the inquiry as a hopeless one. He employs the dimensions of the eye as given by PETIT and JURIN, and he assumes JURIN's index of refraction for the human crystalline lens, though it is almost exactly the same as that of an ox, as given by HAWKSBEE. These, indeed, were the best data he could procure; but he should have inquired if the most probable law of visible direction was compatible with any other dimensions of the eye, and any other refractive powers of the humours which were within the limits of probability; and above all, he ought to have examined experimentally the truth of his fundamental assumption, *that visible points are really seen in their true places when they are not in the axis of vision.*

Now it is quite certain that these points are not seen in their true direction, and that there is an *ocular parallax*, which is the measure of the deviation of the *visible* from the *true direction* of objects. This parallax is nothing in the axis of the eye, and it increases as the visible point is more and more distant from that axis; and hence it follows, that, during the motion of the eyeball, when the head is immoveable, visible objects not only change their place, but also their form.

Had the eye consisted of only *two* concentric coats, a *cornea* and a *retina*, filled with a homogeneous fluid, vision would have been performed by centrical

* *Opuscules Mathematiques*, Tom. I. Mem. ix. p. 266.

pencils;—the visible and the true direction of points would have coincided, and objects would have changed neither their form nor their position during the motion of this hypothetical eyeball round the common centre of the two coats. But as such an eye could not have afforded sufficiently distinct vision, the introduction of the crystalline lens became necessary; and it is owing to the secondary refractions at its surfaces and within its mass of variable density, that the parallax of visible direction is produced.

The following experiment will establish the existence, and explain the nature of this parallax. Let MN, Fig. 1, be the eyeball, C the centre of curvature of the retina, and also the centre of motion of the eyeball. Having placed an opaque screen S several inches from the eye, till its inner edge just eclipses a luminous object A, look away from the screen, and the object A will appear. Keeping the head steady, place another screen S′* so that, when viewed directly, it does not eclipse another luminous object B, the line CS′B just grazing the outer edge of B. When the screens and luminous objects, therefore, are so arranged that A is *invisible* when the axis of the eye is directed to S or to A, and B visible when the axis of the eye is directed to S′ or B,—then by turning the eye from A to B, A will *appear*, and B will disappear, exhibiting the curious effect *of an invisible body appearing by looking away from it, and of a visible body disappearing by looking at it!*

Had the eyeball MN been our hypothetical one, these effects would not have been produced. All objects, near and remote, would have retained their relative positions and magnitudes during its rotation.

Hence it follows, that we are not entitled to reject any law of visible direction, because it gives a position to visible objects different from their real position.

Having removed this difficulty, I proceeded to examine the other data of D'Alembert. Making the eyeball and the retina spherical, he assumes that the centre of the latter is equidistant from the *foramen centrale* of the retina, and the centre of the crystalline lens. This, however, is far from being the case. M. Dutour, and Dr Thomas Young, have made the centre of curvature of the retina coincident with the centre of curvature of the spherical surface of the cornea, as in our hypothetical eye; and this centre, in place of being almost half-way between the apex of the posterior surface of the lens and the *foramen centrale*, is actually almost in contact with the latter! The dissections of Dr Knox and Mr Clay Wallace, of New York, give similar results. When we add to these considerations the fact that the refractive power of the crystalline lens assumed by D'Alembert is nearly *triple* of what it really is, we are entitled to reject the results of his calculations.

Assuming, then, the most correct anatomy of the eye, namely, that according

* The two screens S, S′ may be the opposite edges fo a triangular notch in a card held in the hand.

to which the cornea and the retina are concentric, it is obvious that if there was no crystalline lens, pencils, incident perpendicularly on the cornea, would pass through the common centre, and fall perpendicularly upon the retina. Hence, in this case, the line of *visible* direction would coincide with the line of *real* direction, and also with the incident and intromitted ray. Now, the refractions at the crystalline are exceedingly small, and, at moderate inclinations to the axis, the deviations from the preceding law are very minute. At an inclination of 25° or 30°, a line perpendicular to the point of impression on the retina passes through the common centre already referred to, and does not deviate from the line of real visible direction more than half a degree, a quantity too small to interfere with the purposes of vision. The deviation, of course, increases with the inclination; but as there is no such thing as distinct vision out of the axis, and as the indistinctness increases with the inclination, it is impossible to ascertain, by ordinary observation, that any deviation exists. Hence the mechanical principle of D'ALEMBERT, which he himself has rejected, and the law of visible direction, which I have established, are substantially true. As the Almighty has not made the eye achromatic, because it was unnecessary, so He has, in the same wise economy of His power, not given it the property of seeing visible points in their real direction.

Had it been necessary to make the visible ray coincident in direction with the incident ray, it might have been effected by giving such a form and variable density to the crystalline lens as to make the ray which it refracted cross the axis of vision at the centre of curvature of the retina; and if the crystalline lens were such that this crossing point was variable, this variation might have been compensated by making the retina spheroidal, with a variable centre of curvature.

That a visible point is seen in the direction of a line perpendicular to the surface of the retina at which the image of the point is formed, may be established experimentally in the following manner. Having expanded the pupil by belladonna, look directly at a point in the axis of the eye. Its image will be formed by a cone of rays variously inclined from 85° to 90° to the surface of the retina. While the point is distinctly seen, intercept all these different rays in succession, and it will be found that each ray gives vision in the same direction, the visible point retaining its position. Hence it follows, that on the part of the retina in the axis of vision, all rays, however obliquely incident, give the same visible direction perpendicular to the surface of the membrane. That the same property is possessed by every other part of the retina cannot be doubted, and may be proved by direct experiment.

Although D'ALEMBERT states it as unquestionable, that when the visual ray is in the axis of vision, or the optic axis, and passes to the retina without refraction, the point which emits it will be seen in the direction of a line passing from its image to the visible point; yet, after he has found that his mechanical prin-

ciple is not correct, he gives loose reins to his scepticism, and maintains the extraordinary paradox, that objects even which are placed in the optical axis are not always seen in this axis. The following is the argument he employs, which I shall give in his own words.

"If we direct the two optic axes AE, BE, Fig. 2, towards a star E, it is certain that this star appears much nearer to us than it really is: It is true that we estimate its distance only in a very imperfect and vague manner; but it is not less certain that this distance perceived, whether apparent or presumed, is greatly *below* the real distance. If, then, we see the star in each of the optical axes AE, BE, we should see it in each of these axes in the points *e*, *e*, which are incomparably nearer A and B than E. Thus we should see two stars *e*, *e*, and their apparent distance *e e* would be nearly equal to AB. Observation, however, proves that we see only *one* star, and this star is seen nearly at the middle point ϵ of the line *e e* in the direction of lines A ϵ, B ϵ, different from the optic axes. It is true that these lines, though really different from the optic axes, deviate from them but very little, but still they do differ from them; and this experiment is sufficient to prove *that objects which are at a considerable distance from the eye are not seen exactly in the optical axis, even when we look at them directly.*

"Whence, in general, nothing is less certain than this common principle in optics, that *objects are seen in the direction of the ray which they send to the eye.*"*

It is almost impossible to believe that D'Alembert is serious in maintaining these doctrines. The major proposition of his syllogism is absolutely incorrect. It is not true that we see the star E nearer than it is. The eye does not see distances directly: the mind only estimates them, and, according to its means of judging, it forms a right or a wrong opinion. The second proposition is equally incorrect. We do not see the star along the lines A ϵ, B ϵ. We see it along the lines AE, BE, at the very place where it is, and whether we consider it nearer or more remote than it is,—whether we think that it touches our eye, or exists at the remotest verge of space,—the position of the optical axis of each eye remains as before, and our vision of the star is not affected by the truth or falsehood of our judgment.

2. *On the Law of Visible Direction in Binocular Vision.*

In admitting the correctness of the law of visible direction in monocular vision, which I have endeavoured to establish in the preceding section, Professor Wheatstone justly remarks, "that the result of any attempt to explain the single appearance of objects to both eyes, or, in other words, *the law of visible direction for binocular vision*, ought to contain nothing inconsistent with the law of visible di-

* *Opuscules Mathematiques*, Tom. I. Mem. ix. § iv. p. 273–4.

rection for monocular vision."* Properly speaking, however, there is no such thing as a law of visible direction in binocular vision, because there is no such thing as *a centre* of visible direction, or *a line* of visible direction in binocular vision. When we see an object distinctly with both eyes, it is actually seen in *two* directions, and the point where these directions intersect each other determines the visible place of the object. But if we follow Mr WHEATSTONE in considering such a law as equivalent to the law which regulates "the single appearance of objects to both eyes," we can readily deduce it as a corollary from the law in monocular vision. A visible point is seen single with two eyes only when it is at the intersection of its lines of visible direction as given by each eye separately. It is obvious that this law does not harmonize with the doctrine of corresponding points, or with the binocular circle of the German physiologists. It is, however, rigorously true; for no philosopher can adopt the monstrous opinion that the functions and laws of vision which belong to each eye, acting separately, are subverted when they act in concert. Hence it is obvious that the single vision of points with *two* eyes, or with *two hundred* eyes, is the necessary consequence of the convergency of the *two*, or the *two hundred*, lines of visible direction to the same point in absolute space; and although we think that objects appear single with both eyes, yet it is only the points to which the optic axes and the lines of visible direction converge that are actually seen single, and the unity of the perception is obtained by the rapid survey which the eye takes of every part of the object.

The phenomenon of an erect object from an inverted picture on the retina, which has so unnecessarily perplexed metaphysicians and physiologists, is a demonstrable corollary from the law of visible direction for points. The only difficulty which I have ever experienced in studying this subject, has been to discover where any difficulty lay. An able writer, however, in a recent number of *Blackwood's Magazine*,† in discussing the BERKLEYAN theory of vision, has started a difficulty of a very novel kind, and has called upon me personally to solve it. Were this the proper place for such a discussion, I should willingly enter upon it; but I must content myself with stating, that the doctrine which the very ingenious author calls the *ordinary optical doctrine*, was never maintained by any optical writer whatever, and that the doctrine which he substitutes in its place is that which all optical writers implicitly adopt, though they have thought it too elementary to require illustration. A visible point which throws out *two* separate particles of light, an *upper* and an *under*, will be inverted on the retina, but a *smaller* visible point, which throws out only *one* particle of light, cannot be *inverted*, because *inversion* implies a change in the relative position of *two* visible points.

* Phil. Trans. 1838, p. 388. † June 1842, vol. li. p. 830.

3. *On the Vision of Objects of Three Dimensions.*

(1.) *By Monocular Vision.*—If we look with one eye at a solid body, for example a six-sided pyramid with its apex directed to the eye, and uniformly illuminated, we recognise at a single glance that it is not a drawing of the pyramid. When the eye adjusts itself to distinct vision of its apex, all the more distant parts are seen indistinctly, but the eye quickly surveys the whole, adjusting itself to distinct vision of its base and of its edges, and by these successive efforts, at one time contracting the pupil and the eyebrows to see the near parts, and expanding them to see the more remote ones, it obtains a knowledge of the relative distance of its different parts. The vision of the pyramid thus obtained is nearly perfect. There is no inequality of illumination produced by the act of single vision; and there is no flickering in the outlines of the figure. The only apparent imperfection is, that when we see one point very distinctly we do not see the other parts with equal distinctness; but this imperfection is unavoidable in vision, whether with one or two eyes; and, in place of being a defect, is the very means by which we judge of the relative distance of its parts. If we saw all its lines and parts with equal distinctness, without moving the eyeball, or without altering the mechanism for its adjustment, we should not have been able to distinguish the pyramid from its projection upon a plane surface.

Hence we draw the conclusion that the vision of bodies of three dimensions with one eye is perfect.

(2.) *By Binocular Vision.*—If we now place the pyramid before both eyes, so that the pictures of it on each retina are nearly similar, the one being the reflected image of the other, we shall see the pyramid with great distinctness. It will appear more luminous with the two eyes, and if the observer wished to estimate the distance of its apex, or any other point of it, from himself, the convergency of *both* eyes to that point would enable him to form a more correct judgment than with a single eye. These, doubtless, are advantages, but they do not in the least degree improve our vision of the pyramid, which is independent of them. More light may injure vision as well as improve it; and if we could project a foot-rule from each eye, and read upon it the distance of every part of the pyramid, the vision of it would not in the slightest degree be affected. May we not add also, that the intromission of scattered light through *two* eyes in place of one, and the possible dissimilarity, however small, between the curvatures and densities of their humours, which would give rise to two pictures of different magnitudes, would entitle us to give the preference to single vision, in reference to its power of giving us a distinct view of objects of three dimensions.

Hence, we conclude, that when the pyramid is placed in a position of symmetry between the two eyes, *binocular* is not superior to *monocular vision.*

But if the pyramid is so placed that the left eye sees only *four* faces of it, while the right eye sees all the *six*, then the *monocular* vision of the pyramid is more distinct than the *binocular* one. The vision of faces 1, 2, 3, and 4 is sufficiently distinct with two eyes; but the faces 5, 6, being seen only with one eye, are less luminous than the other faces, and as the optic axes do not perform their functions with the same accuracy when the object to which they are directed is visible only to one eye, the part of the object seen by single vision will not unite with that seen by double vision; and, in the case of the pyramid, we shall observe its apex actually projecting upon the faces 5, 6, of the pyramid, and destroying the symmetry of the picture. When all the faces but No. 6 are seen by the left eye, vision is still unsatisfactory with both eyes, and yet more so when only *three* of the faces are seen by the left eye.

Hence we conclude that, in these cases, *binocular* is *inferior* to *monocular* vision.

Let us next suppose that the object viewed is a table knife, so placed that, when the back of it is towards the observer, the *left* side of the blade is seen by the *left* eye, and the *right* side of the blade by the *right* eye. As the back is seen by both eyes, the picture presented to the mind is a compound of one double and two single sensations, and, consequently, a very unsatisfactory representation of the object.

Hence we conclude that, in this case, *binocular* is still more inferior to *monocular* vision.

These results stand in direct opposition to those given by Professor WHEATSTONE, who considers it an established fact, "*that the most vivid belief of the solidity of an object of three dimensions arises from two different perspective projections of it being simultaneously presented to the mind.*" Before entering, however, upon this branch of the subject, I must examine Mr WHEATSTONE's views respecting the *binocular* vision of figures of different magnitudes.

4. *On the Binocular Vision of Figures of Different Magnitudes.*

Mr WHEATSTONE seems to have been the first person who made experiments on the binocular vision of unequal figures. Having drawn on separate pieces of paper "two squares or circles, differing obviously, but not extravagantly, in size;" he placed them in the stereoscope, and concluded from his observations that the *two unequal pictures* "coalesced, and occasioned a single resultant perception;" and that the binocular image thus perceived was apparently intermediate

in size between the two unequal monocular ones. This perfect coalescence of the two images he considers as demonstrated, and he deduces from it the important conclusion, that, if it were otherwise, "objects would appear single only when the optic axes converge immediately forwards." That is, we see objects single when the optic axes converge laterally in virtue of the coincidence of two unequal images.

These extraordinary results are obviously subversive of the established laws of vision, but especially of the law of visible direction; and if they are true, they must arise from a sudden change in the properties of the humours, or in the functions of the retina. The lesser image may become greater, or the greater less, by a variation in the refractive density or the form of the cornea and the crystalline lens, or, what would be more probable, the retina may become subject to a new law of visible direction. Assuming this to be the case, we must suppose the change of law to take place in each eye, so that the *larger* image must be seen *less*, and the *smaller* image seen *greater*, than they really are. Now, this change must take place instantaneously at the moment of coalescence, for the two images retain their proper magnitude till their apparent union takes place; and the eye must recover its ordinary functions as instantaneously, for the moment we intercept one of the images the other resumes its proper size.

In order to understand what the nature of this supposed change actually is, let MN, M′N′ (fig. 3, 4) be the two eyes, AB the larger image, and *a b* the smaller one; then if C be the centre of curvature of the retina, the points A,B will be seen in the directions, A*n*, B*m* intersecting at C, and the points *a*, *b* in the directions *as*, *br* intersecting at C′. But when these separate images coalesce, in consequence of AB becoming less and *b c* greater, the points A,B, *a*, *b* must be seen in the directions A*n*, B*o*, *av*, *bt*, intersecting at new centres of visible direction *c*, *c*′, the one farther from, and the other nearer to, the retina. If we now shift the *larger* picture to the *right* eye, and the smaller to the *left* eye, the function of the retina will be again changed: the *left* eye MN will have its lines of visible direction as in fig. 4, and the *right* eye as in fig. 3. Such an oscillation of the *binocular centre* of visible direction on each side of the *monocular centre*, produced solely during the attempt to unite unequal images, would indicate a function of the retina so extraordinary, that the most incontrovertible experiments, and the universal experience of accurate observers, could alone give it credibility.

There is no doubt that the two unequal images appear to coalesce; but if we make the outlines of the squares and circles luminous, by pricking small holes in their outlines, and exposing them to very strong light, we shall find it impossible to produce a coincidence. The best way to make this experiment is to take two lines, AB, *a b*, fig. 5, of unequal lengths, and with a large pin to perforate the lines at A,B, *a*, *b*, so that when we attempt to unite them, as at fig. 6, we shall see with perfect distinctness their four luminous extremities. When the point *a*

is made to pass into A, I have never succeeded in making *b* pass into B. Whenever there is an appearance of this, either turn round the paper, or the head, so as to separate the lines as in fig. 6, and it will be invariably seen that if *a* springs out of A, *b* will spring out of a point between A and B. The apparent coincidence, therefore, of AB with *a b*, fig. 6, when it is seen, arises from the disappearance of one or other of the extremities of the two lines.

But Mr WHEATSTONE has described another very interesting experiment, of the same character as that which we have been examining, and he regards it as "proving that similar pictures, falling on corresponding points of the two retinæ, may appear double and in different places." Draw a strong vertical line, AB, fig. 7. and another CD inclined some degrees to it, and also a faint line *m n* parallel to AB, and cutting CD at its centre S, then, according to Mr WHEATSTONE, the two strong lines AB, CD, when seen with different eyes in the stereoscope, or brought together by looking at a nearer object, "will coincide, and the resultant perspective line (CD) will appear to occupy the same place as before; but the faint line (*m n*) which now falls on a line of the left retina, which corresponds with the line of the right retina, on which one of the coinciding strong lines, viz., the vertical one (AB) falls, appears in a different place." In repeating this experiment, I have occasionally observed an apparent coincidence similar to that which is described in the preceding passage; but after numerous and varied observations, made with lines *coloured* and *uncoloured*, *opaque* and *transparent*, *similar* and *dissimilar* both in strength and form, I have no hesitation in affirming that the phenomenon described by Mr WHEATSTONE is an illusion, arising from the actual disappearance of one or more parts, or even of the whole of one of the lines, and from the difficulty of observing the separation or superposition of images in the circumstances under which the experiment is made.

The following are a few of the variations of the experiment which I have found the best calculated to exhibit the real place of the combined images.

1. In Mr WHEATSTONE's form of the lines shewn in fig. 7, the *strong* line A B assumes more readily the appearance of uniting with the *similarly strong line* C D; but if *m n* is a *strong* line and CD a *weak* one (fig. 11), or an *interrupted* one, AB will unite with *m n*, and not even apparently with CD. In like manner, if AB be a *weak* line, it will unite with the *weak* line *m n* rather than with CD. (See fig. 12.)

Now, the apparent coalescence of similar lines arises from the fact, that when corresponding, or nearly corresponding, parts of the retinæ are impressed with similar images, one of the two more readily vanishes, independent of its liability to vanish from its being out of the axis of vision. Whenever two images interfere with one another so as to impede vision, one of them disappears—or rather, is not taken cognisance of by the eye. Hence it is, that many sportsmen shoot

with both eyes open; and hence it is that, in very oblique vision, one of the eyes resigns its office, and leaves the other to view the object distinctly and singly.*

But, in point of fact, AB, fig. 7, does not coalesce with CD. If the eye strives to see distinctly any object at the point S′, then AB coalesces with *m n*. If the eye looks fixedly at C when A is united with C, AS′ will unite with CS′ and S′B with S′*n*; and if the eye is fixed intently on D when B and D are united, S′B will coalesce with S′D and AS with *m* S′. In these two last cases, the coalescence arises from the same cause as the coalescence of dissimilar forms in Mr WHEATSTONE's fundamental experiment, as I shall now shew.

2. If we join C *m*, D *n*, fig. 7 (as is done in fig. 8), we may regard AB and C *m* S′ *n* D as dissimilar images of a solid, consisting of two triangles C *m* S′, D *n* S′, united at their apex. In this case, AS, fig. 8, will coincide with CS′ and S′B with S *n*. If the two dissimilar images are, as in figs. 9, 10, 11, and 12, AB will not appear to coalesce with CD. In fig. 13, the coalescence is not complete; but it becomes so by removing the portion *a b* of the line AB the part A *a* coalescing with C, and *b* B with D. In fig. 14, the line AB will not coalesce with CD; but each separate portion of AB will, when the other two portions are concealed or removed, coalesce with the corresponding portion of CD.

The *ocular equivocation*, as it may be called, which is produced by the capricious disappearance and reappearance of images formed on nearly corresponding parts of the retina of each eye, is placed beyond a doubt by Mr WHEATSTONE's own experiments.† Having inscribed the letters A, S, fig. 15, in two equal circles, he unites the circles, and finds, that, while the common border remains constant, "the letter within it will change alternately" from A to S. At the instant of change, the letter "breaks into fragments; while fragments of the letter which is about to appear, mingle with them, and are immediately replaced by the entire letter." I have long ago‡ described an affection of the retina, of an analogous kind, which illustrates the subject under consideration. "If we look very steadily and continuously with both eyes at a double pattern —such as one of those on a carpet—composed of two single patterns of different colours, suppose *red* and *green;* and if we direct the mind particularly to the contemplation of the *red* one, the *green* pattern will sometimes vanish entirely, leaving the *red* one alone visible: and, by the same process, the *red* one may be made to disappear." When we join to these various facts the remarkable phenomena of the disappearance of objects seen out of the axis of vision by one or by both

* The fact of objects seen obliquely not being double, is ascribed by Mr WHEATSTONE to the coalescence of the images of different magnitudes given by each eye.

† *Phil. Trans.* 1838, p.386, § 14.

‡ *Letters on Natural Magic*, p. 54.

eyes,* we shall find it difficult to believe that two similar unequal figures can coalesce; or that "similar pictures, falling upon corresponding points of the two retinæ, may appear double, and in different places."

5. *On the Cause of the Perception of Objects in Relief by the Coalescence of Dissimilar Pictures.*

Mr WHEATSTONE concludes his interesting paper with an inquiry into the cause "*why* two dissimilar pictures, projected on the two retinæ, give rise to the perception of an object in relief." "I will not attempt," he adds, "at present, to give the complete solution of this question, which is far from being so easy as at a first glance it may appear to be, and is, indeed, one of great complexity. I shall, in this place, merely consider the most obvious explanations which might be offered, and shew their insufficiency to explain the whole of the phenomena."

Mr WHEATSTONE then proceeds to describe the process of vision in the same manner as we have done in § 3; but impressed with the conviction that his previous results are correct, he adds, "All this is *in some degree true*; but were it *entirely so*, no appearance of relief should present itself when the eyes remain intently fixed on one point of a binocular image in the stereoscope." He then gives the following experiment as decisive on the subject:—"Draw two lines, about two inches long, and inclined towards each other as in fig. 7, on a sheet of paper; and having caused them to coincide by converging the optic axes to a point nearer than the paper, look intently on the upper end of the resultant line, without allowing the eyes to wander from it for a moment. The *entire line will appear* single, and in its proper relief," &c. After making this experiment with the greatest care, we admit that it may *appear* single, without being single. To us it does not appear single, but exactly the same as a line having the same length and the same position appears in ordinary vision. Now, though this latter line appears single to most eyes, yet it is certain that every point of it is double and indistinct, excepting the point on which the attention is fixed, and to which the optic axes converge. The vision of objects in relief from the union of dissimilar pictures, is performed by the very same process as the vision of real objects in relief by the ordinary agency of our two eyes; and in establishing this principle, the true cause of the phenomenon discovered by Mr WHEATSTONE will be readily obtained.

Mr WHEATSTONE considers it as experimentally established, "that the most vivid belief of the solidity of an object of three dimensions arises from two different perspective projections of it being simultaneously presented to the mind;" and that "the simultaneous vision of two dissimilar pictures suggests the relief of

* See *Letters on Natural Magic*. Lett. III., p. 54.

objects in the most vivid manner." Having already explained, in § 3, the true process by which solid bodies are seen in relief, I shall now endeavour to shew, that, in the vivid relief produced by the union of two dissimilar plane pictures, this union is merely a necessary accessory, and not the cause of the phenomenon in question.

When two of the images of two perfectly similar objects are united either by looking at a nearer or a remote object, the compound image, thus formed, is seen at the place where the two optic axes converge, and is larger and more remote than the single image if we look at a more distant object, and smaller and nearer if we look at a nearer object.* The best mode of conducting this class of experiments is to suspend two equal rings by invisible fibres, or to cement them upon a large plate of glass, whose surface and figure are not visible to the observer. The object of this arrangement is to prevent the observer from having any knowledge of their distance from the eye. When the rings, thus placed, are doubled, interpose an aperture, so as to permit only the united rings to be seen; and it will be found that they appear at the place to which the optic axes converge, appearing smaller and nearer, or larger and more remote, according as the optic axes are converged to a point nearer or more distant than the actual rings. In both these cases, the similar rings are seen in identically the same manner, having the same apparent magnitude and position as if a similar real ring were placed as an object at the spot to which the optic axes converge. Let us now apply these facts to the vision of the apparent solid produced *in consequence* of the union of two dissimilar plane pictures of it. For this purpose, I shall take the case of the frustum of a cone, after having considered the process by which we see a real frustum of a cone by both eyes—the nature of the compound picture which we do see—and the cause of the apparent single picture of which the mind takes cognizance.

When we look at the real frustum of a cone (ABCD, placed as in fig. 16), the right eye R sees a solid, whose projection is $a'b'$CD, or $abcd$, fig, 17; and the left eye to a solid, whose projection is A′B′CD, or ABCD, fig. 17. The smaller circle CD appears nearer to the observer than the base AB, because the eye cannot see it distinctly without adjusting itself to the distance RC, LD, and converging its optic axes to that distance. Each eye, acting alone, sees the cone single, and the various points of its outline are seen more or less distinct, according as they are more or less remote from the point to which the eye is for the instant adjusted. But so rapid is the motion of the eye, and so quickly does it survey the whole of the solid, that it obtains a most distinct perception of its form, its surface, and its

* Several curious facts establishing this result have been given by Dr SMITH in his *Compleat System of Optics*, vol. ii. 387–389; and *Remarks*, § 526–527.

solidity. When we view the cone with *both eyes*, we have the same indistinctness of outline when the optic axes are converged to a single point: but in addition to this, we have the greater indistinctness arising from every point of the figure being seen *double*, except the single point to which the axes are converged. But this imperfection, too, is scarcely visible, from the rapid view which the eyes take of the whole solid, converging their axes upon every point of it, and thus seeing each point in succession single and distinct. Hence, we must draw a marked distinction between the vision of the solid (as an optical fact) when the eyes are fixed upon one point of it, and the resultant perception of its figure arising from the union of all the separate sensations received by the two eyes.

Let ABCD, fig. 16, be the solid frustum of a cone, having its axis MN produced, bisecting at O the distance LR between the two eyes L,R. Draw AL, AR, BL, BR; and also CL, CR, and DL,DR. Then, if we look at this solid with the left eye L only, the projection of it will be as shewn in fig. 17 at ABCD, and in fig. 16 at A′B′CD; AC being much greater than DB, and the summit-plane CD appearing on the right-hand side of the centre of the base AB. The reason of this is obvious from fig. 16, where the left eye L sees the side AC under the angle ALC, while it sees the other side DB under the much smaller angle BLD; the apparent magnitude being in the one case A′C, and in the other DB′. In like manner, the right eye R sees DB under the large angle BRD, and with an apparent magnitude D b'; while it sees AC under the smaller angle ARC, and with an apparent magnitude C a'. Hence it follows, that, with both eyes, we shall see the solid in perfect symmetry, with its summit CD concentric with AB; and hence the reason is obvious why the two dissimilar pictures in the retina give a resultant picture corresponding with the solid itself.

Quitting our solid frustum of a cone, let us now suppose that its two dissimilar projections ABCD, *a b c d*, fig. 17, are united by the two eyes L,R, converging their axes to a point nearer the observer. By drawing lines from A,B,C,D, *a*, *b*, *c*, *d*, to L and R, the centres of visible direction, it will be seen that the circles AB, *a b* at the base, can be united only by converging the optical axes to M, and the summit circles CD, *c* D only by converging the axes to N. Hence, *m n o p* will represent the solid frustum of a cone, whose axis is MN. Now, all the rays which flow from any point of the two projections AB, *a b*, cross each other at the figure *m n o p*; and, consequently, this figure is seen by both eyes in identically the same manner as if the rays which really emanate from the plane figures had emanated from their points of intersection, that is, from the outlines of the solid figure *m n o p*.

In order to see the base *m n*, the optic axes must be converged to M, or any other point of the base; and in order to see the summit *o p* distinctly, the axes must be converged to N. But the distance MN is so very small, that the whole outline *m n o p* will be seen with great distinctness; though it is certain that every point of it, but one, is seen double.

The height MN of the cone, fig. 18, is $=cot\ \frac{1}{2} A - cot\ \frac{1}{2} A'$, A, A' being the angles of the optic axes LMB, LNR, and OL or OR radius. But as these angles are not known, we may find MN thus :—Let D=distance OP ; d=S s, the distance of the two points united at M ; $d_{,}$=S′s', the distance of the two points united at N ; C=LR=2½ inches. Then MP$=\frac{Dd}{C+d}$; NP$=\frac{Dd'}{C+d'}$; and MN$=\frac{Dd'}{C+d}$. When the two figures are united by converging the axes beyond P, the base $m\ n$ of the line will be nearest the eye ; and, consequently, the cone will appear *hollow*. In this case, M′N′$=\frac{Dd}{C-d}-\frac{Dd'}{C-d'}$; and the cone will be much larger than in the other case. If we make

$$D = 9.24 \text{ inches,}$$
$$C = 2.50\text{; then}$$
$$d = 2.42\text{;}$$
$$d' = 2.14\text{; and}$$
$$MN = 0.283\text{, the height of the cone.}$$

Whereas, in the second case, M′N = 18.9 feet !

Considering that the summit-plane $o\,p$ rises above the base $m\,n$, in consequence of the convergency of the optic axes at N, it may be asked, how it happens that the frustum still appears a solid, and the plane $o\,p$, where it is, when the optic axes are converged to another point M, so as to see the base $m\,n$ distinctly? Should not the relief disappear, when the condition on which it depends is not fulfilled? But, instead of the relief disappearing, the summit-plane $o\,p$ maintains its position there as fixedly as if it belonged to the real solid; and it ought to do so, for the rays emanate from it in exactly the same manner, and form identically the same image on the retina as if it were a real solid. Now, by the mere advance of the intersection of the optic axes from M to N, the rays from the circles AB, CD, &c. still produce the same picture on the retina of each eye, and the only effect of the advance of the point of convergence from N to M, is to throw that picture a little to the *right* side of the optic axis of the *left* eye, and a little to the *left* of the optic axis of the *right* eye; so that the summit $o\,p$ still retains its place, and is merely seen double.

6. *On the Doctrine of Corresponding Points.*

Our celebrated countryman, Dr Reid, calls those *points* in the retina of each eye *corresponding*, which are similarly situated with respect to the *foramen centrale*, or centre of each retina; and he maintains that objects painted on those points have the same visible position. He observes "that the most plausible attempts to

account for this property of the eyes have been unsuccessful, and that it must be either a primary law of our constitution, or the consequence of some more general law which is not yet discovered." This doctrine has been very generally admitted; and if great names could have given it currency, those of NEWTON and WOLLASTON, supported by a number of anatomists and metaphysicians, might have placed it, both optically and metaphysically, beyond the reach of challenge. The doctrine of the semidecussation of the fibres of the optic nerve, as explained by NEWTON, gave great support to the theory of corresponding points. The idea that each fibre of the nerve divided itself into two, one of which went to a given point in the retina of one eye, while the other went to the corresponding point in the retina of the other eye, seemed to be at once an explanation and a proof of the doctrine.

Whether the anatomical supposition be true or false is a matter of little consequence at present, as the doctrine which it supports is not true excepting in the single case where the optic axes are parallel, and in this case it is true only because it is a necessary consequence of the general law of visible direction.

Along with the theory of corresponding points, we must rank the *binocular circle* of the Germans in which it is embodied. Let R, L, fig. 18, be the right and left eyes whose centres of visible direction are C, C′, and whose optic axes CA, C′A, converge to any point A. Through the three points A, C, C′, describe the circle ABCC′. This circle is called the *Binocular Circle*, because if we take any point B in its circumference, and draw BCE, BC′E′, the points E, E′ on the retinæ will be corresponding points, that is, points equidistant from D (because the angles ACB, AC′B being equal, DC′E′ and DCE are also equal), and consequently when the optic axes are directed to A, an object at B will have its image formed upon the corresponding points E, E′, and will be seen single.

Now, when the optic axes are directed to A, a ray from B will fall upon the left eye at L with a greater angle of incidence than on the right eye at R; and consequently it will strike the retina at a point farther from D in the left eye than in the right eye; that is, if the ray BR is refracted to E, the ray BL will be refracted to some point *e*, and consequently the lines of visible direction EC, *e* C will meet in a point without the circle ABC. The real *binocular curve*, therefore, is everywhere without the circle. Hence the doctrine of corresponding points is not true; and if it had been true, it would have been so because it was a necessary consequence of a law of visible direction.

7. *On the Vision of Cameos and Intaglios.*

The beautiful experiment of converting a *cameo* into an *intaglio*, and an *intaglio* into a *cameo*, by *monocular* vision, is well known. In 1825 I had occasion

to investigate this subject, and in January 1826 I published an account of my observations, with an ample notice of the previous labours of other authors.*

Mr WHEATSTONE has ingeniously connected this optical fallacy with the union of dissimilar images on the retina, though he does not refer it to this union as its cause. After quoting my previous explanation of the illusion, he makes the following observations upon it. "These considerations do not *fully* explain the phenomenon, for they suppose that the image must be inverted, and that the light must fall in a particular direction; but the conversion of relief will still take place when the object is viewed through an open tube without any lenses to invert it, and also when it is equally illuminated in all parts."† In thus objecting to the *fulness* of my explanation, Mr WHEATSTONE has overlooked the great number of experiments by which I have supported it; and especially those facts in which I observed the fallacy when the object is viewed *without even an cpen tube,—without inversion;—with both eyes open*, and when it is *placed in broad daylight*. Mr WHEATSTONE then gives his own opinion as follows. "If we suppose a cameo and an intaglio of the same object, the elevations of the one corresponding exactly to the depressions of the other, it is easy to shew that the projection of either on the retina is sensibly the same.‡ When the cameo or the intaglio is seen with both eyes, it is impossible to mistake an elevation for a depression; but when either is seen by one eye only, the most certain guide of our judgment, viz., the presentation of a different picture to each eye, is wanting; the imagination therefore supplies the deficiency, and we conceive the object to be raised or depressed according to the dictates of this faculty. No doubt, in such cases our judgment is in a great degree influenced by accessory circumstances, and the intaglio or the relief may sometimes present itself according to our previous knowledge of the direction in which the shadows ought to appear; but the *real cause* of the phenomenon is to be found in the *indetermination of the judgment*, arising from our more perfect means of judging being absent."§

Now, what Mr WHEATSTONE calls the *real cause* of the illusion is no *cause* at all,—it is merely a previous state of the mind which is favourable to the operation of the real cause. Two eyes, like two witnesses, must always bear a better testimony to truth than one; and, in the present case, the want of the convergency of the optic axes to estimate the distance of the highest and lowest points of the cameo and the intaglio, undoubtedly favours the illusion, and allows the real cause to influence the judgment; but even here this admission has its limits,

* This account was published anonymously in the *Edinburgh Journal of Science* for January 1826, No. VII. vol iv. p. 97; and a popular abstract of it afterwards appeared in my *Letters on Natural Magic*, Letter V. p. 98.

† *Phil. Trans.* 1838, p. 383.

‡ This is true only when they are not seen obliquely.—D. B.

§ *Phil. Trans.* 1838, p. 384.

for in very shallow cameos and intaglios the illusion takes place with both eyes.*

Without repeating in this place the various facts respecting mother-of-pearl and other phenomena in which I observed the illusion when both eyes were used, I shall content myself with quoting the following observation, made in Egypt by Lady GEORGIANA WOLFF. "Lady GEORGIANA," says the Rev. Mr WOLFF, "observed a curious optical deception in the sand about the middle of the day, when the sun was strong; all the *foot-prints*, and *other marks that are indented in the sand, had the appearance of being raised out of it*; and at those times there was such a glare that it was unpleasant for the eye."†

8. *On the Change in the Apparent Position of the Drawings of Solid Bodies.*

Although this illusion may have been previously observed, yet I believe Professor NECKER of Geneva is the first person who has described and explained it. He mentioned it to me in conversation in 1832; and afterwards sent me a notice of it, which I published in the London and Edinburgh Philosophical Journal.‡ Mr NECKER describes the illusion in the following manner. "The rhomboid AX, fig. 19, is drawn so that the solid angle A should be seen the nearest to the spectator, and the solid angle X the farthest from him, and that the face ACBD should be the foremost while the face XDC is behind. But in looking repeatedly at the same figure, you will perceive that at times the apparent position of the rhomboid is so changed that the solid angle X will appear the nearest, and the solid angle A the farthest, and that the face ACDB will recede behind the face XDC, which will come forward; which effect gives to the whole solid a quite contrary apparent inclination." Professor NECKER observed this change "as well with one as with both eyes," and he considered it as owing "to an involuntary change in the adjustment of the eye for obtaining distinct vision. And that whenever the point of distinct vision on the retina was directed on the angle A, for instance, this angle seen more distinctly than the others, was naturally supposed to be nearer and foremost; while the other angles seen indistinctly were supposed to be farther away and behind. The reverse took place when the point of distinct vision was brought to bear upon the angle X." Upon this explanation Mr WHEATSTONE makes the following observations: "That this is not the true explanation is evi-

* When the cameo and intaglio are viewed very obliquely, one of the causes of deception disappears. In the case of a cameo appearing depressed, the depression disappears the instant that the shadow of the cameo encroaches distinctly upon the plane surface from which it is raised, because an intaglio never can, however obliquely viewed, throw a shadow upon the plane surface out of which it is excavated. For the same reason, an intaglio seen very obliquely will not rise into a cameo, because the shadow on the plane surface is wanting.

† Journal of the Rev. Joseph Wolff, 1839, p. 189. ‡ Vol. i. p. 334.

dent from *three* circumstances: in the *first* place, the two points A and X being both at the same distance from the eyes, the same alteration of adjustment which would make one of them indistinct would make the other so; *secondly*, the figure will undergo the same changes whether the eye be adjusted to a point before or behind the plane in which the figure is drawn; and, *thirdly*, the change of figure frequently occurs while the eye continues to look at the same angle. The effect seems entirely to depend on our mental contemplation of the figure, or of its converse. By following the lines with the eye, with a clear idea of the solid figure we are describing, it may be fixed for any length of time; but it requires practice to do this, or to change the figure at will. As I have observed before, these effects are far more obvious when the figures are regarded with one eye only."

In a case of this kind, where one eminent individual assures us that he has proved his explanation to be true in *three* different ways, and another maintains that this explanation is evidently not the true one from *three* different circumstances, there must be a misapprehension to be removed, as well as a difficulty to be solved. It is impossible to read Mr NECKER's paper without discovering that Mr WHEATSTONE has entirely mistaken his meaning, though the mistake is partly owing to Mr NECKER's use of the phrase, "adjustment of the eye for obtaining distinct vision." Mr WHEATSTONE understands this to mean the adjustment of the eye to A or X, as if they were at different distances from the observer; whereas Mr NECKER clearly refers to that indistinctness of vision which arises from distance on the retina from the *foramen centrale*, or point of distinct vision. When the eyes are converged upon A, X is seen indistinctly, and *vice versa;* and that his is Mr NECKER's meaning is obvious from the following conclusion of his letter: "What I have said of the solid angles is equally true of the edges,--those edges upon which *the axis of the eye, or the central hole of the retina, are* directed, will always appear forward; so that now it appears to me certain that this little, at first so puzzling, phenomenon, depends upon the law of distinct vision." That this is the true cause of the phenomenon I have no hesitation in affirming. By hiding A with the finger, or making it indistinct with a piece of dimmed glass, or throwing a slight shadow over it, X appears forward, and continues so when these obscurations are removed; and the same effect is produced by hiding X, A becoming then nearest to the eye. This experiment may be still more satisfactorily made by holding above the rhomboid a piece of ground glass (the ground side being farthest from the eye), and bringing one edge of it gradually down till it touches the point A, the other edge being kept at a distance from the paper. In this way AX, and all the lines diverging from A, become dimmer as they recede from A, and consequently A becomes the most forward point. A deep plano-convex lens, with its convex side ground, will answer the purpose still better, the apex of the lens being laid upon A or X; or the effect may be still farther improved

by making the roughness increase either from the apex of a convex surface, or any fixed point of a plane one.

Following out his general opinion of the superiority of binocular vision, Mr WHEATSTONE remarks, that the illusion which we have been examining is most obvious with one eye. It is not so with my eyes; and I conceive it should not be so, as the convergency of the optic axes can have no efficacy in preventing illusion when the figure occupies a plane surface.

In the course of the investigation which I have now brought to a close, I have had occasion to observe many very interesting phenomena, which it would be out of place to describe at present. They relate partly to the effects produced by uniting unequal and dissimilar pictures which have a tendency to represent incompatible solids;—to the union of dissimilar pictures, when the parts of the solid which they tend to produce lie wholly or principally in a plane perpendicular to the line joining the eyes and to the plane of the optic axes;*—to the union of pictures, one of which is more or less turned round in its own plane;—to the phenomena exhibited by uniting the images of two similar real solids, the one elevated and the other depressed;—to the union of dissimilar plane figures which should at the same time give a solid in relief, and in the converse of relief;†—and to the union of portions of dissimilar figures, those which are wanting in the one figure existing in the other. Among the singular effects produced under these various conditions, nothing is more remarkable than the tendency or desire, as it were, of the eyes, to unite and fix the two pictures hovering before them, to convert them into some figure of three dimensions (sometimes in relief, sometimes in the converse, and sometimes in both at the same time); and the suddenness with which the two images start into union, give birth to a solid figure on which the optic axes are converged, and release the eyes from that unnatural condition in which they had previously been placed.

ST LEONARD'S COLLEGE, ST ANDREWS,
January 1843.

* Such as the magnified teeth of a saw, as in fig. 14, or a thin section of a hexagonal prism whose axis is parallel to a line joining the eyes.

† In order to produce simultaneously this double effect, the lines of the pyramid, for example, which are to give the converse of relief, should be fainter than the other lines, or in different and feebler colours.

XXV.—*On the Growth and Migrations of the Sea-Trout of the Solway* (Salmo trutta). *By* Mr John Shaw, *Drumlanrig. Communicated by* James Wilson, Esq., F.R.S.E.

(Read 27th March 1843.)

Although the sea-trout (*Salmo trutta*) cannot be considered either of so much importance to the community as an article of food, or so interesting in its habits and economy to the naturalist as the true salmon, nevertheless, it is universally allowed to rank next in value to that species. It holds a high place in the estimation of the public as an article of diet, and is, consequently, an object of great commercial value to our fisheries. Its history being still almost as involved and obscure as was that of the salmon some seasons back, I am induced to offer the following remarks with a view to its elucidation.

From the circumstance of the sea-trout fry, in its earlier stages, bearing such a marked resemblance to the young of the common river-trout (*Salmo fario*), enquirers, even of the closest habits of observation, have had much difficulty in assigning to it any other place among our British Salmonidæ than that of a mere variety of the last named species. However, by proceeding on the mode of artificial or mechanical impregnation, the exact species of the parent fishes being previously ascertained, there can remain no doubt as to the identity of the progeny, under whatever diversity of colour and markings they may shew themselves, in their progress to the adult state.

It being a question among naturalists whether the so-called herling (*Salmo albus* of Dr Fleming) is actually a distinct species, or only a certain progressive state of the young of the sea-trout, I have taken every opportunity of determining the question by marking the fins, &c., of a considerable number of these fishes for several years, according as the seasons and conditions of the river enabled me to obtain them; and many specimens, as the details of my experiments will shew, were taken and retaken, for several successive seasons, as common sea-trout, increasing in dimensions from year to year. But as I now know the herling to be only one of the links in the progressive chain, it will be better, instead of referring separately to that state of the fish, to narrate the details of the several experiments in the order in which the species advances in size, rather than that in which the experiments upon their several stages were conducted.

In consequence of experiencing much difficulty in capturing the parent sea-trouts in the act of depositing their spawn in the tributaries, I had recourse for many years to the plan of taking them from the cruive on the river, during the

summer months, when the real species to which they belonged was less difficult to determine. I then placed them in ponds, with a good supply of wholesome water, and spawned them artificially when the due season arrived. This method, however, I never considered as a very legitimate mode of procedure, from the circumstance of a possibility of error from an improper selection and combination of the parent fishes, and I therefore watched every opportunity of capturing them in the act of depositing their spawn in the natural bed of the streams or tributaries, each fish accompanied by a mate of its own selection. It was not, however, until the autumn of 1839 that I had the good fortune to capture the parent fishes under these circumstances, and this I effected by the aid of my fowling-piece.

On the 1st of November 1839, having discovered a pair of sea-trouts (See parent specimens A) engaged in depositing their spawn in the gravel of one of the small tributaries of the river Nith, and being unprovided at the moment with the necessary apparatus for their capture, I had recourse to shooting, as the only mode within my power of insuring instant possession of them. However, the vigilance exercised by both parents in protecting the ova from being devoured by multitudes of smaller fishes which surrounded them, rendered it exceedingly difficult to seize the precise moment at which both might be disabled by one discharge of the piece. This, however, was at length effected by shooting immediately across the heads of the pair as they lay parallel to each other, but more by the influence of concussion, than the actual effects of the shot, they being at the time in about six inches depth of water. Having taken them ashore, I proceeded to spawn them by pressing the ova from the body of the female into a little water by the side of the stream, and afterwards, by the same process, I caused the milt from the body of the male to mingle with it. I then removed the impregnated ova in a copper-wire gauze bag, in which some fine gravel had been placed, to a little stream connected with my experimental ponds. The temperature of the water was at this time 47°, but during the winter it ranged a few degrees lower. By the 40th day after impregnation, the embryo fish were visible to the naked eye, and on the 14th January 1840 (75 days after impregnation) the fish were excluded from the egg.

The specimen No. 1 exhibits the young as it existed the day on which it was hatched, with a single specimen of the ovum to shew the condition the day before hatching. The brood at this, the earliest period, exhibit no perceptible difference from the young of the salmon of a corresponding age, except that they are somewhat smaller in size, and of a paler blue upon the body, the vitelline bag being likewise of a lighter red.

The specimen No. 2 is the young sea-trout of two months old, taken from the ponds on 17th March 1840. It is about 1 inch in length, and has assumed those lateral markings which seem to characterize the earlier stages of all the

species of this family, but its progressive growth, the lapse of time considered, has been extremely small.

No. 3 shews the state of the young sea-trout when four months old, taken in May. It measures about 2 inches in length, and by its considerably enlarged size, and improved condition, exhibits the effects of an increase of food and temperature.

No. 4 is a specimen of young sea-trout 6 months old, taken in July. It measures 2½ inches in length, and exhibits a corresponding progressive improvement in size and condition.

No. 5 is a specimen 9 months old, taken in October. It measures about 3 inches in length, and also exhibits an improved aspect.

No. 6 is the same species when 12 months old, taken in January 1841. It measures about 3½ inches in length, and presents an example of the average size, and somewhat defective condition, of the brood during the winter months.

No. 7 is a specimen of young sea-trout 21 months old, taken in October 1841. It measures about 6 inches in length, and has now lost all the lateral bars or transverse markings which are so characteristic of the younger stages of the British Salmonidæ. It forms one of the most interesting specimens of the series exhibited. It bears a very marked resemblance to some of the varieties of the common river-trout, and it has also now attained that age (from 18 to 20 months) at which it appears that the whole of the males of the migratory species of our Salmonidæ are capable of procreation,—none of the females, however, of a corresponding age, in any of my broods, having ever been observed to mature their roes. But, from the experiments which I have repeatedly made with the milt from the young males of other broods of this age (18 months or upwards), I find them quite capable of reproducing their kind with the adult females.

No. 8 is a specimen of the young sea-trout upwards of 2 years old, taken in May. It measures about 7¼ inches in length, and has now, along with three-fourths of the brood, assumed the migratory dress. As the young of this species, in the migratory state, are not unfrequently mistaken by ordinary observers for that of the real salmon, it may be proper here to endeavour to give some general description of the colour and markings which they exhibit when first taken from the ponds. The whole brood, at the age of 2 years, average about 7 inches in length, and are of a dark brown on the back, passing gradually into a white silvery appearance on the sides and belly; the pectoral fins are white, with one-third part (the extremities) orange; the ventral fins are pure white; the anal fin is also white, with a faint marking of dusky on each side; the dorsal fin is of a lightish brown, inclining to black at the extreme points of the anterior rays, which are tipped with a very little white; the posterior portion of the rays of the same fin have a faint tint of orange, and the whole fin is very much spotted; the adi-

pose fin is dark brown, margined with red; the caudal rays are of a light colour near the base, running into dark orange, terminated by a faintly marked double margin of black. The spots on the back and sides vary much in individuals of the same brood, but the specimens produced exhibit a pretty correct example of the general markings of the brood. The spots prevail principally along the back, with a few below the lateral line. Each spot is surrounded by a circle of a lighter colour than the general surface of the body, and this appears to be a prevailing character of the trout species, and one which the sea-trout fry exhibits even after having assumed the migratory dress, when every other feature of resemblance to the common trout has disappeared. The spots on the gill-covers are also more numerous than those of the salmon, generally amounting to 5 or 6.

From the ovum up to the migratory state, the natural economy of the sea-trout appears to bear a resemblance to that of the real salmon. However, from the latter stage to that of the herling (which, as I have said, is beyond all doubt the young of the sea-trout), there is certainly a singular departure from the uniform progress of that species. It appears from experiments very carefully conducted, that there is always a certain number of individuals of both sexes (probably about one-fourth of each brood) that never assume the silvery exterior, or migratory dress; and even if those which have assumed that appearance be detained in fresh water for a month or two, they will reassume the dusky coating; and by the beginning of the ensuing autumn, the females mature the roe sufficiently to reproduce their species with young males of corresponding age. As an evidence of this fact, I may here detail the particulars of an experiment made upon individuals in the condition alluded to. Having discovered, on the 25th of November last, that the whole of the females of the brood, as well those which had been silvery as those which had not, were exhibiting signs of being in the breeding state, I took six individuals (three males and three females, the latter having matured their roe for the first time at the age of $2\frac{1}{2}$ years, the former being *milters* for the second time), and commingling the milt and roe, I placed the impregnated ova in wire bags in a stream connected with the ponds, and in 76 days thereafter they were hatched, and the brood now exhibits as healthy a condition in every respect as those produced by adult parents from the sea.

From these facts, it may perhaps be inferred that the sea-trout bears, in some respects, a closer affinity to the common trout than it does to the real salmon; and that that portion of each brood which does not assume the migratory dress, but matures healthy roe and milt, and is capable of reproduction at the proper season without going to sea, forms one of the supposed varieties observable in all rivers to which migratory trout have access. It is then by no means improbable, that portions of each brood are permanent residents in fresh water, as they are never observed to migrate in a dusky state, along with the shoals of silvery fry,

at the usual season of migration. In support of such views, we have the authority of Dr M'Culloch, who states, that sea-trout are now permanent inhabitants of a fresh-water loch in the island of Lismore.

Having detailed the several particulars relating to my experiments on the ova of the sea-trout up to the migratory stage of the brood, it will now be necessary to recur to the results of experiments made on the fry while migrating to the sea.

On the 9th of May 1836, having observed the salmon fry descending towards the sea, I took the opportunity of capturing a number of them, by admitting them into the salmon cruive, and, on examination, I found about one-fifth of each shoal to be what I regarded as sea-trout; and conceiving this to be a favourable opportunity of ascertaining the fact by actual experiment, I proceeded to mark every individual of that species which entered the cruive in the course of the day. They amounted to about 90. A fresh, however, taking place in the river in the evening, prevented my following out the experiment any farther that season. In experimenting on migratory fishes at 25 miles distance from the sea, windings included, the chances of recapturing the individuals marked is comparatively small, and I therefore did not calculate upon retaking more than an individual or two out of the 90. My expectations were not agreeably disappointed by any better success than I had anticipated. However, on the 16th July, just 80 days afterwards, I recaptured a herling in the cruive, with the mark which I had put upon the young sea-trout of the previous May, viz., the whole of the adipose fin being taken off, and three-fourths of the posterior rays of the dorsal fin removed. It measured about 12 inches in length, and weighed 10 ounces. The average weight of the sea-trout fry, at the age at which they migrate to the sea, is about $3\frac{1}{2}$ ounces, so that the specimen referred to exhibited an increase of weight of $6\frac{1}{2}$ ounces in about 80 days' residence in salt water. It was my intention to have retained specimens of the several individuals on their return from the sea, as they were successively retaken, with the view of exhibiting more correctly the development and growth of the species in salt water. This single specimen, however, of that year having been injured in the cruive, was deemed unfit for the object in view, and was therefore set at liberty, in the hope of obtaining one in a more perfect state. In this, however, I was disappointed, no other having presented itself during the remainder of that season. But No. 9 is a specimen of the herling of the Nith, taken from the river in July, and is exhibited as an example of the next progressive stage ensuing that of the silvery condition of the smolt or fry (No. 8). It may also be considered as a correct representative both of the specimen alluded to, and of the general state of the species at that age. These herlings enter our rivers in most abundance in the months of July and August.

No. 10 is a specimen exhibiting the next state of advance beyond the her-

ling, and is actually one of the 90 individuals marked as fry in May 1836, though not captured on its first return. It was retaken on the 1st of August 1837,—fifteen months after being marked as a fry on its way to the sea. It weighed about two pounds and a half, and represents pretty correctly the average size of sea-trout on their second migration from the sea. These older fish usually make their appearance in our rivers in greatest abundance in the months of May and June. In consequence of the specimen now before you having been in fresh water for a considerable time, it has acquired a dusky exterior. To shew the better and brighter aspect of the species, I have placed beside it a specimen of corresponding size taken from the river in May.

In consequence of the herling having greatly abounded in the river Nith in the summer of 1834, I took the opportunity of marking a great number of them (524), by taking off the adipose fin, and returning them into the river. During the following summer (1835), I recaptured 68 of the above number as sea-trout, weighing on an average about 2½ pounds. On these I put a second distinct mark, and again returned them to the river; and on the next ensuing summer (1836) I recaptured a portion of them, about 1 in 20, averaging a weight of 4 pounds. I now marked them distinctively for the third time, and once more returned them to the river, also for the third time. On the following summer (23d day of August 1837), I recaptured the individual now exhibited (No. 11) for the fourth time. It then weighed 6 pounds. This fish exhibits the nature of the several different marks put upon itself and the other individuals, as they were successively recaptured, from year to year, on their return to the river. These marks were as follows: 1*st*, The absence of the adipose fin (herlings in 1834); 2*d*, One-third part of the dorsal fin removed (sea-trout in 1835); 3*d*, One-half of the anal fin taken off (large sea-trout in 1836). Captured and killed in 1837.*

I also marked, in the summer of 1835, about 120 sea-trout, by putting copper-wire into the dorsal fin of one-half of that number, while I marked the other half by twisting a small portion of the copper-wire round the right maxillary bone. Of the latter group I recaptured five individuals the following summer, and found that they also had gained an average increase in weight of 1½ pound. None were recaptured with the wire in the dorsal fin, which I attribute to the circumstance of that part being of too fragile a nature to retain the wire for a sufficient length of time; and therefore, though they no doubt returned, they could not be recognised.

From the numerous and long-continued experiments which I have thus been conducting for many years on this species of migratory trout, I have come to the

* Where only the cartilaginous portion of the fins is taken off, they frequently prove defective marks, as nature always makes an effort to heal and restore those important organs of locomotion, when injured. But when taken off close to where they articulate with the body, the parts are never restored. See, as examples, the dorsal and anal fins of No. 11.

conclusion that the small fry called *Orange Fins*, which are found journeying to the sea, with the true salmon fry, are the young of the sea-trout of the age of two years; that the same individuals, after 9 or 10 weeks' residence in the sea, ascend our rivers for the first time as herlings, weighing about 9 or 10 ounces, and on the approach of autumn pass into our smaller tributaries, for the purpose of continuing their kind; that having spawned, they soon again make their way to the sea, during their residence in which, they almost wholly acquire their increase of weight, viz. about $1\frac{1}{2}$ pound per annum; and that they return annually, with an accession of size each season, to the same river in which their parents gave them birth.*

* Among innumerable authentic and well-known instances of the migratory Salmonidæ returning to their own rivers, I may state, that, of the many hundreds of this species which I have marked, I am not aware that even one of them has ever found its way into any of the tributaries of the Solway, saving that of the river Nith.

I may here note, in reference to the change of colour in fishes in relation to the bed on which they rest, that if the head alone is placed upon a peculiar shade, whether light or dark, the whole body of the fish will immediately assume a corresponding shade, entirely independent of the colour of the ground on which the body itself may happen to rest.

XXVI.—*On the Optical Phenomena, Nature, and Locality of Muscæ Volitantes; with Observations on the Structure of the Vitreous Humour, and on the Vision of Objects placed within the Eye.* *By* Sir David Brewster, *K.H.D.C.L., F.R.S., and V.P.R.S. Edin.*

(Read, 6th March 1843.)

Although some of the phenomena of *Muscæ volitantes* may be seen by persons of all ages, and with the best eyes; and though those which are more peculiarly entitled to the name are exceedingly common beyond the middle period of life; yet no account has been given of them that has even the slightest pretension to accuracy. M. De La Hire, in his *Differens accidens de la Vue*, describes these *Muscæ* as of *two* kinds; some permanent and fixed, which he ascribes to small drops of extravasated blood upon the retina; and others, as flying about, and changing their place, even though the eye be fixed. The *first* kind, he describes as like a dark spot upon a white ground; and the *second*, as like the knots of a deal board. Some parts of them, he says, are very clear, and surrounded with dark threads, and are accompanied with long fillets of irregular shapes, which are bright in the middle, and terminated on each side by parallel black threads.

In order to account for these knots and irregular fillets, De La Hire supposes that "the aqueous humour is *sometimes* troubled with some little mothery ropy substance, some parts of which, by the figures of their little surfaces, or by refractive powers different from the humour itself, may cast their distinct images upon the retina. He supposes them in the *aqueous* humour rather than in the vitreous, because of its greater fluidity for a freedom of descent, and because they will then appear to descend, as being situated *before* the pupil, or, at least, *before* the place of intersection of the pencils." *

Dr Porterfield, who has given a very inaccurate drawing of the filamentous *muscæ*, considers them as produced by diaphanous particles and filaments, that swim in the *aqueous* humour before the crystalline; and he regards the distinct pictures of them upon the *retina* of *long-sighted* persons, as produced by the rays which pass *through* the dense particles, having suffered a greater refraction than those which pass *by* them, so as to be converged to *foci* upon the retina.†

The latest writer on this subject, Mr Mackenzie of Glasgow, describes the *muscæ* as resembling *minute, twisted, semi-transparent* tubes, partially filled with

* Smith's Optics, vol. ii. Rem. p. 5.

† Treatise on the Eye, vol. ii. p. 74–80.

globules, which sometimes appear in motion; while another set are more opaque, or perfectly dark, and follow the motions of the eye. The latter he considers as "of a more dangerous character than the former, and as occasioned, generally, by a partial insensibility of the retina," either from the pressure of some "irregular projecting point or points of the choroid, or from some other cause." Mr MACKENZIE regards the globules within the semi-transparent tubes, as probably "blood passing through the vessels of the retina, or of the vitreous humour;" and he remarks, "that neither these semi-transparent tubes themselves, nor any of the filamentous muscæ, or black spots (which are so frequently complained of), possess any real motion, independent of the general motion of the eyeball;" and hence he concludes that they "must be referred either to the retina itself—including, of course, the three laminæ of which it is composed,—or to the choroid coat." "The probability is," he adds, "that the semi-transparent muscæ, of a tubular form, are owing to a dilatation of the branches of the *arteria centralis retinæ*.*"

Such was the state of our information on the subject of *Muscæ volitantes*, when my attention was specially directed to it, in consequence of finding in my own eye a good example of the phenomenon; and, having carefully investigated the facts as observed by other persons in their own eyes, I trust I shall be able to lay before the Society a correct description, and a satisfactory explanation, of the general phenomena.

Although the bodies which are within the eyeball, and give rise to the phenomena under consideration, are often seen under ordinary circumstances, yet, in order to see them with distinctness, we must look at the sky, or a luminous object, either through a very minute aperture, or, when the light is limited or feeble, through a lens or microscopic doublet, of very short focus, held close to the eye. By this means, we shall observe a luminous ground, covered, more or less, with transparent filaments or tubes, transparent circles, exceedingly minute, and (when they do exist) with *Muscæ*, or black spots like flies.

In examining the transparent filaments, I have observed them of *four* or *five* different sizes, the smallest of which are the most distinct. These distinct filaments are bounded by two sharp black lines, and the space between them is more luminous than the general ground on which they are seen. In the larger filaments, the black lines are coloured at their edges, and, on the outside of each of them, are one or more coloured fringes.

The minute transparent circles, when smallest, have a luminous centre, with a sharp black circle round it. In the larger ones, this circle is coloured at its edges; and, on the outside of it, are one or more circular coloured fringes. These spherical bodies sometimes exist singly, and sometimes in groups, partly connected by small filaments, and partly by an invisible film, to which they seem attached.

* Practical Treatise on the Diseases of the Eye, 1830, pp. 748, 750.

They sometimes adhere to the outside of the filaments, and very frequently occur *within* the filaments, so as to prove that these filaments are *tubular*. These spherical bodies have, like the filaments, *four* or *five* different sizes.

In making observations on these spherical bodies, the observer will sometimes see luminous spots pass through the field; but as these arise from the state of the lubricating fluid on the outside of the cornea, they have no connection with the phenomena under our consideration.

The transparent filaments, already described, are seldom seen single. Two or three are united, like threads crossing one another; and sometimes a great number are united, like a loose heap of thread, in which case, obscure spots appear at the places where the crossings of the filaments are most numerous.

In some cases, a single long filament is once or twice doubled up upon itself, and sometimes a *knot* is, or appears to be, tied upon it, consisting of several folds, as it were, of the filament. This *knot* has several *very dark spots* at the places where the different portions of the filament are in contact; and this accumulation, as it were, of black specks, constitutes the real *muscæ*. In many, indeed in almost all of these *muscæ*, when distinct, a little bright yellow light accompanies the black specks.

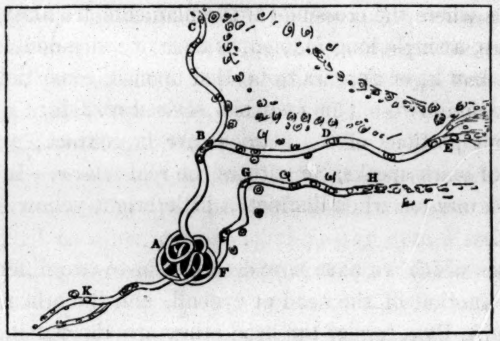
All the bodies which we have now described have two different motions; one arising from the motion of the head or eyeball, and the other when the eyeball is absolutely fixed. By a toss of the head, they are thrown into different absolute and relative positions, sometimes ascending and descending in succession, sometimes oscillating between two limits, and generally with different velocities. When the eye is first applied to the lens or aperture, the field of view is tolerably free of these moving bodies; but the light seems to stir them up, as it were, and, to a certain extent, the longer we view them, the more numerous do they become.

If the centre of motion of the eyeball coincides with the centre of visible direction, the *Muscæ* will *ascend* when the eye looks *upward*, and *vice versa*, whether they are placed before or behind that common centre. If the eyeball remains fixed, the *Muscæ* in *front* of the above centre will have the *direction* of their *real* and *apparent* motions the same, and those *behind* that centre will have these two directions *different*. Hence the appearance of two opposite currents when the eyeball is turned quickly from one extreme of its range, either vertically or horizontally, to its mean position; and so rapid is their motion through the luminous field, that it seems covered with continuous lines parallel to the direction in which the eyeball has been moved,—an effect arising from the duration of the impression of light upon the retina.

If we mark individual filaments, or groups, or knots, we shall find that they change their shapes, one part of a filament doubling itself over another, and again resuming its elongated form. The minute spherical bodies separate and approach

one another; but I have not been able to satisfy myself that those within the tubular filaments change their place. They often *appear* to do so; but as this *may* arise from the bending of the filament, or from the varying obliquity of different parts of it arising from its change of form or place, we are not entitled to consider them as moveable within the tube. It is certain, however, that they have no progressive motion, as supposed by Mr Mackenzie.

In order to obtain a correct knowledge of the phenomena of the real *Muscæ*, I confined my attention to one in my own eye, of which I first made a drawing in October 1838. It is represented in the annexed figure, and consists of four fila-



ments, ABC, BDE, FGH, and AK. Between BC and BDE there is a sort of transparent web containing a great number of minute spherical specks, and something similar, though less extensive, below FGH. The real *Musca* exists at A, and has obviously been produced by the accidental overlapping of the different filaments which are united with it. In four and a half years, the *Musca* at N has perceptibly increased in size, and the length of the associated filaments has diminished. It is distinctly seen without any of its accompaniments in ordinary light, but is, in no respects, injurious to vision, as it is never stationary in the axis of the eye. When seen by means of the lens, the long branch FGH takes various positions, sometimes falling below the knot or *musca* A, and sometimes crossing the main branch AB, below B. The branch BDE has often a loop at D, and FGH another at G.

Having had occasion to study the phenomena of the diffraction of light, as produced by transparent fibres and films of different forms, I could not fail to observe that the phenomena above described were the shadows formed on the retina by divergent light passing by and through transparent filaments and particles placed within the eyeball. They are indeed perfectly identical, and may be accurately imitated in various ways. If we crush a crystalline lens in distilled water, or

macerate some very thin laminæ of it, and dry a drop of the fluid on a piece of glass, we shall perceive, with a fine microscope a little out of focus, or with an ill adjusted illuminating apparatus, a number of minute fibres, single and in groups, and knots, with minute spherical particles, which display the very same phenomena as the analogous bodies within the eyeball.

Hence it follows, that the filaments and spherical particles, whose diffracted shadows have four or five different sizes, have the same magnitude, and are placed at four or five different distances from the retina; those which give the sharp, black, and minute shadows, being placed near the retina, and those which are large and ill defined at a distance from it. These various bodies, though they change their place, still preserve their general distance from the retina, thus clearly indicating that the vitreous humour is composed of cells within which the filaments and *muscæ* are lodged. That they do not exist in the aqueous humour is very obvious, because if they did, they would either rise to the top or sink to the bottom of the aqueous chamber when the eyeball was at rest, and thus withdraw themselves entirely from the field of view, which they never do.

In order to obtain farther information respecting these *muscæ*, I fixed the eyeball in different positions, and looking at a sheet of white paper, I marked upon it the various positions on the paper where the *Musca* rested. It never withdrew itself from the field of view, and suffered no sensible change in its size; but it rested in positions at different distances from the axis of vision. In one position of the head, I could bring the *musca* into the optic axis so as to obtain the most perfect vision of it, but in all other positions of the head, it rested at a distance from the optic axis; though in these it could, by a toss of the head, be made to cross the axis of vision. In making these experiments, we must recollect that, as the *musca* is generally seen by oblique vision, it will very frequently disappear, though it has not withdrawn itself from the field of view. In all positions of the head, the *musca* appears to descend, so that it must actually *ascend* in the vitreous humour, and be specifically lighter.

Now, it is obvious, that, if we determine the visible position of the *Musca* when at rest in different positions of the head, we determine the direction of lines passing from the centre of visible direction through the points in the vitreous humour where the musca rested, and thus obtain a general notion of the *form of the cell* in which it is contained. But we may go still farther, and determine with considerable accuracy the diameter of the *Musca* or its filaments, and also their distance from the retina, and thus obtain a knowledge of its locality, and of the form of the cavity by which its excursions are limited.

In order to do this, I place before the eye two bright sources of light, so as to obtain from them, by the method already described, two divergent beams of light, and I thus obtain double images on the retina of all objects placed within the eyeball. The filaments or *Muscæ* in the anterior part of the vitreous humour

will have their double images very distant: those in the middle of it will have their double images much nearer: those near the retina will have their two images close or perhaps overlapping each other; while any object on the retina itself, any black spot arising from defective sensibility, will have only one image, as it were. Now, if we measure the distance of the two sources of light from each other, and also their distance from the centre of visible direction, when the two images of the filaments, &c., are just in contact, we may determine the size of the filament and its exact position, as well as its distance from the retina. In making this experiment, I first found that the angle of apparent magnitude of the shadow of the filament ABC was eight minutes, and consequently that it subtended this angle at the centre of visible direction.* Now, if we take the radius of the retina as 0.524 of an inch, the diameter of the shadow of the filament will be 0.0122, or $\frac{1}{820}$th of an inch, and its distance from the retina 0.018, or $\frac{1}{85}$th part of an inch.

When we use a small aperture alone for producing a divergent pencil, the centre of divergency must necessarily be without the eyeball; but we may throw the centre of divergency within the eyeball, and place it at any distance from the retina, by using a lens of the proper focus. If we wish to place this centre near the retina, a lens of considerable focal length must be used, and as the light collected by it will be powerful, it will extinguish all the smaller filaments and minute spheres, and allow only the larger *Muscæ* to be seen. We must therefore reduce its aperture by looking through a pin-hole or other minute opening. When we wish to have a clear field of view for examining the larger *Muscæ*, we may extinguish all the smaller ones by increasing the luminosity of the field. If we wish to study the filaments or *Muscæ* that may be placed about the middle of the vitreous humour, we must use a lens of such an aperture as will obliterate all those more remote from the retina.

It is very obvious, from the preceding observations that objects placed within the eyeball are not seen, as Dr PORTERFIELD believes, by rays *which pass* THROUGH *dense particles having suffered a greater refraction than those which pass* BY *them*. A fibre or particle of glass of nearly the same refractive power as the vitreous humour will be seen distinctly by means of its image formed on the retina by diffracted pencils. If the light is not sufficiently divergent, or is too intense to produce and exhibit the diffracted image, the object will be invisible, unless it be of such a size, and so near the retina, as to shew itself by its ordinary shadow. But in whatever way the image of the object is formed, the mind takes cognizance of it, or gives it an external locality, by means of the same law of visible direction which regulates the vision of objects placed without the eyeball.

* This may be done by projecting it upon a luminous surface, and marking its apparent size; or by comparing it with the images of objects of known dimensions seen with a fine microscope.

While these results exhibit the true physical cause of all the optical phenomena and limited movements of the filaments and *Muscæ*, they lead also to some important and useful conclusions of a more general nature. It had been conjectured that the vitreous humour of animals was enclosed in separate bags or cells connected with the hyaloid membrane by which the vitreous mass is enveloped. The preceding experiments not only appear to demonstrate that this is the structure of the vitreous humour in man, but to shew that there are at least *four* or *five* cells between the retina and the posterior surface of the crystalline lens. The limited motion of the *Muscæ* indicates that the cell in which they float is of very limited extent. When the vertical diameter of the eyeball, in its natural position, is placed, by the inclination of the head, 30° to the right hand of a vertical line, and the optic axis of the eye directed 20° below a horizontal line, the *Musca* is seen along the optic axis, and consequently in the most perfect manner. One point of its cell must therefore touch the optical axis.

I have endeavoured, with the assistance of my eminent colleague Dr Reid, to discover cells in the vitreous humour of quadrupeds and fishes by the aid of the microscope and other means, but we have not succeeded: and unless some chemical substance shall be found which acts differently upon the albuminous fluid and the membranous septa, it is not likely that they will be otherwise rendered visible.*

Mr Ware, in a paper on the *Muscæ Volitantes of nervous persons*,† describes some as "globules twisted together, and others as like the flue that is swept from bedrooms," and he considers it "probable that they depend on a steady pressure on one or more minute points of the retina which are situated near the axis of vision."‡ In the cases described by Mr Ware, the *Muscæ* were liable to great and sudden changes in intensity and number, particularly from causes affecting the nervous system, and hence they cannot be regarded as of the same character as the *Muscæ* described in this paper, unless we suppose that *Muscæ*, invisible under ordinary circumstances, become visible in consequence of an increased sensibility of the retina.

This supposition, however, is by no means probable, because the *Muscæ* are not visible by any light of their own, and an increase of sensibility in the retina would affect equally the luminous field on which they are seen. But, as this point is of some importance both in a physiological and a medical aspect, I have submitted it to direct experiment. With this view, I examined the *Muscæ*

* The vitreous humour, when slowly dried, either by itself, or along with parts of the septa in which it may be contained, shoots into beautiful crystalline ramifications proceeding from the four angles of a quadrilateral crystal. Thin six-sided plates frequently occur, but they seem to exercise no action upon polarised light, probably on account of their thinness. The same effects were produced when the vitreous humour from a fresh eye was well washed in distilled water.

† Medico-Chirurgical Trans., 1814, vol. v., p. 255. ‡ Id. Id., p. 266.

in the morning before the sensibility of the retina had been diminished by exposure to daylight, and found that they were neither increased in number or intensity. I varied this experiment by diminishing the sensibility of the retina. This was done by holding a bright gas flame close to the eye, and near the axis of vision, till the retina lost its sensibility to all the rays of the spectrum, except a few of the more refrangible ones.* In this case, too, the *Muscæ* were as numerous and distinct as before, and we may therefore consider it as certain, that the *Muscæ* described by Mr Ware, in so far as they were of the same character as those in the healthy eye, are not affected by any variation in the sensibility of the retina. I am disposed to think that they consisted of the ordinary *Muscæ* seen simultaneously with others produced by the pressure of the bloodvessels on the retina, and that it was the latter only which underwent the variations which he describes.

It is not easy to form any rational conjecture respecting the cause and purpose of the numerous filaments by which the *Muscæ* are produced; for as they exist in all eyes, whether young or old, they are neither the result of disease, nor do they indicate its approach. Were they fixed or regularly distributed, we might regard them as transparent vessels which supply the vitreous humour; but existing, as they do, in detached and floating portions, they resemble more the remains of some organic structure whose functions are no longer necessary. But though these filaments have no morbid character, they may nevertheless obstruct and even destroy vision. They certainly interfere with nice microscopical observations, and in observing the minute and almost imperceptible lines in the solar spectrum, I have found them to be occasionally injurious. It is quite possible that some of the cells near the retina and around the optic axis might be filled up with accumulated *Muscæ*, and produce a considerable degree of blindness; but this is an effect of them which there is little occasion to apprehend.

Mr MACKENZIE† informs us, "that few symptoms prove so alarming to persons of a nervous habit or constitution as *Muscæ volitantes*, and they immediately suppose that they are about to lose their sight by cataract or amaurosis." Professor PLATEAU of Ghent, to whom I had communicated, at his own request, some of the preceding results, mentions to me, that few physicians are able to distinguish between the *Muscæ* described above, and those appearances which indicate amaurosis, and that they often, without cause, alarm patients who consult them for the first time respecting such affections of the eye. He assures me that he has already been the means of freeing from alarm many persons with *Muscæ volitantes*, and that he had even done this to a distinguished physician.‡

* Lond. and Edin. Phil. Mag., 1832, vol. i., p. 172, vol. ii., p. 168.

† Practical Treatise, &c., p. 751.

‡ Professor PLATEAU mentions that he had been led to suppose that the *Muscæ* had their seat in

The details in the preceding pages may, therefore, be considered as establishing the important fact, that *Muscæ volitantes* have no connection whatever either with *Cataract* or *Amaurosis*, and that they are nearly altogether harmless. This result has been deduced by the aid of a recondite property of divergent light, which has only been developed in our own day, and which seems to have no bearing whatever of an utilitarian character. And this is but one of numerous proofs which the progress of knowledge is daily accumulating, that the most abstract and apparently transcendental truths in physical science will sooner or later add their tribute to supply human wants, and alleviate human sufferings. Nor has science performed one of the least important of her functions when she enables us, either in our own case, or in that of others, to dispel those anxieties and fears which are the necessary offspring of ignorance and error.

St Leonard's College, St Andrews,
March 4. 1843.

the vitreous humour rather than in the aqueous; but that he had been stopped by the difficulty of reconciling this opinion with the viscosity of the vitreous humour. As the vitreous humour is *perfectly fluid* within each cell, the viscosity here supposed being only apparent, no longer presents any difficulty.

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XXVII.—*On the Specific Gravity of certain Substances commonly considered lighter than Water.* By John Davy, *M.D., F.R.S., L. and E., Inspector-General of Army Hospitals, L.R.*

(Read, 3d April 1843.)

That the common varieties of wood which float in water, owe their apparent lightness to air contained in their structure, is generally admitted by those who have paid any attention to the subject. By means of the air-pump, the fact is clearly demonstrated. Under the exhausted receiver, after a certain time, the time varying with the quality of wood, all the different specimens which I have tried have sunk; I may mention two or three in particular, as examples. A piece of oak, weighing 29.7 grs., sank in distilled water, after having been subjected to the air-pump three days;—a piece of deal, weighing 16.3 grs., similarly acted on, floated ten days;—and a portion of the pith of the elder, weighing only .133 grain, floated seven days. The temperature of the room in which the experiments were made, was about 50° F.; the air-pump was frequently worked in the course of each day, and was in perfect order. After the exhaustion of the air was carried as far as it well could be, the specific gravity of each wood, exclusive of hygrometric moisture,* was found to be as follows: oak, 1.58; deal, 1.18; pith of the elder, 1.45.†

The remark made on the common woods is applicable also to pumice and all vesicular minerals, and admits of proof in the same manner. The specific gravity of pumice is stated to be, according to Brisson, .914; according to Hoffman, .752 and .770; that is, in its ordinary condition, when its cavities are full of air. But, when acted on by the air-pump, I find it is as high as 1.94. The subject of the experiment was a portion of a specimen from Lipari, that weighed 31.8 grs.,

* The oak-wood lost by thorough drying, at a temperature a little below the scorching point (including a small loss from the action of cold water), 18.3 per cent.; the deal, 14.2; and the pith of the elder, 13.3.

† According to Count Rumford (Nicholson's Journal, vol. xxxiv., p. 322), the specific gravity of oak is 15,344; of fir, 14,621, to water as 10,000. He arrived at these results, not by means of the air-pump, but by the expulsion of air by boiling in water. The specific gravity of deal or fir-wood, as given by him, is nearer the truth than that in the text, which is too low, for a reason which will afterwards be assigned. I find, that when air is entirely, or nearly entirely expressed from it by compression in water, that it sinks in a fluid of specific gravity 1.5; and that the pith of the elder, similarly treated, sinks in the same fluid.

The number 1.45, given in the text, as the specific gravity of the pith of the elder, was determined hydrostatically, using a very delicate balance, affected by the one thousandth of a grain, when loaded with 500.

thoroughly dried. It floated on water, in the exhausted receiver, about thirty hours; and continued to give off air—extremely little indeed in quantity—until about the thirtieth day, reckoning from the commencement of the exhaustion.*

There is a small number of other substances generally believed to be lighter than water, respecting which doubt may be entertained, such as cork, caoutchouc, camphor, wax, spermaceti, cholesterine, stearine,—which, like the preceding, may owe their apparent lightness to entangled air.

To endeavour to determine this question, I have made some experiments, the results of which I shall now have the honour of submitting to the Society,—believing the subject to be deserving of some attention, practically considered, especially in connexion with the examination and analysis of certain vegetable and animal compounds, the oily and fatty contents of which are daily becoming more interesting, in connexion with theoretical views respecting elementary cells and their nuclei.

The great buoyancy and apparent extreme lightness of cork is well known: its specific gravity is stated to be as low as .2400.† When subjected to the air-pump, much air is disengaged from it; it subsides a certain way in the water, but does not sink. A portion of cork weighing 12.4 grains, was kept under the exhausted receiver from the 23d of January until the 3d of April, when it continued to float; and even minute portions of this substance, not exceeding one tenth of a grain in weight, appear incapable of being sunk by the action of the air-pump.‡ This, it may be conjectured, is owing to the elastic cellular structure

* Reduced to powder, after having been subjected to the air-pump, and weighed hydrostatically, it was found to be of the specific gravity 2.41, which is about that of obsidian,—the mineral substance from which, it would appear, that pumice is formed by the action of volcanic fire. As no air was disengaged when the pumice was crushed under water, it seems probable, from the circumstance of its specific gravity being increased by its cells having been broken, that some of them may be destitute even of air. This brings to my recollection the result of an experiment made many years ago, on exposing obsidian to a high temperature in a gun-barrel, in which I assisted a distinguished member of this Society, Sir George Mackenzie. The air disengaged from the obsidian had a distinct smell of nitrous acid gas. Now, supposing that this acid is always set free in the production of pumice from obsidian, part of it may be re-absorbed, and tend perhaps, with steam, to form the minute vacua which I have supposed may exist in pumice,—vacua, the existence of which it is easy to imagine, considering the nature of the substance, in reality a vesicular glass, and differing chiefly from obsidian, or, as it has been significantly called, volcanic glass, in its vesicular condition. This is well displayed by the microscope, under which, with a high power, its minute fragments appear as transparent glass, in some of which cavities are distinguishable.

† Henry's Chemistry, vol. ii., p. 506.

‡ Whether cork kept in water unaided by pressure, would ever sink, seems very doubtful; probably it would continue to float so long as the plates constituting its cells retained their integrity and elasticity,—that is, so long as its substance resisted decomposition. The portion of cork, the subject of the experiment described in the text, which weighed in air 12.4 grains, after having been in water, under

of cork offering resistance to the escape of air, when highly rarified, similar to that presented by a strong solution of gum-arabic, or any other viscid fluid acted on by the air-pump, when we see, after the pump has been worked some time, and the exhaustion is as complete as it can well be made, that bubbles rise, on which the farther working of the pump seems to have little effect, and which appear to break rather in consequence of evaporation than of exhaustion. In accordance with this view, when the cells of the cork are forcibly compressed and broken down, then their substance ceases to be buoyant, and the cork sinks readily in water. The effect is easily shewn by compressing and breaking up small portions, as by forcibly crushing them in a mortar under water. I find, after this has been done, that cork sinks not only in water, but also in a saturated solution of common salt, which is of the specific gravity 1.148, and in sulphuric acid of specific gravity 1.5, at which strength the acid has no immediate charring effect. This last result would seem to indicate that its specific gravity exceeds 1.5. If the base of cork be considered as wood (a conclusion I am disposed to adopt, rather than the idea that it is a distinct substance, suberine), it is probable that its specific gravity is as high even as 1.6, which I believe to be that of the matter wood or lignin in its purest form, being, as I find, that of cotton and of linen.*

The specific gravity of caoutchouc, according to Brisson, is .93. I have found it, when its outer black pellicle has been removed, even lower, viz. .91. Under the microscope, using a high power, this specimen appeared to have within its substance minute cavities, which, from the transparency of the mass, were sufficiently distinct. From the properties of caoutchouc, it could not be expected that any air contained in it, in closed cavities, could be extracted, either entirely or even in considerable part, by the air-pump, or by boiling, or by compression. Solution in ether, and precipitation by alcohol and the addition of water purged of air, seemed to afford a probable means of determining the question, whether or not the presence of air in this substance is connected with its lightness.

To a hot saturated solution of caoutchouc in sulphuric ether, alcohol was added; the caoutchouc thrown down, resembling in appearance a mass of fibrin separated from the blood by stirring, was taken up by a forceps and transferred to water that had ceased to give out air under the exhausted receiver, and was immediately acted on by the air-pump. The effect of the removal of the atmospheric pressure on it was remarkable, owing, no doubt, to the ether adhering to its

the exhausted receiver 33 days, had increased in weight, from the absorption of the water, to 20.5 grains; and after 22 days more, its farther increase was only 1 grain.

* I have found the specific gravity of cambric carefully freed from air 1.600; of hemp cord, 1.560; of fine cotton cloth, 1.605; and of cotton thread, 1.61, at 50° F. The cambric and cotton thread were first boiled in distilled water, and then subjected to the air-pump before weighing in water; they were thoroughly dried before being weighed in air, and weighed whilst still warm. The cord and thread were treated in the same manner, excepting that they were not previously boiled.

substance. When the exhaustion was nearly complete, the little mass of caoutchouc was thrown into violent motion, resembling in its movements a piece of potassium on the surface of water, being driven from side to side with strong effervescence or ebullition. Even after many hours, bubbles continued to be disengaged: on their cessation, I found its specific gravity .93. But as, on examining this caoutchouc with the microscope, it also was found to contain minute cavities, which might be filled with the vapour of ether or of alcohol, if not vacua, the specific gravity, as ascertained, was liable to the same objection as in the case first referred to of common caoutchouc.

To endeavour to obviate the interfering circumstances in this experiment, another was made. To an etherial solution not saturated, alcohol in considerable quantity was added, in a manner to prevent the particles of caoutchouc, as they were precipitated, from cohering and forming a mass. After the addition of a portion of water, the mixture was subjected to the air-pump, and was kept under the exhausted receiver till the greater part of the spirit had evaporated. The fluid was turbid from particles of caoutchouc suspended in it. Transferred to a tube and carefully watched, some of them, chiefly the largest, were seen to ascend; others were seen to descend. The specific gravity of the fluid was found to be ·97. This result seems to be in favour of the conclusion that the true specific gravity of caoutchouc differs but little from that of water, as in all probability even the smallest visible particles contained cavities in which might be included the lighter substance either of ether or of alcohol.

The specific gravity of camphor, according to Brisson, is ·9887. Subjected to the air-pump, floating on distilled water, air is disengaged from it, and the mass of camphor gradually sinks in the water; but unless very small, not entirely. On close inspection it may be seen commonly to be buoyed up by minute globules of air, either adhering to it or included in its substance. Now, if the mass be broken in the water and reduced to a coarse powder, and again submitted to the pump, after the exhaustion, many of the little fragments will be seen to have subsided, and some will be seen at the bottom. If, however, the warm hand be applied to the bottom of the vessel, then the bits of camphor will be found to rise and fall with the ascending and descending currents produced in the fluid. The inference from this experiment obviously is, that the specific gravity of camphor exceeds that of water, but only in a very slight degree. Adding salt, I find the particles free from air ascend and descend with the currents, excited by the partial application of heat, when the water has acquired the specific gravity 1·005, at about the temperature 50°; which therefore may be concluded to be about that of the substance itself. And confirmation of this was obtained by dissolving a portion of camphor in alcohol, precipitating by water deprived as much as possible of air, and adding a portion containing particles of the precipitated camphor to the salt and water. Many of them remained stationary below the surface,

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when the temperature of the water was steady, or followed its currents when its temperature was disturbed. Further, it may be remarked, that when the precipitated camphor is thrown into a large quantity of water, even at the temperature 40°, part of it subsides to the bottom. That the whole does not subside is no more than might be expected, considering that the smallest globule of air attached may suffice to render a particle, or congeries of particles, buoyant. Examined with the microscope, the particles that had subsided, or were suspended, appeared to be quite homogeneous, of a globular form, or an approach to that form; none of them were crystallized.

The specific gravity of unbleached wax, according to FABRONI, varies from ·9600 to ·9650, and of white wax from ·8203 to ·9662. Owing to the peculiar properties of this substance—either impervious or little pervious to air in its solid state—the rapid manner in which, when melted, on reduction of temperature it congeals at the surface, and its great degree of contraction on cooling—owing to these properties, the ascertaining of its specific gravity as a solid mass is peculiarly difficult.

In its liquid state, at the boiling temperature of water, I have found the specific gravity of yellow wax to be 0.856, distilled water of the same temperature being considered as 1.000. This is the mean of two experiments; according to one of which it was ·854; according to the other, ·858. At 100°, its specific gravity appeared to be to water of the same temperature as ·952; and at 52°, as ·989. The specific gravity of white wax, at the boiling point of water, was found to be ·861; and at 50°, it appeared to be ·988. The manner of conducting the experiments which gave these results was the following. The melted wax, in the first instance, was poured into a bottle fitted for ascertaining the specific gravity of liquids, immersed in boiling water, and the stopper heated was then introduced into the bottle. Thus filled, the bottle weighed, of course gave the specific gravity of wax at the boiling temperature of water—its weight, filled with boiling water, having been previously determined. For the lower temperatures, the bottle, charged with melted wax, reduced as near to its congealing point as was compatible with its liquidity, was immersed immediately after the introduction of the grooved stopper, into water purged of air by the air-pump, and then allowed to cool previous to weighing. By this method I had hoped to exclude air, and obtain satisfactory results. But the examination of the congealed wax satisfied me that I was mistaken. On slicing the mass of wax, cavities were found in its substance—some of large size, containing water, others of small size, many of them extremely small, requiring the aid of the microscope to be seen distinctly—apparently dry and empty—it may be, they were filled with air. The general appearance, I may remark, called forcibly to recollection the condition of certain minerals and rocks, containing cavities, supposed to be formed during

consolidation from a state of fusion, and the contents of which have been found to be so various, and in their theoretical bearings so instructive.

It appearing impracticable to ascertain the true specific gravity of wax in its solid state in mass, recourse was had to another method. The wax was dissolved in hot alcohol, and then poured into distilled water, deprived of air by the air-pump. The precipitated wax, in a flocculent state, sank in the mixture of water and spirits. It was divided into three portions, to which a solution of common salt was added in different proportions. In the lightest of specific gravity .992, at 50°, the greater part of the flocculi sank to the bottom, or were suspended midway; in another, of specific gravity 1.005, a considerable portion was suspended midway, a little sank; and, in the third of specific gravity 1.014, the greater portion rose to, or towards the surface; a few flakes were suspended, and a very few subsided. These results induce me to believe, that the specific gravity of wax, free from air, exceeds very little that of water; and the results of the trials on the specific gravity of bleached and unbleached wax at the boiling temperature of water, seem to show, that the former is rather the heaviest of the two.

Wax, as is now well known, is composed of two substances, cerine and myricine. I have not examined them apart in regard to their specific gravity;—I have thought it the less necessary, as the latter in bees'-wax is in small proportion, and its specific gravity is stated to be the same as that of water, whilst the specific gravity of cerine is made as low as .969.*

Spermaceti is stated to be of the specific gravity .9433.† In connection with the properties of this substance, analogous to those referred to when treating of wax, there is not less difficulty in determining accurately its specific gravity in mass. At the boiling point of water, I find its specific gravity to be .839; at 100°, which is about 12° below its point of congelation, it appears to be about .910; and at 50°, apparently .96, using the same method as that employed with wax. This last number, as in the parallel instance of wax, and for the same reasons, is, I believe, too low. I find, that when spermaceti is dissolved in hot alcohol, and is precipitated by admixture with water freed of air, that the flocculi thrown down are suspended when a solution of common salt is added in just sufficient quantity to make the specific gravity of the mixture that of distilled water; and which, therefore, probably, apart from entangled air, is the true specific gravity of this substance, and also of cerine, of which the purified crystalline spermaceti, the subject of trial, almost entirely consists.

The specific gravity of cholesterine is commonly considered inferior to that of

* Noveau Système, de Chimie Organique, par F. V. RASPAIL, iii. 406.

† HENRY's Chemistry, 6th edit. ii. 505.

water; but I am not acquainted with any author who states the difference numerically, excepting in the instance of biliary calculi, of that kind which is composed almost entirely of this substance: Gren found the specific gravity of one so low as .803. From the trials, I have made, it would appear, that the specific gravity of cholesterine, in its pure state, is greater than that of water. Crystals formed in alcohol on cooling, well washed with distilled water, are, I find, suspended in a solution of salt of specific gravity 1.0102 at 50°, and which, therefore, may be considered as the specific gravity of the cholesterine itself. The apparent greater lightness of biliary calculi formed of cholesterine, is clearly owing to the air which they contain; and to the same cause must be referred the circumstance, that when crystals of this substance procured by the cooling of an alcoholic solution are thrown into water, most of them float, being buoyed up by the minute air-bubbles disengaged on the admixture of the water and alcohol. If the crystals are minutely examined, none will be seen to float excepting those to which air-globules are adhering,—and some of the larger size will be seen to sink, although each of them may have attached to it a very minute air-globule.*

Stearine, it is stated by M. Raspail, is of the specific gravity .795. From the trials I have made on stearine, obtained from the suet of beef and of mutton by boiling alcohol, I am disposed to infer, that, in its solid state, at ordinary temperatures, its specific gravity differs but little from that of water. Alcohol, containing a sediment of stearine, which had separated and subsided on cooling, after the addition of a portion of water purged of air, was submitted to the air-pump, and was left under the exhausted receiver several hours without agitation. The greater part of the stearine was found suspended midway; a smaller portion had reached the bottom, and a smaller still was at the surface. This admixture of spirit and water was of specific gravity .98. Another mixture, which was turbid throughout, from particles of stearine diffused through it, after rest of many hours under the exhausted receiver, was of specific gravity .991. In this instance, there was a thin stratum at the surface more opaque than the mixture generally, from containing a larger quantity of stearine;—an excess which may have been owing to the entanglement of a little air (for a small quantity was disengaged on exhaustion), or to those particles not being free from oleine; or, it may be, they contained included in them a little alcohol. In favour of this latter conjecture, it may be mentioned, that when stearine, deposited from alcohol, has been a considerable time in water, its specific gravity seems to increase, its particles are carried up and down in the ascending and descending currents of the fluid, or when these

* Since the above was written, I find that Dr J. Lawrence Smith, in a short article on Cholesterine, published in Silliman's Journal for January 1843, has pointed out the common error relative to the specific gravity of this substance, but without endeavouring to determine it exactly. His conclusion was drawn from finding it sink when fused and thrown into water.

are not excited, remain stationary, or almost so, showing only a very slight tendency to ascend.

The result of these experiments admits of some practical applications, and may aid to explain some phenomena of an obscure kind, in certain processes.

Were the substances treated of lighter than water, it might be expected that in every instance, when mixed with water, whether precipitated from an alcoholic, or obtained from an etherial solution, or mechanically detached, as in the operation of boiling, that they would of necessity find their place of rest, and be collected at the surface. But, on the contrary, if their specific gravity is either the same, or in the smallest degree superior to that of water, then the same could not be expected;—all that could be expected would be, that no more of each substance would rise to the surface on admixture with water, than is buoyed up by the adhering particles of air: and no confidence would be placed in the circumstance of specific gravity in an operation of analysis, for collecting the whole of the substance sought. In illustration, cholesterine may be specially mentioned —a substance of common occurrence in animal concretions and morbid depositions; indeed, as I have satisfied myself by recent inquiry, much more common than is generally supposed. If a concretion containing cholesterine be digested in hot alcohol, and the alcoholic solution be precipitated by water, a portion of the cholesterine will rise to the surface, and appear there as a pearly film;* whilst another portion, not rendered buoyant, will subside, and, on careful inspection, will be found at the bottom. Or if the concretion be broken up, and boiled in water,—as cholesterine is not fusible at the boiling temperature of water,—its crystalline plates, on rest, will form a sediment, and may be separated by decantation; or if extremely minute,—and they are sometimes met with not more than $\frac{1}{3000}$th of an inch in width,—they may be suspended for a considerable time, imparting a milky opaqueness to the fluid.

The raising the cream of milk may be mentioned as another instance of the influence of disengaged air. It is well known, that in cold weather, cream rises slowly. Is not this owing chiefly to the milk, at a low temperature, resisting that change to which it is so prone in warm weather,—the fermentation of its saccharine part, and the formation of carbonic acid? Milk, the instant it is drawn from the cow, is, I find, destitute of air: I have been able to obtain none from it, when collected with proper precautions and subjected to the air-pump.†

* Oleine is a frequent accompaniment of cholesterine in animal concretions, and when present, being considerably lighter than water, may be looked for in the film alluded to in the text. Mixed with cholesterine and air, its appearance is very like that of cream on milk.

† Researches, Physiological and Anatomical, vol. ii. p. 221. I find that, when milk fresh from the cow is subjected to the air-pump, a small portion of cream soon collects at the surface; and farther, that it may be kept many days (I have kept it twelve days) without any sensible increase in the quantity of cream, or distinct diminution of the opaque whiteness of the milk,—seeming to indicate, that a part of

But cream, whenever separated, however fresh, is found to abound in air. Were it not for air attached to the cream globules, it seems questionable that any separation of them would take place, as their albuminous envelope (adopting the inference of MÜLLER and HENLE, that they are so provided)* seems to give them a specific gravity about the same as that of the medium in which they are suspended.†

In pharmaceutical processes, in which it is required to suspend powders in fluid mixtures, the efficacy is well known, of subjecting the powder to attrition with a very little water or alcohol. This seems to be owing entirely to the separation of air, either contained in the substance of the powders, or adhering to their surface in minute globules, and is well illustrated by the effect of attrition, as described in a preceding page, on cork.

On the contrary, whenever great lightness and buoyancy are necessary, whether for raising bodies in the atmosphere, or floating them in water, or making them run or press lightly on the earth's surface, air is in some way included. The hollow bones of birds, filled with air—the swimming-bladders of fishes, distended with air—the elastic cellular structure of cork, the rigid cellular and tubular structure of pumice, full of air—or, in the instance of the latter, nearly so filled,—are examples of the kind, and may be deserving of being studied, particularly the last mentioned substances, with wax and caoutchouc, in relation to the principles on which vessels, and implements, and edifices should be made, requiring an unusual degree of buoyancy or lightness, especially if intended for a permanency.

EDINBURGH, 3d *April* 1843.

the cream globules, those which rise, may be without an albuminous envelope, and that another part, those which do not ascend, may be provided with such a membrane.

* Histoire des Tissus et de la Composition Chimique du Corps Humain, par J. HENLE, p. 165.

† Butter, I find, when as pure as it can be rendered by melting, is, at the boiling point of water, of the sp. gr. .902; at 100°, apparently .913; and at 48°, .932,—employing the method used in the instances of wax and spermaceti. The lightness of the substance of butter increasing with its temperature, must necessarily expedite the raising of cream, as when the "scalding" process is employed.

but could, which [illegible] it seems questionable [illegible]
and for air [illegible] to the [illegible] globules [illegible]
[illegible] of them would [illegible]
[illegible]

[illegible]

XXVIII.—*Biographical Notice of the late Sir* Charles Bell, *K.H.*
By Sir John M'Neill, *G.C.B.*

(Read 17th April 1843).

The pleasure which honourable and enlightened minds must feel in acknowledging their obligation to the individuals who have advanced useful knowledge in any department of science,—who have contributed to the means of promoting human happiness, or of alleviating human suffering, has, in all times, led men to seek an opportunity of recording their sentiments of admiration and of gratitude towards the distinguished instructors of mankind. They have felt, too, that the time when one of these guiding lights has been quenched, when a contributor to the treasury of knowledge has just terminated his labours, is peculiarly fitted for the discharge of this duty. The whole amount of his contributions is then presumed to be before them, and they are restrained by no fear of offending his delicacy by their praise, or of having their own feelings hurt by a misconstruction of their motives. They know, that what might have been regarded as adulation of the living, is often admitted to be but justice to the dead.

To this Society, whose express object is the advancement of science,—whose especial care it therefore must be to watch over the reputation of every one to whom science is indebted, and which is not only entitled, but required, to take a leading part in determining the measure of praise that each labourer in the various fields of its own domain may have merited, no apology can be necessary for laying before it a short sketch of the late Sir Charles Bell's claims to be ranked high amongst the men who have established a title to its admiration. But I may perhaps owe it to you, as well as to myself, to say, that having been so long a stranger to the subjects with which I shall chiefly have to deal, I should not have ventured to undertake this task, had I not been led to set aside all such considerations by a desire to comply with the wishes of persons whose sentiments are at all times, and especially on this occasion, entitled to respect and deference from me. At the same time, I did not doubt but that I should experience your indulgence while I endeavoured to do what I have thought it my duty to attempt.

Sir Charles Bell, the youngest son of the Rev. William Bell, a clergyman of the Episcopal Church of Scotland, was born at Edinburgh, in the month of November 1774. Having studied at the High School and the University of this city, he devoted himself, at an early age, to the medical profession, and especially to the study of anatomy, under his brother, the late Mr John Bell, who was twelve years older, and who had already laid the foundation of his fame as an anatomist and as a surgeon. But Mr John Bell was not merely an anatomist

and a surgeon second to none in his time; he was a man of enlarged mind, of extensive acquirements, of elegant accomplishments, and of refined taste; and those who remember his powers of conversation, and the keenness of his wit, will probably acknowledge that they have rarely seen them surpassed. If in the later period of his life he was so unfortunate as to have "fallen on evil days and evil tongues," we can only lament that prudence and discretion should not always accompany genius such as his.

Under the guidance of this enlightened teacher, CHARLES BELL soon began to give evidence of the talents which seem to have been inherited by every member of his family. JOHN BELL found in his younger brother a distinguished pupil, an able coadjutor, and then a worthy rival in the race of usefulness and of fame. In the preface to the third edition of his work on the Nervous System, Sir CHARLES acknowledges how greatly he was indebted to his first instructor. "The author," he says, "began his public labours as an assistant lecturer to his brother JOHN BELL, who gave up to him that part of the course of anatomy which treats of the nerves, and he advised him to demonstrate the relations of the brain to the base and spinal marrow, instead of cutting it into horizontal sections. The intelligent student will at once perceive, that much of what is contained in this volume may be traced to the aspect in which the author was accustomed, during all his after labours, to look upon the relations of the brain to the rest of the nervous system."

While yet a pupil, Sir CHARLES BELL had published the first volume of his System of Dissections, illustrated by engravings from his own drawings,—a work which exhibited some originality, and which was regarded as a valuable guide to the student of practical anatomy. On the 1st of August 1799, he was admitted a member of the College of Surgeons, and his admission to that body brought him at once into a situation which tested his practical proficiency and skill; for the whole surgeons of Edinburgh were then, in rotation, Surgeons of the Royal Infirmary. His knowledge of anatomy, and the admirable use of his hands, exhibited both in his dissections and in his drawings, were already conspicuous; and in the hospital, he distinguished himself by the dexterity and the simplicity of his operations. He also eagerly availed himself of the opportunities which his attendance there afforded him, to improve his knowledge of pathology; and having now been associated with Mr JOHN BELL in his lectures on anatomy and surgery, he was assiduous in making preparations, drawings, and models, for the use of the class, from the dissections at the hospital. He even invented a method of representing morbid parts in models, of which some specimens were long afterwards purchased by the Royal College of Surgeons, in whose museum they are still preserved.

But a controversy arose respecting the arrangement of medical attendance in the Infirmary. This contest, which was carried on with great ardour, some

wit, and much asperity on both sides, by the late distinguished and respected Dr Gregory and Mr John Bell, ended in a new arrangement, which excluded many of the surgeons from the only hospital within their reach. Sir Charles Bell happened to be of this number; and so highly did he prize the advantages he had lost, that in a printed memorial, presented to the Managers of the Infirmary, he offered to pay L.100 a-year, and to transfer to them, for the use of the students, the Museum he had collected, on condition that he should be "allowed to stand by the bodies when dissected in the theatre of the Infirmary, and to make notes and drawings of the diseased appearances."

This proposal, which was made in October 1804, was rejected; and perceiving that he had so many difficulties to contend with in Edinburgh, he went in the course of the following year to London, to inquire into the expediency of removing thither. The prospect there could not have been very encouraging; but he had relinquished all hope of being able to surmount the numerous impediments which stood in his way here; and in 1806 he went to the capital.

It is impossible not to admire the courage with which Sir Charles Bell, then a solitary and unsupported stranger in London, trusted to his own resources for success in a field which was already occupied by Cline, Abernethy, Cooper, and other eminent surgeons, supported by the great hospitals with which they were connected, and then lecturing daily to large audiences. To have failed in such an enterprize would have been no disgrace; but to have succeeded and to have established a high reputation as a teacher, in a department of science so preoccupied, is unquestionable evidence of the highest merit.

He immediately commenced a course of lectures on anatomy and surgery, and rapidly rose to distinction. "In the lecture-room," says one of his able successors in the Middlesex Hospital, "in the lecture-room he shone almost without a rival. His views were nearly always solid,—they were always ingenious,—and his manner and language enchained the attention of his audience. Dull indeed must have been the pupil who could have slumbered when Charles Bell was in the professorial chair. In short, Sir Charles Bell made his pupils think; and interesting as anatomy is, even if considered as a mere branch of natural history, he taught them to value it most of all as a guide to the art of healing." *

Previous to his departure from Edinburgh, he had written his work on the "Anatomy of Expression," which was published shortly after his arrival in London, and immediately attracted public attention. He had felt as a physiologist, as an artist, the want of some philosophical and systematic explanation of the rationale of expression; of those muscular movements which are the natural external indications of the passions and emotions of the mind. He had observed that many works of art, otherwise excellent, exhibited anatomical inconsistencies,

* Arnot's Hunterian Oration, 1843.

which he attributed to the want of some competent guide to a knowledge of the principles on which these movements are regulated; and, perhaps, no other man was so well qualified, by his profound knowledge of anatomy, and his practical acquaintance with art, to supply the want. But he did not confine himself to the illustration of what was useful to the artist; he also explained how an acquaintance with the anatomy of expression might be available to the surgeon or to the physician, in distinguishing the nature or the extent of some important diseases.

Independent of its intrinsic merit, this work has another interest, for there is reason to suspect that his inquiries into the functions of the nerves in connection with the anatomy of expression led him to prosecute those investigations which terminated in the most remarkable anatomical discovery of our times.

But before attempting to give an account of Sir Charles Bell's discovery of the different functions of the nerves, corresponding with their relations to different portions of the brain, I must beg your indulgence while I state shortly the opinions upon this subject, which were taught in anatomical schools prior to the announcement of his views. This is the more necessary, because these views have now been so generally adopted, both in Great Britain and on the Continent, that we are apt to forget what the previous state of our knowledge really was. And I may perhaps be permitted to make a few preliminary observations, not immediately connected with the subject, but which may serve to make it more intelligible to such of you as may not have attended to the history of anatomy, and which may also assist us in appreciating the comparative value of the truths which Sir Charles was the first to explain.

In the higher classes of animals, there are three great ramified systems which are distributed to every part of the body. The arteries and veins; the lacteals and lymphatics; and the nerves. It is little more than two centuries since we have obtained a tolerably accurate knowledge of the true functions of any of these systems. The earliest anatomists believed, that the arteries in their healthy state contained nothing but air, as the name which they still retain denotes; and the veins were then believed to be the only blood-vessels. In the second century of our era, Galen is said to have discovered that the arteries also were blood-vessels; but it was still believed, that there was a flux and reflux of the blood in the arteries and veins,—that the blood which flowed through these vessels from the heart or the liver to the extremities, flowed back through the same vessels to the heart and the liver; and various theories were devised to reconcile this belief, with the natural phenomena which presented themselves. At length in 1628, Harvey set the question at rest, by publishing his discovery of the circulation of the blood, propelled through the arteries to the extreme parts, and returning through the veins, in two great circles from the right and the left cavities of the heart.

The more obscure vessels called lacteals and lymphatics, altogether eluded the observation of ancient anatomists. The existence of the lacteals was discovered accidentally, and their functions were partly conjectured by ASELIUS of Pavia, a cotemporary of HARVEY; and their office, that of conveying the nutritive part of the food from the intestines to mingle with the blood, and thus to be distributed to all parts of the body, was demonstrated by PECQUET, a French anatomist, who had also the candour to acknowledge that his discovery was accidental. The lymphatics were shortly afterwards discovered by RUDBECK and by BARTOLINE, the one a Swede, the other a Dane, who shrewdly suspected what their functions were; and the subject was further illustrated, and the functions of these vessels fully explained, by the late Mr HUNTER and the late Dr MONRO, who proved them to be absorbents, that is, the vessels by which the waste of the body, which the lacteals supplied new matter to replace, was carried off.

It is worthy of remark, that both these offices had been assigned to the veins, which, as we have seen, were also, at one time, regarded as the only blood-vessels; and although the manner in which the work of absorption is divided between the lymphatics and the veins is still somewhat obscure, yet the constant result of these successive discoveries has been to shew, that the function of each portion of these vessels is simpler than it had been supposed to be; and that nature perfects the performance of the animal functions, by multiplying the organs and simplifying the duties of each, rather than by simplifying the general structure, and complicating the functions of its parts; and we shall find that the nerves afford a further illustration of this principle.*

The nerves had been noticed from the earliest times, and their functions were long known to be to transmit the mandates of the will from the brain, which has always been regarded as the sensorium, to all the parts which are under the control of the will; and to communicate to the sensorium, intelligence of the condition of their own extremities, which we call sensation. They were divided by anatomists into cranial and spinal or vertebral nerves, with reference to their origin from the brain or the spinal marrow.

In the same manner as it had been taught before the discoveries of HARVEY, that there was a flux and reflux of the blood in the arteries and the veins; that it flowed "backwards and forwards like the tide of Euripus;" so it was taught in our own days, that the same nerves transmitted the mandate of the will from the sensorium to the organs of voluntary motion, and likewise carried to the senso-

* Another general fact, which seems to be well ascertained, may be referred to the operation of the same principle, and, in this respect, has also some analogy to the great discovery of Sir CHARLES BELL in regard to the nerves, viz., that different portions of the small arteries, which are similar in size, structure, and degree of subdivision, have nevertheless very different relations to the blood which they carry, and suffer very different portions of that blood to transude through their coats, so as to maintain the functions of secretion and nutrition; thus affording another instance of the natural subdivision of labour.

rium intelligence of the condition of their extremities, or sensation. It was taught that, in some mysterious manner which no one could explain, these two impulses might be simultaneously communicated along the same cord, in opposite directions, without impairing the efficiency of either. This proposition was certainly startling; but so long as each spinal or vertebral nerve was regarded as a simple cord, composed of one bundle of similar filaments, the inference was inevitable; for if we divide the trunk of one of these nerves, at any point, we leave unimpaired the power of motion, and the sensation of the parts which intervene between the point of section and the brain; but we paralyze at once both motion and sensation in the parts over which its remoter ramifications are distributed. The cord thus divided was, therefore, necessarily and truly inferred to be the channel through which volition acted to move the muscles, and through which sensation was communicated from other parts of the body to the sensorium.

It is nevertheless true, that physiologists had not been uniformly satisfied with this theory. The fact that a limb, which had lost the power of voluntary motion, often retained sensation, had led some discerning men, at an early time, to question whether there might not be different nerves for motion and for sensation. GALEN asserted this opinion in a part of his writings; but he elsewhere maintains that one nerve may minister to both offices; that motion is active, and sensation passive; and that a nerve may retain this passive power after it has lost that which is active.

BOERHAAVE, following GALEN, asserted that there were two kinds of spinal nerves—the one serving for motion, the other for the use of the senses. Speaking of the spinal marrow, he uses these remarkable expressions: "Ex hac medulla exit duplex genus nervorum, unum motui, alterum sensuum inserviens, nec unquam inter se communicans;" and then he adds the inquiry, "Quis dicet hic, hoc movet hoc sentit?" This was certainly a striking and ingenious speculation; but BOERHAAVE did nothing towards solving the question he had put, or the doubts he seemed desirous to raise; accordingly, these speculations produced no change in the opinions of anatomists and physiologists, and the old theory not only maintained its ground, but appeared to be confirmed by further investigations.

The renowned HALLER, who carefully investigated this subject, and who must have been well acquainted with the writings both of GALEN and of BOERHAAVE, rejects a theory which neither of these distinguished authors had supported by any evidence, and which they had not even uniformly maintained. "But I know not," says HALLER, "a nerve which has sensation without also producing motion; the nerve which gives feeling to the finger, is also that which moves the muscles; and the fifth nerve of the brain branches to the papillæ of the tongue, and also to the muscles."

DR ALEXANDER MONRO maintained similar opinions; and he combated the

theory that ganglia were for the purpose of cutting off sensation, on the express ground, that they were to be found on the posterior half of all the spinal nerves of the voluntary muscles; thus shewing that, to be a nerve of voluntary motion, was by him regarded as conclusive evidence that it must also be a nerve of sensation, and that he believed all those spinal nerves which passed through ganglia to be motor nerves. On this Sir Charles Bell justly remarks, "If I had ascertained nothing more than that no motor nerve passes through a ganglion, the observation would have been important towards the true doctrine of the nerves."

Bichat (a distinguished name in modern anatomy and physiology) distinctly asserts that there are not nervous cords appropriated to sensation, and others to motion.

Baron Cuvier maintained, that the difference in the functions of the nerves depends rather on the different organization of the parts to which they are distributed, than on any essential difference between themselves;* and M. Serres, in his work on Comparative Anatomy, published as late as 1824, quotes with approbation this opinion of Cuvier's, even maintaining, in conformity with it, that in certain animals a part of the fifth nerve answers the purpose of the optic nerve; and without making allusion to Sir Charles Bell's experiments and observations. But he admits, at the same time, that it is doubtful whether these animals are really endowed with the sense of sight.

Dr Barclay of Edinburgh, a learned man and an eminent anatomist, who communicated the history of Anatomy to the Edinburgh Encyclopædia published in 1810 or 1811, not only makes no allusion to any discovery of the varied functions of the nerves, but, having related the discovery of the lymphatics, and described their functions, referring to the conflicting claims of Hunter and of Monro, he expressly tell us, that this system of absorbents is the last great and leading discovery made in anatomy by means of dissection.

In short, that which has already been stated to have been the doctrine of the Anatomical Schools, viz. that the same nerves ministered at once to motion and sensation, that the impulses of volition and of sensation were transmitted back-

* "On pourrait penser d'apres cela qu'au fond toutes les parties du systême nerveux sont homogènes et susceptibles d'un certain nombre de fonctions semblables, à peu près comme les fragmens d'un grand aimant que l'on brise deviennent chacun un aimant plus petit, qui a ses pôles et son courant; et que ce sont des circonstances accessoires seulement, et la complication des fonctions que ces parties ont à remplir dans les animaux tres élevés, qui rendent leur concours nécessaire, et qui font que chacune d'elles a une destination particulière.—Il paroit, en effet, quant à ce dernier point, que si certains nerfs ne nous procurent que des sensations déterminées, et que si d'autres ne remplissent également que des fonctions particulières, cela est dû à la nature des organes exterieurs dans lesquels les premiérs se terminent, et à la quantité de vaisseaux sanguins que reçoivent les autres, à leurs divisions, à leurs reunions, en un mot, à toute sorte de circonstances accessoires, plutôt qu'à leur nature intime."—*Leçons d' Anatomie Comparée de Cuvier*, tom. ii., p. 95.

wards and forwards along the same cord, continued to be taught, or was left to be inferred, by all the teachers of Europe, for at least a year after Sir CHARLES BELL had announced to his friends his ideas on the nervous system.

To the genius and to the patient and laborious investigations of Sir CHARLES BELL we owe the discovery, that no one nerve serves the double purpose of ministering to motion and to sensation;—that the spinal nerves and the fifth nerve of the brain, which had been regarded each as one nerve, consisted each of two distinct nerves, connected with different portions of the brain, enclosed in one sheath for the convenience of distribution, but performing different functions in the animal economy, corresponding with the different portions or tracts of the brain to which they could be traced; the one conveying the mandates of the will to the muscles of voluntary motion *from* the sensorium, the other conveying *to* the sensorium intelligence of the condition of distant parts, or sensation. That, to use the illustration I have already employed, as the arteries carry the blood *from* the heart and the veins carry it *to* the heart, so one set of nerves carry the impulses of volition *from* the brain, and another set of nerves carry the impulses of sensation *to* the brain;—that the brain is divided, together with the spinal marrow which is prolonged from it, into separate parts, ministering to the distinct functions of motion and sensation;—and that the origin of the nerves, from one or other of these sources, seems to endow them with the particular property of the division whence they spring. Such were the leading features of BELL's great discovery, one of the most remarkable that the history of anatomy will now have to record.

Let us not forget that the steps by which human knowledge has advanced have at all times been short and slow. It has rarely or never been permitted to the same mind to originate the idea, and to perfect the development of any of the great truths of nature. The greatest discoveries in science have either been dimly seen at a distance and imperfectly shadowed forth, or conjectured as matters of speculation, or the minor truths on which they are founded have been divulged by those who went before, but who failed to arrive at the conclusion which opens up to our view what till then had been hidden, and which expounds to us one of the great laws of nature. But it is to him who, pressing on in advance of his fellows, takes this last and greatest step, and establishes the truth on a sure foundation, making it practically available to other men,—a permanent contribution to human knowledge, and a fresh illustration of the perfection of created things,—that we justly attribute the glory of a discovery; and to that glory Sir CHARLES BELL is justly entitled.

The circulation of the blood through the lungs was known to GALEN and to many of his successors; and it was demonstrated by COLUMBUS, the disciple of VESALIUS. CÆSALPINUS not only knew the circulation through the lungs, but he also discovered that there was a communication between the extreme branches of

the arteries and the veins in other parts of the body; and FABRICIUS pointed out the valves in the veins, which prevent the reflux of blood in these vessels; yet they did not deduce from these facts the theory of the circulation, though, now that it is known, we wonder how they could have failed to discover it. But in the progress of knowledge, the mind has much to unlearn as well as much to acquire; and when our opinions have been sanctioned by the concurrent belief of successive generations, the former is often the more difficult task of the two. When HARVEY announced his great discovery, almost every physician of his time denied its truth, and none of them who were above forty years of age ever, it is said, admitted it. When its truth could no longer be disputed, efforts were made to deprive its author of the merit and the glory of the discovery. Some searched the works of previous writers for evidence that it had been known before his time; and others who followed him, sought to appropriate the honour that belonged only to him. Somewhat similar was the reception Sir CHARLES BELL'S discovery encountered on its first announcement to the world in 1811 and 1821. But as the name of HARVEY is inseparably connected with the great truths which he was the first to ascertain, so will the name of BELL for ever be united in the records of science with his discovery of the varied functions of the nerves.

Insulated facts and unsupported speculations are forgotten and lost, but great discoveries never perish; for they become fixed and established portions of knowledge on which the mind reposes in security. Their leading facts become familiar to all educated men—a part of every man's ordinary information; and the light with which genius illuminated the high places of science is not only shed on the paths which lead up to them, but pierces far into the darkness beyond, and lights on successive generations in their ascent to the loftier heights of a more exalted knowledge.

Confidence in the perfection of the works of creation, and a conviction that the nervous system appeared to be utter confusion, only because of our own ignorance, was BELL'S leading principle in all his investigations; and to this confidence we must attribute the unwearied perseverance with which he prosecuted his enquiries, without any other support or encouragement during so many years of his life.

It would detain you too long were I to trace, step by step, the progress of these inquiries, till he caught a glimpse of the truth in 1807;—but the letter in which, with joy and exultation, he communicated the intelligence to his brother, Professor GEORGE JOSEPH BELL, is too remarkable to be omitted, although it has already been made public; and as it bears the post-mark of London, December 5, and Edinburgh, December 8, 1807, it puts an end to all question, if there ever could have been a reasonable question, as to the originality of his views, and the priority of his discoveries.

"My anatomy of the brain is a thing that occupies my head almost entirely. I hinted to you formerly that I was 'burning,' or on the eve of a grand discovery. I consider the organs of the outward senses as forming a distinct class of nerves from the others. I trace them to corresponding parts of the brain totally distinct from the origin of the others. My object is not to publish this, but to lecture it, * * * as it is really the only new thing that has appeared in anatomy since the days of HUNTER; and, if I make it out, as interesting as the circulation, or the doctrine of absorption. But I must still have time. Now is the end of a week, and I shall be at it again."

In another letter, bearing the post-marks March 28 and 31, 1808, is the following passage:—"I have been thinking of having a room five or six miles from town, and pursuing there my physiology of the brain—*that which is to make me, I am convinced.*"

Others have followed in the same track, and walking by the lights which he had furnished, and in the path which he had pointed out, have advanced our knowledge and confirmed the truth of his opinions. Amongst these, his relative pupil, and coadjutor, Mr JOHN SHAW, has been conspicuous; and to him Sir CHARLES BELL was indebted for some important experiments. Mr HERBERT MAYO, another of his pupils, has prosecuted similar inquiries. In France, in Italy, and in Germany, the method of investigation first employed by Sir CHARLES BELL to determine the functions of the nerves, by attending to their roots, and not to their trunks, has been followed by MAJENDIE, LONJET, BELLINGERI, and the most distinguished physiologists of those countries. They have instituted experiments in imitation of Sir CHARLES BELL'S; and the practical precepts which were first deduced from his discoveries, by himself and by Mr JOHN SHAW, have thus been extended and multiplied.

Mr ARNOT, of the Middlesex Hospital, has stated with so much discrimination and distinctness the precise nature of Sir CHARLES BELL'S discoveries in the physiology of the nerves, that I shall take the liberty of concluding my observations on this part of the subject in his words. After acknowledging whatever he thought incomplete or imperfect in BELL'S writings on the Nervous System, and especially that his views in respect to certain nerves being superadded in the higher animals, for the purposes of respiration, had not been fully proven, he goes on to say—

"But after all these acknowledgments, there remains to BELL, clearly and unequivocally, the merit of having first shewn—

"That in investigating the functions of the nervous system, we must direct our attention to the roots and not to the trunks of the nerves.

"That the nervous trunks, conveying motion and sensation, consist of two distinct sets of filaments in the same sheath.

"That the filaments for motion form a distinct root from those for sensation,

and that the anterior roots are for motion; leaving it to be inferred that the posterior are for sensation.

"That the portio dura is a nerve of motion, and the fifth a nerve of motion and sensation.

"And, lastly, of having been the first who, dissatisfied with the observation and study of the mere form of the various parts of the nervous system, applied the method of experiment to aid him in determining their functions.

"In a word, there belongs to BELL the great discovery,—the greatest in the physiology of the nervous system for twenty centuries,—that distinct portions of that system are appropriated to the exercise of different functions."

The Royal Society of London acknowledged his merit by assigning to him, in the year 1839, the first annual medal of that year, given by his Majesty GEORGE IV. for discoveries in science; and when a new order of knighthood for men of science and literature was instituted, on the accession of the late King to the throne, Sir CHARLES BELL was amongst the first who were invested. But this was the only public reward he received for his labours,—a reward which he would have merited for the services he rendered to the wounded after the battles of Corunna and Waterloo, if he had never rendered any other either to his country or mankind.

In 1812, he was appointed Surgeon to the Middlesex Hospital, and a few years afterwards Professor of Anatomy, Physiology, and Surgery to the College of Surgeons of London. In the hall of that noble institution he delivered a course of lectures which was attended by a very numerous audience, including men of high professional and literary reputation. On the institution of the London University College, he was solicited to place himself at the head of the medical department,—an office which he afterwards resigned, in consequence of dissensions which arose in the establishment. In 1836 he was appointed to the Chair of Surgery in our own University.

It is not my intention to say more of his various writings on the practice of different branches of his profession, than that they place him in the highest class amongst our writers on surgery.

But there is another series of his works which must interest every reader, and which, of all his labours, were perhaps the most congenial to his feelings, and afforded him the greatest pleasure.

In his treatise on Animal Mechanics, written at the desire of the Society for Diffusing Useful Knowledge, he embodied the substance of some of his lectures, which had been so much admired in the College of Surgeons, on the evidences of creative design to be found in the anatomy of the human body. These views had long been deeply impressed upon his mind, and the manner in which he illustrated them probably pointed him out to the executors of the late Earl of BRIDGEWATER as

a fit person to maintain the great argument which it was the purpose of that nobleman's bequest to have published. The part which Sir CHARLES himself selected was "The Hand," that which seemed chiefly to have been in the mind of the testator; and we all know how admirably he executed the task.

Still following out this favourite subject of his contemplation, he associated himself with Lord BROUGHAM in the illustration of Dr PALEY'S Natural Theology, published in 1836; and every one who has looked into that publication must acknowledge the high additional interest which these illustrations derive from his delightful contributions.

Of his private character this may not be the place to speak; but the highest eminence in science receives so great an additional lustre from being associated with the most amiable and estimable social virtues, that it would be unjust not to remind you how largely he was endowed with these. It was in the exercise and the indulgence of the friendship and the affection of social and domestic life, and in the contemplation of still higher objects, that he found the reward of his labours and a solace in his difficulties and disappointments; and if he was but ill requited by his country, for devoting his life so successfully to the advancement of science, instead of employing it, as he might have done with equal success, in improving his own circumstances, he enjoyed while he lived a happiness which wealth alone could not have bestowed, in the devoted attachment of one who was in every way worthy of the undivided affection with which he regarded her.

After a cheerful and peaceful day of calm contemplation and tranquil enjoyment, he was suddenly seized with a spasmodic affection duringt he night, and died, after an hour's illness, on the 29th of April 1842, at Hollow Park, in Worcestershire; and if he left behind him none of the wealth which a more sordid mind might, with his genius, have accumulated, he left an enduring and unsullied reputation, of which the most ambitious of his surviving friends may well be proud, and with which the most virtuous must be more than satisfied.

XXIX.—*On the Determination of Heights by the Boiling Point of Water.* By James D. Forbes, *Esq., F.R.S., Sec. R.S. Ed., Inst. Reg. Sc. Paris. Corresp., and Professor of Natural Philosophy in the University of Edinburgh.*

(Read 6th March 1843.)

It was observed by Fahrenheit, that the boiling point of water depends on the height of the barometer, the pressure of the air hindering the conversion of water into steam by a resistance which must be overcome by an increase of heat. Deluc* and De Saussure† contrived apparatuses for making the observation in the open air, and at great heights, and appear to have contemplated the substitution of the thermometer for the barometer upon occasion. They, as well as Dr Horsley,‡ Sir George Schuckburgh,§ and Mr Cavendish,‖ seem to have regarded the question as one which concerned the fixity of the point used in graduating thermometers, and its requisite corrections, rather than as applicable to barometric purposes generally. Several of them have given empirical tables for correcting the boiling point within the limits of the usual barometric variations, but one only, M. Deluc, has given a formula for connecting the indications of the barometer with the boiling point of water throughout the range which the barometer has been observed to vary on the earth's surface. This is the *only* formula immediately deduced from direct observations of the boiling point; and having been verified by De Saussure at a height greater than the limits for which it was constructed, and having elsewhere been declared by him to be so accurate as to supersede farther experiment on the subject, it might have been expected to be generally adopted, or at least known. We find, however, that though it has been occasionally copied into the formal articles of Encyclopædias, as a correction in graduating thermometers, observers who have used the boiling point for the determination of heights, have always preferred the ordinary tables which give the *elasticity of steam* in terms of its temperature, determined from experiments of quite a different kind from the boiling of water.

Dr Dalton, indeed, has given a table from observation under the air-pump of the boiling point,¶ and that table shews a manifest deviation from the elasticities and temperatures of vapour determined by himself, and now generally accepted as the most accurate below 212°. In boiling, the temperature requires

* Modifications de l'Atmosphere, tom. ii.

† Voyages, § 1275, 2011.

‡ Phil. Trans. vol. lxiv.

§ Ibid. vol. lxix.

‖ Ibid. vol. lxvii. p. 816.

¶ Meteorological Essays, 2d edit. p. 127.

to be *higher*, under a given pressure, than the temperature of steam which has the same tension. Thus, comparing DALTON'S two tables—

Temperature.	Pressure under which Water boils.	Tension of Vapour.	Difference.
212	30·0	30·0	0·00
200	22·8	23.64	+ 0·84
190	18·6	19·00	+ 0·40
180	15·2	15·15	— 0·05

it is exactly at the part of the scale where the difference is most practically important that it is most conspicuous, namely, between 190° and 212°. The method of observation used by Dr DALTON, does not admit of any great accuracy in observing the boiling points, and the numbers he has given are evidently only approximate. Still, from observations made under *naturally* low pressures (the only ones worthy of much confidence in this case), I have found the same nonconformity of the theoretical tension of steam and the atmospheric pressure.

In 1817, Archdeacon WOLLASTON described a thermometer destined particularly for the purpose of determining heights.* But he seems not to have been aware of the progress which the subject had already made in the hands of DELUC and DE SAUSSURE. The latter used a thermometer indicating $\frac{1}{1000}$ of a degree of REAUMUR. WOLLASTON'S instrument, though a neat laboratory one, has almost every fault which a travelling instrument can have, excepting only its small dimensions, to which everything is sacrificed. It is apt to break, and still more apt to be deranged, the contrivance for extending the scale being excessively incommodious; finally, it is impossible to use it in windy weather, and its indications are in an arbitrary scale. No was the method of calculating the heights more happy. At first he contented himself with assuming the progression of height to be proportional to the fall of the boiling point, near 212°;† but he afterwards‡ extended his calculation from Dr URE'S table of tensions of vapour, expressly stating, that he had used the proportionality of 1° of FAHRENHEIT to 0.589 inches of the barometer, or 530 feet, merely as an approximation for small heights.

A reference in BOUÉ'S *Guide du Geologue Voyageur*, directed me to a paper by Mr PRINSEP, in the Journal of the Asiatic Society of Bengal for April 1833. I hoped there to have found a table of boiling temperatures observed at great heights in India. But it only contains a modification of TREDGOLD'S Formula of

* Phil. Trans. vol. cxx. p. 183. † Ibid. p. 192. ‡ Ibid. vol. cx. p. 295.

Fig. 1.

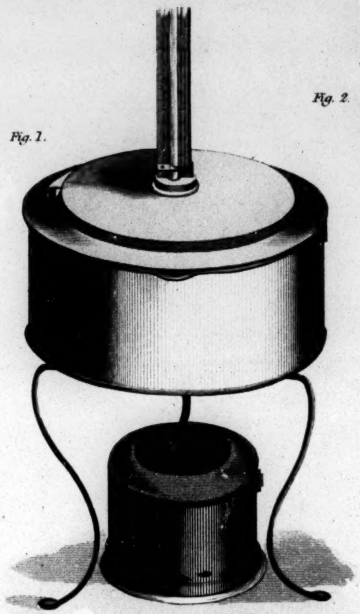


Fig. 2.

Fig. 6.

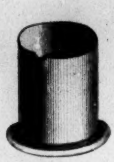


Fig. 9.

Fig. 8.

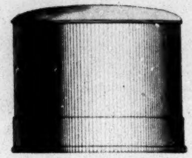


Fig. 5.

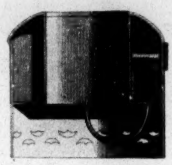


Fig. 7.

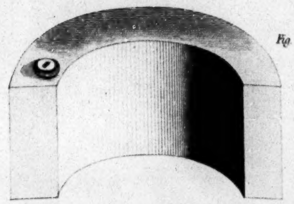


Fig. 4.

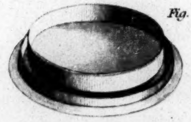
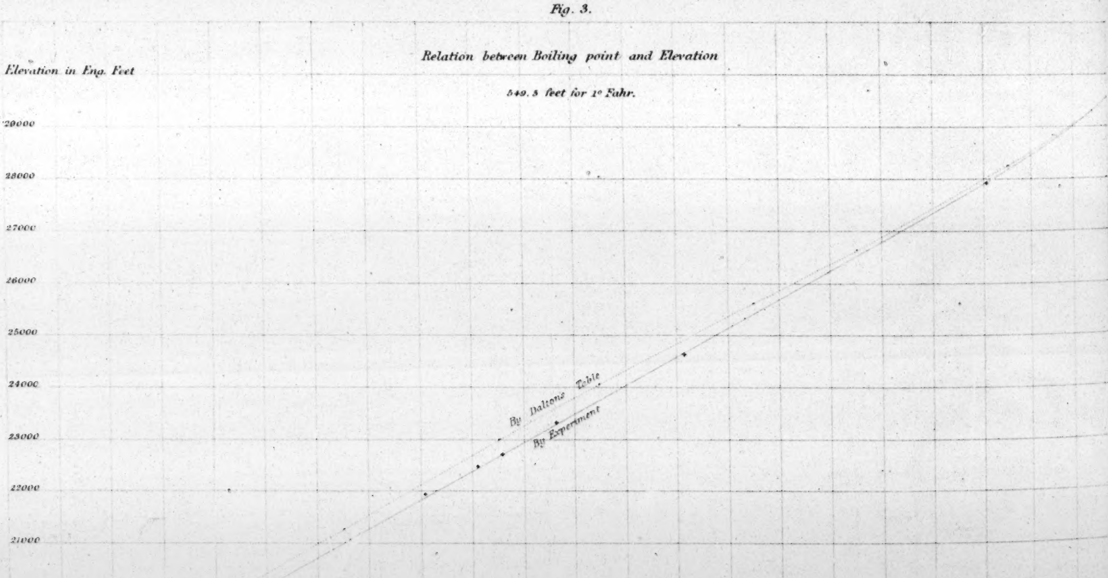


Fig. 3.



the Elasticities of Steam adapted to the measurement of heights by the thermometer, and no original observations.

During a late journey in Switzerland (in 1842), I made several observations on the boiling point of water at great heights. Having long since abandoned WOLLASTON'S thermometrical barometer as practically useless, I was led to resume the method in consequence of a very ingenious and compact apparatus for chemical or culinary purposes having been shewn to me the preceding winter, by Mr STEVENSON, instrument maker, under the name of a Russian furnace, and which was, I believe, introduced into the country from Russia by Dr SAMUEL BROWN. It consists of a very thin cylindrical copper-pan for holding water, Fig. 1, Plate XI., with three moveable wire-legs. The bottom is flat, so that the flame of a spirit-lamp plays fully upon it. This lamp or furnace consists of two parts; a flat dish or saucer, Fig. 4, containing a little alcohol, which is set on fire, and then covered by the double dome-shaped vessel, Fig. 5, also of thin copper, with an air-tight plug *a*, by which a certain quantity of spirit of wine is introduced, and the lower part communicating with a bent tube or nozzle *b*, by which alcohol in ebullition is violently projected by the pressure of its own vapour, when heated by the flame in the saucer. The jet of burning spirit thus thrown up like a volcanic explosion through the aperture of the dome, has such force as to resist the blast of a hurricane, and plays right upon the bottom of the cylindric boiler or pan. Two fluid ounces of spirit of wine, will thus boil above a pint of water in still air in four minutes; and I have frequently first melted snow, and then brought it to boil to the amount of a pint, with little more alcohol, but, of course, in a longer time.

The furnace and boiling apparatus, together with a reservoir of alcohol, packs into the copper-pan, and that into a cylindrical leather case 4 inches high, and 6 in diameter. The thermometer, Fig. 2, is carried separately. It is 15 inches long and the degrees measure $\frac{3}{10}$ inch, which is quite sufficient in practice. Parallax is avoided, by having the scale repeated on each side of the tube on two pieces of copper not in the same plane.

Fig. 6 represents the spirit measure, Fig. 7 a reservoir for spirits, Fig. 8 a water measure or cup, Fig. 9 a handle which opens all the plugs, and serves also for lifting the lamp and pan when heated.

I immediately saw the value of the apparatus for determining the boiling point, and directed Mr ADIE to adapt a thermometer to it, graduated from 185° to 214° of FAHRENHEIT'S scale, divided to 10ths of a degree, the divisions admitting an estimation to 100ths. I am well assured, however, that in no circumstances, even the most favourable, is the observation true to less than $\frac{1}{20}$ of a degree. But this quantity corresponds to only 25 feet of elevation, and is therefore accurate enough for most purposes. The minute subdivisions of DELUC'S, DE SAUSSURE'S, and WOLLASTON'S instruments, are quite unavailing, as I have found by using the instrument of the latter with every precaution.

My barometer having been broken in the course of my journeys, I was glad to have recourse to the boiling point as a means of estimating (only roughly as I expected) some remarkable elevations not before measured. In several cases I had the advantage of comparing my thermometric boiling point with a barometer, and lately I resolved to discuss these observations empirically, without reference to any theory or tables, or previous observations.

I first projected the barometric pressures in terms of the corresponding thermometric observations. These were the following :—

Date.	Place.	Boiling Point.	Barometer reduced to English inches, and to 32°
1842.			
August 4	Tacul	200°·10	23·154
... 6, 7½ A. M.	Tacul	200°·6	23·353
... 13, 8 A.M.	St Bernard	199°·08	22·674
... 16, 8 P.M.	Prarayon	201°·58	23·893
... 17, 9 A.M.	Col Collon	195°·15	20·77
... 29, 11 A.M.	Gressonay	204°·20	25·143
September 5, 7 P.M.	Martigny	210°·12	28·489

I obtained a curve, which resembled a flattish logarithmic, the barometric numbers appearing to be in geometrical progression, whilst the temperatures varied uniformly. This recalled to me an idea which I had entertained some years ago, that the boiling point would be found to vary simply with the height to which I was led from knowing DELUC's formula; but the idea had since escaped me, or been postponed to other occupations. Now, however, I projected the simple elevations of the points of observation (derived from the barometric pressures from the common tables for computing heights uncorrected for the temperature), in terms of the boiling points, as in the Plate XI., Fig. 3, and I was gratified to find, that a straight line passed almost quite through the whole of them, *shewing that the temperature of the boiling point varies in a simple arithmetical proportion with the height*, namely, 549·5 feet for every degree of FAHRENHEIT; so that the calculation of height becomes one of simple arithmetic, without the use of logarithms, or of any table whatsoever.

When I had ascertained this fact, I looked back to DELUC's formula, and found my old conjecture entirely confirmed. Its form is

$$a \log p + \mathrm{C} = h,$$

h being the height of a thermometer plunged in boiling water under a pressure p;

a and C constants. But the first side of this equation is the very form which gives elevations in terms of the barometric pressure. Hence the boiling temperature varies as the height. In other words, the pressure varies in a geometrical ratio, when the temperature of boiling water varies uniformly; but the pressure varies geometrically when the heights above the sea vary uniformly: hence the heights vary uniformly with the boiling temperatures.

It is very singular that so elegant and simple a result should have escaped every writer on the subject (so far as I know); even DELUC himself who proposed the logarithmic law, and WOLLASTON, who unawares adopted the true law as a first approximation, and then took a wrong one.*

It is not to be supposed that the coincidence appears close, because the observations are not accurate enough to test it. Of seven observations between 195° and 210°, no one differs 20 feet of elevation from the mean line,—a quantity corresponding to $\frac{3}{20}$ of a degree, an amount which can by no means be considered as being beyond the errors of observations; and the small errors $\pm$ are well distributed throughout. On the contrary, when the tensions of vapour, from DALTON'S Table, are projected beside them, as in the dotted curve of the figure, not only do they lie wholly above the line, but these tensions cannot be represented (when treated as representing barometric heights) as a straight line at all. They have a manifest curvature convex upwards. In short, as is well known, the tensions of steam cannot be represented by a geometrical progression in terms of the temperature; but when water boils in the free air, *the pressures are then exactly in geometrical progression.*

I never saw any ground for believing that the two laws must be the same. Our theory of vapours is not sufficiently perfect to admit of our drawing any such conclusion. Indeed I cannot help thinking that the influence of the pressure of the *air* upon the elasticity of *nascent steam*, is a fact not easily reconciled with DALTON'S theory of the pressure of elastic fluids. It is one thing to ascertain the elasticity of steam of maximum density which water of a given temperature can yield, and it is another to ascertain under what pressure of air water will yield steam of a given temperature. In practice I have observed the temperature of the boiling water, and not of the steam. The construction of the apparatus required this. But by moving the furnace to a side, so as to prevent the flame from disengaging the steam immediately under the thermometer, I have found the indications as steady

* He says,—"Having occasion last summer of visiting Caernarvon, which would afford an opportunity of trying the instrument on the known height of Snowdon, and *being aware that in* 3550 *feet the variations of the boiling temperature were not to be considered uniform, as they might in small elevations*, on which alone I had before tried the experiment, I wished to provide myself previously with a table for making the necessary correction, and from Dr URE'S paper was supplied with data for calculation."—Phil. Trans. 1820, p. 295. The table given from URE'S law of tensions gives a gradually increasing number of feet, corresponding to every degree that the thermometer falls.

as I believe can be got in any other way. The mass of the water and also of the thermometer favours this.

But I had a farther test of the exactness of the arithmetical progression above established, and that as severe as could well be proposed. It was to compare DE SAUSSURE's observations on Mont Blanc and the pressure there observed with the result of my formula. But first, it was necessary to correct the zero point of his instrument, and to render it comparable to mine. DE SAUSSURE's boiling point, 80° of REAUMUR, or 212° of FAHRENHEIT, was adjusted at 27 French inches, or 28.777 English.

At that pressure my thermometer (ADIE) shews 210.58 F. DE SAUSSURE's stood, therefore, 1°.42 F. higher than mine. Now, on the top of Mont Blanc, the barometer stood at 17.133 English inches.

The boiling temperature by DE SAUSSURE was . .	187°.234 Fahr.
Reduced to ADIE, ...	185°.814 ...
But the boiling point of ADIE's thermometer, with the barometer at 30 inches, is	212°.62
Subtract . . .	185°.81
At Mont Blanc, below boiling point at 30 inches,	26°.71
By GALBRAITH's Tables, ... 30.000 inches =	29228 feet.
27.133 — =	14593
Height uncorrected for temperature,	14635

Now, by the proportion found empirically above,

Height uncorrected for temperature $= 26.71 \times 549.5 = 14677$ feet,—a coincidence really surprising.

I have already stated, that DE SAUSSURE found DELUC's formula to conform accurately to his observation on Mont Blanc. It may therefore be concluded, that DELUC's formula and mine agree closely. In fact, if we take its conversion into English measures, as given by Dr HORSLEY,*

$$\frac{99}{8990000} \log z - 92.804,$$

which gives the boiling point, in degrees of Fahrenheit, *reckoned from* 32°, z being the height of the barometer in *tenths of an English inch*, we find that this gives

544.7 English feet of ascent for 1° Fahr.

Practically, I consider it sufficient to find the difference of height, in feet, between two stations, to multiply the difference of the boiling points by 550, and then correct as in barometric observations for the temperature of the air.

* Phil. Trans., vol. lxiv., p. 226.

If the barometer at one station is to be compared with the boiling point at another, the simplest way is to find what elevation the barometer expresses, compared to an imaginary station, where the barometer stands at 30 inches, the boiling point at 212°. Then the height of the station, where the thermometer has been observed, above the imaginary station, is found by the preceding rule.

For example: The corrected boiling point on the *Col d'Erin* between Evolena and Zermatt, in the Vallais, on the 19th August 1842, was 191°.93, the external thermometer 34°., the barometer (English) at Geneva was 28.73, and the temperature 72°, required the height.

Then, by GALBRAITH's table, for 30 inches,	. . .	29228 feet.
28.73 ...	. . . .	28098
Difference,	. .	1130

Consequently, supposing the *atmospheric temperature* 32°, the barometer stood at 30 inches, at a level 1130 feet below Geneva. The boiling point at the upper station was 20°.07 below 212°. The Col d'Erin was, therefore, $20.07 \times 549.5 = 11028$ feet above that imaginary sattion, or 9898 feet above Geneva. Corrected for temperature, this gives 10377; and Geneva being 1343 feet above the sea, the height of the Col d'Erin is 11720 feet.

This is purposely given as a complex case; but let us suppose that the boiling point, at the level of the sea, is assumed to be 212°, then the approximate height of the Col d'Erin is $549.5 \times 20°.07 = 11028$ feet; and supposing the mean temperature of the column 54°, the height will be 11586 feet above the sea.

XXX.—*On the Presence of Organic Matter in the Purest Waters from Terrestrial Sources.* By ARTHUR CONNELL, *Esq., Professor of Chemistry in the University of St Andrews.*

(Read 17th April 1843.)

EVER since the discovery by BERZELIUS of crenic acid in the iron ochre of the water of Porla, in Sweden, chemists have admitted the usual presence of that acid in mineral waters, or those springs containing notable quantities of dissolved inorganic constituents. In such natural waters, also, as are visibly coloured, organic matter is usually understood to be present. Any ideas, however, which may have been entertained respecting the occurrence of organic matter in the perfectly colourless, transparent, and comparatively pure water of ordinary springs, wells, and rivers, have been merely vague and conjectural.*

There is a simple experiment, which must have been familiar to most chemists, viz., that when a solution of acetate of lead is added to the water of springs, wells, and rivers, a more or less dense white cloud is almost invariably formed. This reaction, so far as I know, has been commonly attributed to the presence of inorganic salts, such as carbonates, sulphates, and muriates. No doubt, where these salts are present, in sufficient quantity to affect the lead solution, they will produce their proper agency; but, having often been struck with the much more marked effect caused by this reagent in such waters, than by the other ordinary tests of the common impurities in such sources, I was led to suspect that the effect must be usually due, in whole or in part, to some other cause; and a very little investigation soon satisfied me that this was the case. The ordinary circumstances attending the reaction, I find to be as follows: The precipitate by acetate of lead is formed even after the water has been boiled for some time, and is then soluble without sensible effervescence, if a drop or two of an acid is added immediately. The absence of effervescence may be noticed with a lens in a large test-tube; or by allowing the precipitate to subside in a well corked vessel, and then acting on it by acid. These facts shew that it cannot be due, in such cases, to the presence of carbonates or sulphates; and its ready solubility, on the immediate addition of a drop or two of acetic acid, proves that it is not a phosphate. Farther,

* Since this paper was read, my attention has been directed to a passage in Dr CHRISTISON'S Dispensatory, p. 155, in which he states that all pure spring-waters contain "some vegeto-animal impregnation," the presence of which is shewn by the discoloration of the residual salts, obtained by evaporation, when farther heated. I do not know of any other chemical writer who expresses himself in equally broad terms.

the circumstances that nitrate of silver seldom shews an equal, and generally a much less, degree of muddiness, and that that reagent, in no case of such waters which I have tried, ever produces a dense, curdy precipitate, establish that the effect is not due to any muriate; for a solution containing such a constituent, and giving even a curdy precipitate with nitrate of silver, may, nevertheless, be too weak to be affected by acetate of lead. The most probable view, therefore, which occurred was, that the reaction is due to the presence of organic matter; and this became the more likely, when it was observed that rain-water is scarcely affected by acetate of lead, although some of the other reagents are not without action on it.* Of course, as already stated, where the water contains a sufficient quantity of inorganic salts to produce their proper reactions with the lead solution, the above appearances will be modified accordingly.

I made several attempts to insulate the matter in combination with oxide of lead, by subjecting the precipitate to the action of sulphuretted hydrogen. The water employed for this purpose was the town water of St Andrews, which proceeds from springs in the rising ground to the south side of the town, and is conveyed into the houses in pipes. In its ordinary state it is transparent and colourless. It contains from $\frac{1}{7000}$ to $\frac{1}{8000}$ of solid inorganic constituents, which are sulphate of magnesia, carbonate, sulphate, and muriate of lime, with a trace of muriate of potash. When fresh drawn from the pipes, it deposits a very little ochreous matter; and, on the whole, if it may not be ranked amongst the purest of spring waters, it at least is of greater purity than the ordinary colourless water of wells and running streams. This St Andrews water gives a pretty copious white precipitate with acetate of lead, which is easily dissolved by a drop or two of nitric or acetic acid, without visible effervescence; and previous boiling scarcely diminishes the amount of this precipitate. It is equally formed if the lead-salt is added to the water after the latter has been allowed to stand some weeks in a glass jar, so as to separate every thing which is capable of subsidence. If, after adding the acetate of lead, the water is allowed to remain at rest for about a quarter of an hour, a farther precipitation then begins, which is no longer soluble in weak acids, and which is now sulphate of lead. Barytic salts immediately indicate the presence of sulphates, but the muddiness is

* It will be found, that if the solution of acetate of lead is prepared by dissolving sugar of lead in any well or spring water, which gives a considerable cloud with that salt, and is then filtered, it is less readily affected by carbonic acid in any liquid to which it may be added, than when it has been prepared by solution in distilled water. It was a solution of the former kind that was employed in the above experiments. Of all tests for free carbonic acid in solution, the most delicate is a solution of basic acetate of lead. It instantly indicates traces of carbonic acid in distilled water, on which lime-water has no action, and barytic water a comparatively feeble one. It seems to be for carbonic acid in solution what silver salts are for muriatic acid, or barytic salts for sulphuric.

less than that afforded by acetate of lead. Silver salts produce merely a decided opalescence, not a trace of any dense precipitate.

To a large stoppered bottle containing several quarts of this water, acetate of lead was added as long as a precipitate was produced. The stopper was then replaced, and the bottle left twenty-four hours undisturbed, when the precipitate was found to have entirely subsided to the bottom. The clear liquid was then cautiously decanted by a glass syphon, 2 or 3 ounces of liquid only being left. A current of sulphuretted hydrogen was then conducted through a long tube into this liquid, and the precipitate well stirred up. The liquid was then filtered and heated, to drive off the excess of sulphuretted hydrogen. A colourless solution was thus obtained, which reddened litmus powerfully, and of course contained sulphuric acid, proceeding from the decomposition of the sulphate of lead precipitated from the water, after a certain interval. When a portion of it was evaporated to dryness in vacuo over sulphuric acid, the residual matter was deliquescent, from the presence of sulphuric acid. This residue, when redissolved, left some flocky matter, and when ignited a little oxide of iron remained. That the liquid obtained, however, also contained some organic matter, was evident from the following circumstances. Saturated with potash, and evaporated in vacuo, it yielded a white crystalline mass, mixed with darker matter; and when this saline substance was heated to redness in a tube retort, it yielded vapour having a strong empyreumatic smell, and left a black coaly mass; and turmeric paper was occasionally made brown by its vapour, although this reaction could not always be distinctly observed, perhaps from the small quantity of matter heated. The acid liquid itself scarcely affected solution of acetate of copper; but when the acetate was made neutral by ammonia, or when the potash salt was used, although there was no immediate change, a greyish-white precipitate formed in a day or two. With persulphate of iron, made as neutral as possible by ammonia, the potash salt gave no precipitate at first; but a little was formed after a day or two. Nitrate of silver was scarcely affected by the liquid. Both the neutral and the basic acetate of lead were abundantly precipitated by it, and the precipitate shewed no trace of effervescence when acted on in mass by acids. The liquid evidently contained the same matter which originally affected the lead salt in the water employed; for when the potash salt was diluted with five bulks of distilled water, and acetate of lead added, a cloud was produced as in the original water, dissolved by acetic acid; soon after which, a precipitation of sulphate of lead commenced.

It appears to me that the legitimate conclusion from all these experiments is, that the original action on the lead salt was due to organic matter in the water employed. The precise nature of that organic matter they are hardly sufficient to determine, although it would rather appear to be an azotised substance, analogous, perhaps, to the crenic acid; and the flocky matter which I always observed to separate when the solutions were evaporated and redissolved, was in all like-

lihood allied to the apocrenic acid. It did not seem that the whole of this matter was procured by the process followed; for when the potash salt was farther diluted than above stated, but to an extent much short of the original bulk of the water, it ceased to be acted on by the lead salt.

My attempts to obtain larger quantities of this substance, by precipitating successive portions of the water in the same vessel, and allowing the several precipitates to accumulate together, were unsuccessful, when the process was continued for a week or two,—the precipitate, by standing, apparently suffering some degree of decomposition, or otherwise escaping the subsequent agency of the sulphuretted hydrogen; for in this way I ultimately obtained considerably less matter than by a single operation concluded without delay, as above described. It was found by BERZELIUS that the salts of crenic acid are extremely liable to decomposition; and to the same cause was probably due the partial loss in the single operation.

I could have wished to try the decomposition of the precipitate from a purer water, and therefore less liable to give an intermixture of inorganic salts; but at the time, I had not access to a sufficient quantity of any of those purer spring waters which I examined on a smaller scale.

Having thus, as I trust, shewn that some degree of confidence may be placed in the reactions to which I have alluded as indicative of the presence of organic matter in water, I shall proceed to mention those instances in which I have convinced myself, by such indications, of the presence of this matter.

Besides the town water of St Andrews, I have found it in the town waters of Edinburgh and Glasgow. Edinburgh, as is well known to its inhabitants, is chiefly supplied from a celebrated spring in the vicinity of the Pentland Hills. The water, as it comes into the town, contains only from $\frac{1}{11000}$th to $\frac{1}{12000}$th of its weight of inorganic salts, which are chiefly carbonate and sulphate of lime, and a little sulphate of magnesia. In this water, by the reactions already detailed, I fully satisfied myself of the presence of this organic matter. Its quantity was greater during dry weather in summer than after rains in winter, a result quite to be anticipated. The town of Glasgow is supplied from the river Clyde; and at the time I examined its water, which was in winter, the river was in high flood from heavy rains, and so muddy, that I could not employ the water taken directly from the channel; but in the water brought from the river into the houses, which has been cleared by subsidence and filtration through sand and gravel, I found this organic matter, although to a less extent than in the St Andrew's and Edinburgh waters, a circumstance which was probably due more or less to the large quantity of rain-water present at the time. I have farther found this matter abundantly in all the running streams and wells which I have examined around St Andrew's; and likewise in such well-waters about Edinburgh as have come under my notice.

An excellent illustration was afforded by the well-known spring of St Anthony's well, at the foot of Arthur's Seat, near Edinburgh. The water of this well may be considered as a very pure spring-water, as respects inorganic constituents; the ordinary tests shewing very feeble reactions with it. On the other hand, acetate of lead produces in it, whether before or after being boiled, a dense white cloud, dissolved without effervescence, by a drop or two of nitric or acetic acid, and no farther precipitate, insoluble in acetic acid, is afterwards formed; in short, whilst in comparison with many other waters, its inorganic purities are insignificant, its proportion of organic matter is notable.

There could be little doubt, that the origin of this organic matter in pure water was to be referred to the decomposition of vegetable matter contained in the strata and soil through which the water had infiltrated, or otherwise had its passage. I therefore could have wished to examine some spring at a considerable elevation, and having as rocky a source as possible, with the view of ascertaining to what extent it might still contain such a constituent; but during the course of these investigations, I had not an opportunity of visiting any more elevated spring than one about two-thirds of the way up the hill of Arthur's Seat, or, by barometric measurement, 522 feet above the level of the sea. This spring issues from the trap-rocks on the NW. face of the hill; but, of course, there is vegetation scattered on their surface. Accordingly I found the organic matter in its water, although to a less extent than in St Anthony's spring at the foot of the hill.

We may anticipate, that if a spring were examined on any elevated mountain, at a height entirely above the limits of any vegetation, we should cease to find this substance. A similar observation may be made respecting the water directly issuing from snow and glaciers. I have already stated, that in rain-water it does not exist. Such water, if collected with ordinary care and boiled a few minutes, is entirely unaffected by the acetates of lead.

It will readily occur, that on the supposition that this matter exists to a greater or less extent in all waters which have infiltrated through strata below the limits of vegetation, it must necessarily perform a part of considerable importance in the economy of nature. Being in solution in water, it is evidently in that state which is best adapted for being taken up by the roots and fibres of plants, and so contributing to their nourishment, in so far as that nourishment has access by these channels. May not a part of the beneficial effects of irrigation be due to such dissolved organic matter? Even as regards the animal economy, we cannot suppose that it will not contribute, in proportion to its amount, to the nourishment of man and other animals partaking of such waters; and this will more particularly be true, if it really be an azotized body.

St Andrew's, *8th March* 1843.

XXXI.—*On the Bebeeru Tree of British Guiana.* *By* Douglas Maclagan, *M.D., F.R.S.E.*

(Read 17th April 1843.)

About three years ago, I received from my friend Dr Watt of West Coast, Demerara, specimens of the bark of a tree, native of British Guiana, which had been found by Mr Rodie, late surgeon R.N., to contain a vegetable alkali, and to possess some value as a remedy in the intermittent fevers of that colony. Mr Rodie made known his discovery by means of a letter which he published in the year 1834. I made some experiments with the piece of bark, at that time in my possession; but the conclusions at which I then arrived did not appear to be worthy of being published. It was obvious to me, however, from the results which I obtained, that Mr Rodie's statement was so far correct, that the bark did contain a bitter matter, having all the general characters of a vegetable alkali, and capable of forming neutral compounds with acids. The exhaustion of my original little store of materials prevented me from proceeding farther, till last year, when, through the kindness of Dr Watt, I received a barrel of the bark, and likewise of the fruit of the plant.

The bark as I have received it, now from several sources, occurs in large flat pieces, from one to two feet long, and varying in breadth from two to six inches. It is about four lines thick; heavy, and with a rough fibrous fracture; dark cinnamon brown, and rather smooth within; and covered externally by a splintering greyish-brown epidermis. It has little or no aroma, no pungency or acrimony, but a strong persistent, bitter taste, with considerable astringency.

The fruit sent to me is a nut, of an obovate form, slightly compressed. The pericarp is greyish-brown, hard and brittle, half a line thick, rather rough externally, except at the part surrounding the point of its attachment to the footstalk, where it is smooth, and has probably been embedded in the calyx. The cotyledons are plano-convex; and when in apposition, are of the size and general figure of a walnut. A section of the cotyledons, when moist and fresh, was pale-yellow, speedily becoming brown by exposure to the air. The juice had an acid reaction, and was intensely bitter. I suspect that those sent to me were unripe, both from their appearance, and from the fact that all the attempts made to grow them have proved abortive.

The plant yielding these productions is still unknown to me. According to the information which I have received from my Demerara correspondent, it is known in the colony by the Indian name of *Bebeeru;* whilst by the Dutch colonists it is called *Sipeeri*, under which name, Dr Watt informs, it is known to

SCHOMBURGH; but I have been unable to trace any reference to such a plant in his catalogues of British Guiana Plants, hitherto published in the Botanical Magazine.

The timber of the tree is well known to wood merchants by the name of *Greenheart;* and I find that several loads of it are imported annually into the Clyde, for the use of carpenters and shipbuilders. There can be no doubt, that this timber is the produce of the tree yielding the bebeeru bark; for I have received from Greenock specimens of the wood with the bark attached, and find the latter identical in characters with that sent to me directly from Demerara. The appearance of the wood justifies the English name. It is of a pale yellowish-green colour, very hard, and heavier than water,—its density, when its pores are full of air, being 1080. It polishes readily; is said to be durable, and answers well for shipbuilding, and for making dock-gates, &c.; but it is difficult to work, and apt to split in driving bolts through it.

The plant appears, both from its use as timber and from the appearance of its bark, to be a large tree. Mr RODIE describes it as a " magnificent variety of laurel;" but beyond this, I possess no information as to its botanical history. I sent specimens of the fruit to Sir WILLIAM HOOKER and Dr LINDLEY, both of whom considered it to be a lauraceous plant, the latter regarding it as allied to the genus *Ocotea*. SCHOMBURGH, who saw a decayed flower of it, also referred it to the Laurineæ, and he considered it as having some affinity to the genus *Persea*. I must own, however, notwithstanding these authorities, that I have in vain searched through NEES VON ESENBECK'S *Systema Laurinarum* for any genus, or even suborder of Lauraceæ, at all corresponding in character with this fruit.

The bebeeru bark has been made the subject of chemical experiment by the original discoverer of its properties, Mr RODIE. He prepared from it a solution of the sulphate of its alkali, some of which was sent to me, but it is obviously mixed with impurities. He does not state what process he followed in preparing it. Mr RODIE had likewise put samples of the bark into the hands of some of the manufacturing chemists of London, including Messrs HERRING and Mr BATTLEY; and more lately, Dr BLAIR, of the Seaman's Hospital, Georgetown, Demerara, had operated upon the seeds, following the London Pharmacopœia process for Sulphate of Quinine. None of these trials, however, seem to have led to any very satisfactory result as to its chemical history. Most of the experimenters seem to have directed their attention too exclusively towards procuring a crystallizable salt of the alkali, which, as the sequel will shew, is not attainable. I received from Dr BLAIR specimens of crystalline matter, which he obtained in his experiments; but these I find to be only sulphate of lime, with a little adhering organic matter. I soon satisfied myself, that any attempt to procure crystalline salts was out of the question,—neither the alkaline matter, nor any of its compounds with acids, evincing any tendency to assume the form of crystals. It is

unnecessary to say anything as to the various unsuccessful attempts which I made to obtain a product presenting any appearance of purity. The following is the method by which I have arrived at the results, which I now venture to submit to the Society.

The bark is boiled in water acidulated with sulphuric acid, as in the ordinary process for sulphate of quinine. The fluid so obtained, which speedily becomes very turbid, is concentrated and allowed to cool. A copious deposit takes place of a light-brown matter, which is a variety of tannin; and along with it a notable quantity of sulphate of lime, in a crystalline state, is likewise deposited. These are got rid of by filtration; and to the fluid, which is of a yellowish-green colour, ammonia is added, which lets fall a dark-grey precipitate. This being collected on a filter and washed, is to be dried over the vapour-bath, being at the same time freely exposed to the air, during which process it gradually darkens in colour, from changes induced in tannin adhering to it, until it becomes of a deep-brown, or almost black tint. It is then suspended in distilled water, and sulphuric acid in slight excess added, which dissolves the alkaline matter; the liquid is treated with animal charcoal, and on filtration, is found to be of a clear yellow colour, and strong bitter taste. From this fluid ammonia throws down a precipitate, which, when washed and dried, is nearly white, and does not in the least darken by exposure to the air. This is the alkaline matter in the form of a hydrate. If this precipitate is treated with rectified spirit, it readily dissolves, leaving only a little brown flocculent matter, and forming a clear solution of a tint intermediate between yellow and orange. It has a powerful alkaline action on reddened litmus paper, and an intense durable bitter taste. The alcoholic fluid, when evaporated, leaves a shining totally uncrystalline matter, a good deal resembling a resin in external appearance, and when in thin layers, quite translucent. It is obvious, however, that this is not a homogeneous product, for in some parts it is pale-yellow, in others orange-brown. It is, however, separable into two distinct portions by the action of ether, which, for this purpose, must be anhydrous, and perfectly free from alcohol. That which I used was of density .735, and had been rectified by distillation from caustic potash.

The ether eventually dissolves by far the larger portion of the alkaline matter; but as the solubility of the alkali in this menstruum is not great, the treatment with ether must be frequently repeated, and ought to be continued, until a portion of the fluid, on being evaporated, leaves no residuum. The ether being recovered by distillation leaves a strongly alkaline and bitter resinous-looking matter, to insure the purity of which it should be dissolved in alcohol, treated again with animal charcoal, and evaporated. As thus prepared it should have an uniform homogeneous appearance, and be, when thoroughly dried, nearly of a canary-yellow colour. In mass it is opaque, and forms a pale yellow powder; but when evapo-

rated in very thin layers it is clear and transparent, separating from the evaporating basin in small glittering yellow scales.

The portion not dissolved by the ether is now to be taken up by alcohol, treated with animal charcoal and filtered, and, on evaporation, is obtained in the form of shining reddish-brown scales, not crystalline. This matter likewise possesses all the characters of a vegetable alkali.

To the former of these alkaline bodies I apply the name of *Bebeerine*, originally used by Mr RODIE: to distinguish the second, I would give it the provisional name of *Sipeerine*, from the Dutch name applied to the tree in Demerara.

The difficulty of procuring these products uniform, and the consequent uncertainty as to their purity, arises from their being uncrystallizable, and being at the same time associated with a substance so troublesome to the chemist as tannin. I have succeeded in obtaining the same results by another process which is somewhat more expeditious, but not so economical. It consists in heating the original grey precipitate in water containing about 6 or 7 per cent. of caustic potash, which forms a deep orange-red fluid, and leaves the greater portion of the alkalies, in the form of hydrate, nearly white, which is to be dissolved in alcohol and treated with anhydrous ether in the manner just detailed. A large proportion of the alkaline matter is dissolved, along with the tannin, &c., in the potash ley, but may in great measure be recovered from it, though in an impure state, by adding muriate of ammonia to the liquid.

Results of a precisely similar character were obtained from the seeds; the process of extraction, however, requiring some modification. As the seeds contain starch, cold water is the proper menstruum for exhausting them, which can be accomplished readily by the method of percolation. For this purpose the seeds should only be coarsely bruised, otherwise the starchy matter is apt to form a dense layer which impedes the passage of the fluid. The percolation should be continued till the water passes without bitterness. The fluid is then concentrated by boiling, during which a quantity of vegetable albumen separates, along with a considerable amount of tannin, and a peculiar reddish-brown substance allied to fatty matter. The fluid, when cooled and filtered, is precipitated by ammonia. The precipitate is of a pale pink colour, but becomes brown on drying. The alkalies can most readily be obtained from it by treating it while still moist with caustic potash, and subsequently by alcohol and ether in the manner formerly described.

There thus appear to exist, both in the bark and seeds, two bodies of an alkaline nature distinct in their properties. It seemed likely that there should be, besides the tannin, a vegetable acid of some kind present where so much organic matter of a basic kind existed, and to this my attention was also directed. I succeeded in separating an organic acid by the following process.

To the concentrated mother liquid, from which, in operating on the seeds, the alkalies had been separated, nitrate of baryta is added. A copious dirty-white precipitate falls, which is to be slightly washed with cold water to free it from the brown fluid. It is now to be dissolved in boiling distilled water, filtered and evaporated till a crystalline pellicle forms on the surface. The crystals are best got by skimming them off as they form during the evaporation; and by repeating the crystallization once or twice, a nearly white product may be obtained. This may be decomposed by sulphuric acid in the usual way; but I have succeeded better by decomposing a solution of the barytic salt by acetate of lead, and subsequently decomposing the precipitate so formed, by sulphuretted hydrogen. The acid liquor thus obtained is to be evaporated to a syrupy consistence, and then placed in vacuo over sulphuric acid to crystallize. It is not, however, pure, being generally of a brown tint, but it may be purified by dissolving it in ether, and again evaporating in vacuo. I have thus succeeded in procuring it in small quantity, in the form of a white crystalline mass with a waxy lustre. The process, however, has not always succeeded well, and I have not yet been able to satisfy myself as to the causes of this difficulty. The acid does not correspond in character with any hitherto described by chemists. I have therefore called it Bebeeric acid.

The properties of these several products may now be shortly considered in detail.

Bebeerine.—The process for obtaining and purifying it has been already described. When its solution in alcohol or ether is evaporated in small quantities, so as to leave a thin layer of residue, it remains in the form of a translucent shining yellow film, but when in mass or in powder it is dull and opaque. It is not at all crystalline. Its alcoholic solution has a strong alkaline reaction on reddened litmus paper. Its taste is strongly and permanently bitter, with a slight resinous flavour, and it evolves feebly a corresponding odour when dissolved in water by the aid of sulphuric acid. This does not seem to arise from any adherent impurity, but appears to be characteristic of the substance. Bebeerine is soluble in five times its weight of absolute alcohol, and it is likewise dissolved with great facility by rectified and by proof spirit. Ether takes up a thirteenth of its weight. It is very sparingly soluble in water, requiring 1766 parts of hot, and 6650 of cold water for its solution.

It will be impossible to maintain with certainty that this substance is chemically pure, until it shall have been subjected to the more rigorous examination of ultimate analysis; but the uniformity of the product which I have obtained, on many separate trials, both from the bark and seeds, and by processes varying slightly from each other, leads me to believe that it may be regarded as such. An unfortunate accident which occurred in my laboratory deprived me of almost the whole store of what I had prepared for the purpose of analysis; I have, therefore,

as yet had only one opportunity of testing its purity by determining its combining proportion. The results obtained, so far as this trial goes, are of a satisfactory nature.

I found, on analysing a portion of its sulphate dried at 240°, that its composition per cent. was, Bebeerine 86.39, sulphuric acid 13.61, which, supposing it to contain one atom of each of its constituents, would indicate for its atomic weight 254.536, or according to the scale now generally in use on the Continent, 3181.19.

Again, 0.1995 dry bebeerine absorbed 0.0295 dry muriatic acid gas; which is equivalent to the following per centage: Bebeerine, 87.56; Muriatic acid, 12.44; numbers which indicate for its atomic weight, 256.506 or 3203.47. These two results are sufficiently approximative to entitle us to suppose that the difference may depend merely on errors of manipulation.

The combination of bebeerine with dry muriatic acid gas takes place rapidly and without fusion. The salt so formed is very soluble in water, forming a clear yellow solution, on evaporating which, it is obtained in the form of transparent yellow scales. The sulphate of bebeerine is equally soluble in water; it has a bright yellow colour and glistening aspect like the muriate; both are intensely and durably bitter, with a slight feeling of astringency on the tongue. I have likewise prepared an acetate of bebeerine, of the same general appearance, and, like the others, uncrystallizable.*

The action of nitric acid on bebeerine is peculiar. My attention was accidentally directed to this, when, in the course of many attempts to purify my alkali, I on one occasion essayed to apply to this purpose M. Couerbe's process for veratria, where nitric acid is the agent employed to precipitate a peculiar resinous matter. (Ann. de Chimie et de Physique, tom. 52.) I found on adding nitric acid to a cold dilute solution of bebeerine in sulphuric acid, that the greater part of the alkali was precipitated in an altered state. Still more is it altered when it is boiled with nitric acid a little diluted with water. In this case it undergoes complete conversion, with evolution of nitrous acid fumes, into a yellow pulverulent substance, readily dissolved by hot but sparingly by cold water, and having, so far as I have examined it, a great resemblance in many of its properties to carbazotic acid.

Sipeerine.—This alkaline matter, which is insoluble in ether, I have obtained in quantities too small to enable me to do more than briefly describe its general properties.

When its solution in alcohol is evaporated, it is obtained in the form of a translucent dark reddish-brown resinous-looking matter, in thin glittering scales,

* To procure these salts of the translucent shining aspect, their solution should be evaporated in thin layers in a smooth porcelain basin. The thin layer of fluid dries up into a transparent yellow pellicle, which is easily detached from the basin. It splinters, however, in every direction, and thus assumes the form of brilliant scales, which give it, when in this condition, a pseudo-crystalline appearance. In mass, the salts are opaque and dull yellow.

but without the least appearance of crystallization. It is readily dissolved by alcohol, and also by proof spirit. It is sparingly soluble in water, and insoluble in ether. It combines with and neutralizes acids, forming uncrystallizable salts of an olive-brown tint, having, when in thin scales, a glistening appearance, which gives them the same false appearance of crystallization as in the case of the salts of bebeerine. Though I have not examined it further, I have little doubt, from its appearance and general properties, that it is a distinct substance.

Bebeeric acid.—When pure, it is white and beautifully crystalline. It deliquesces rapidly, assuming the syrupy form, especially in an atmosphere at all moist. At a temperature of 300° it fuses; and a little above 400° it sublimes, apparently unchanged, and condenses in tufts of acicular crystals. It forms with baryta, lime, and magnesia, salts which are sparingly soluble in water; with potash and soda, salts which are deliquescent, and soluble in rectified spirit; and with lead, a salt which is very sparingly dissolved even by boiling water.

The tannin of bebeeru bark has attracted my attention, chiefly from its marked resemblance in general characters to that variety which exists in the cinchona barks. It strikes a green tint with persalts of iron. It becomes slowly altered by exposure to the air, becoming sparingly soluble in cold water, and giving rise to a deposit which presents the general characters of the well known cinchonic red.

Besides these more important constituents, the bark contains brown resinous matter, gum, woody fibre, and a large proportion of calcareous salts. I have not detected any starch in the bark.

The seeds contain a little sugar, abound in starch, and contain likewise a red fatty matter, which obstinately adheres to the alkalies during precipitation, giving them a pink or reddish tint.

The general composition of the bark and seeds will appear from the following analysis:—

	Bark.	Seeds.*
Alkalies (not quite pure),	2.56	2.20
Tannin and resinous matter,	2.53	4.04
Soluble matter (gum, sugar, and salts),	4.34	9.40
Starch,	...	53.51
Fibre and vegetable albumen,	62.92	11.24
Ashes, chiefly calcareous,	7.13	0.31
Moisture,	14.07	18.13
Loss,	6.45	1.17
	100.00	100.00

The interest which attaches itself to this plant, is not limited to the fact of its adding to the already formidable list of our vegetable alkalies and acids, but arises

* The seeds which I analyzed had dried very much by keeping.

more particularly from the statements made by Mr RODIE as to its power of acting as a febrifuge remedy.

In his printed letter he states, that the solution of the alkali in the state of sulphate of bebeerine was concentrated, and exhibited in intermittent fever, and proved to possess the medicinal qualities of quinine, apparently in a very eminent degree, whilst he conceives it to have less tendency to produce determination to the head, or irritation of the stomach. He further says,—"Reasoning from analogy, we see that quinine is more febrifuge than cinchonine in the proportion in which it is less crystallizable; and therefore bebeerine, being still less crystallizable than the latter substance, might be expected, by that rule, to be still more febrifuge; and the result of the experience we have had of bebeerine, seems to warrant the conclusion."

I am not at all disposed to agree with the analogical conclusions to which Mr RODIE has come, and which were adopted by Sir ANDREW HALLIDAY, in a short notice which he published of Mr RODIE's discovery. (Edinburgh Medical and Surgical Journal, vol. xliv.). Neither am I sanguine enough to expect, that this is ever likely to take the place of sulphate of quinine as an antiperiodic remedy. At the same time, however, I think that I am in possession of sufficient evidence that bebeerine is endowed with powerful febrifuge virtues; and, considering that the tree is large, and a native of one of our own colonies, it may yet be found a good substitute for quinine, when dear or not easily procurable.

One object which I have constantly had in view in my experiments has been to make out a good process for preparing salts of the alkalies, and I have already obtained results which encourage me to make further trials. It is unnecessary to detail the variety of attempts which I have made to accomplish this object and at the same time get rid of the employment of alcohol in the manufacture, which, in Great Britain at least, would much tend to increase the expense. Through the zeal of Mr BROWN, superintendent of the chemical establishment of Mr J. F. MACFARLANE of this city, I have been enabled to procure, by a modification of the process for sulphate of quinine in the Edinburgh Pharmacopœia, a sulphate containing both the alkalies in a state of considerable purity, though not altogether free from traces of tannin. The best product, in point of quantity, obtained in one trial, was two and a half ounces of this mixed sulphate from ten pounds of the bark. My own experiments have likewise indicated, as an average product, about two and a half ounces, or a little more, from this quantity of bark.

Compared with the productiveness of yellow cinchona in the manufacture of sulphate of quinine, which may be said to yield on an average from two and a half to three per cent. of the salt in crystals, the productiveness of the bebeeru bark is much less, being at most about one and a half of sulphate per cent. I have no doubt, however, that the amount of product may be increased by future

improvements in the process; for I have found, from subsequent trials, that a considerable loss was sustained from portions of the alkalies remaining dissolved in the mother liquor of precipitation. Further, it must be remembered that sulphate of quinine in crystals contains 16 per cent. of water of crystallization, whilst the bebeerine salts are anhydrous, or contain at most only a small percentage of hygroscopic moisture.

It is impossible to arrive at any precise estimate as to how far, in point of cheapness, this substance will realize the expectations of my Demerara correspondents, until we know what price will be sought for the bark. I should esteem one shilling a pound the most that it is likely to cost, considering the fact mentioned, on Mr RODIE's authority, by Sir ANDREW HALLIDAY, that "at present the tree is felled only for its timber, and thousands of tons of the bark are destroyed annually." I have in my possession samples of the bark which were thrown out as refuse from a ship-building yard. At this price, then, which I look upon as more than should be asked, and even with a productiveness of not greater than one and a half per cent., I find that it may be prepared at a cost of 6s. an ounce; and, of course, it might be greatly cheaper, were the productiveness increased only fractionally, or the price of the bark were lower. The above price would be but little cheaper than the sulphate of quinine at its present cost, which I find to be 7s. 6d. an ounce; but not long ago, from an enactment of the governments of South America regarding cinchona barks, the price was as high as 11s. and 12s.; and under these circumstances, bebeerine might prove a useful succedaneum.

As to the comparative productiveness of the bark and seeds, I have no hesitation in giving a decided preference to the former, both in point of amount and purity of product. This arises from the difficulty of freeing the alkalies from the red fatty matter by any process applicable to the purposes of the manufacturer.

I shall not detain the Society with entering into details as to the evidence in favour of the antiperiodic powers of bebeerine, but shall avail myself of some other opportunity of submitting this to the attention of my professional brethren. I may mention, however, that last autumn, I sent out a small quantity of the sulphate prepared for me by Mr BROWN, to my friend Dr WATT, and he has sent me the details of several cases of intermittent fever, in which he used it with marked success. I have likewise had an opportunity of trying it myself in three cases of ague; and, in all, the arrestment of the disease was rapid and complete. I have likewise made trial of it in periodic headache, and the effects were such as to leave no doubt in my mind of its activity as an antiperiodic remedy. Both Dr WATT and myself administered it in the doses in which sulphate of quinine would have been employed.

Some time ago a secret medicine, absurdly purporting to be a cure for all fevers, made its appearance under the name of "Warburg's Vegetable Fever Drops." This has been found to be possessed of antiperiodic virtues; and I was

recently informed by Dr GERGENS of Wisbaden, that he and other practitioners had used it with success in Germany. Both Dr WATT and myself were led to suspect, from the history of this substance, that it was a preparation from the bebeeru tree; and chemical examination has decided me in this opinion. On evaporating the fluid, which is alcoholic, treating the residue with water acidulated with sulphuric acid, and precipitating by ammonia, I got from it a pink precipitate, becoming darker on drying, from which, by the action of ether, I extracted bebeerine of its characteristic appearance. I infer from the colour of the precipitate, that the fluid is a tincture of the seeds. It contains but little of the alkali, and abounds in a yellow colouring matter and other ingredients which I did not examine.

I feel that this paper requires some apology, as it is wanting in those points of minute research into the ultimate constitution of the alkalies, &c. which alone can render it of interest in a chemical point of view. These investigations I hope to be enabled to make in the course of the summer. In the mean time I was desirous of making known the results I had already obtained, as it is my intention to have the sulphate prepared in quantity, that its supposed virtues as a medicine may be fairly tested. Should the expectations which have been formed of it be in any measure realized, the original discoverer, Mr RODIE, will be entitled to our grateful consideration.

Eng^d by W. & A. K. Johnston

XXXII.—*Geological Account of Roxburghshire.* *By* David Milne, *Esq., F.R.S.E.*

[Read 5th December 1842 and 9th January 1843.]

It seems extraordinary, that no one should have undertaken a geological survey of Roxburghshire, more especially as the counties to the east and west of it have been examined, and accounts of their formations were published some years ago. It cannot be from its uninteresting character, that the intervening district has been neglected; for it presents as great a variety of apparently distinct formations, as there are in the adjoining counties of Dumfries and Berwick; and some of these have long been the special subjects of speculation and controversy among geologists. The British Association, in the Report of its Meeting held at Cambridge in 1833, propounded the following questions for geological inquiry.

"1. Is the red sandstone of *Kelso* contemporaneous with that of Salisbury Crags; and what relation do they respectively bear to the adjacent coal-fields?

"2. What is the exact northern boundary of the coal-field of the River *Liddell?*

"3. What are the relations as to age of the two series of whin-rocks, one running north-east along the *Liddell in Roxburghshire*, the other south-east in the neighbourhood of *Melrose and Jedburgh?*"

These questions show the opinion entertained by the Geological Section of the Association, as to the interesting geological character of Roxburghshire. But the questions which they propounded have never received an answer; a result not surprising in regard to the last of these questions, as it calls for an explanation of facts which really have no existence. A stronger proof could scarcely be adduced of the ignorance prevailing among our best geologists, of Roxburgh geology.

In describing the different formations existing in this county, I shall treat of them, *first*, with reference to the state in which they now are; and, *second*, with reference to the causes which have apparently produced that state.

I shall describe the formations in the following order:—

I. The Stratified or aqueous rocks.

II. The Unstratified or igneous rocks.

III. The Diluvial or post-tertiary deposits.

I shall not attempt by words, to define the geographical limits of these several formations, but content myself with referring to the accompanying map (Plate XII.), the colours on which represent the several classes of rocks I am now about to describe, and will at once shew the extent of each.

I.—*Stratified or Aqueous Rocks.*

1. The first of these which I shall describe, and which is by far the most abundant, is the *Greywacke*. Almost all those hills which occur in elongated ridges, are composed of this rock.

It has, in most places, the usual colour of bluish grey, though occasionally the colour is red, as in the Leader near Carolside, in the Jed near Kersheugh, and in the Kale near Oxnam. Its strata vary much in thickness; some being almost as thin as paper, and others several feet in thickness;—but its *ordinary* character is slaty, a character for which it is indebted to the presence of mica. On this account, the greywacke rocks very generally exfoliate by the varying influence of the atmosphere, and produce a soil, wet but by no means ungenial. I am not aware that the thin strata are found anywhere in this county so hard as to produce roofing-slate; but the thicker strata afford tolerably good building materials.

The texture of the greywacke is, generally speaking, what is termed "fine granular." A coarser variety (amounting almost to conglomerate) sometimes occurs, as a few miles west of Galashiels, on the turnpike road, where it is quarried.

The greywacke strata are in this county, as in the rest of the British islands, almost every where vertical. Any deviations from this rule observed by me, were very rare. At the Miller's Knowe, near Hawick, they dip at an angle of 80° to the south; at Kirkton, 80° to the south; at Rinkfair, about 75° to north; at Southdean Manse and at Abbotrule, they dip south at an angle of about 50°; above Jedburgh, the dip is to south at an angle of 65°; at Carolside Bridge, about 40° to the north; west of Edgerstone Rig, they make an angle of only 20° with the horizon, dipping south.

In several places, the foldings or contortions of the greywacke strata are well exhibited. On Jed water, about a mile west of Edgerstone, there is a good example, in consequence of the great height of the west bank. The greywacke strata are there bent, forming a very acute angle, opening vertically downwards. The same individual strata which, at the top of the bank, form this rapid curvature, may be seen at a little distance, forming an opposite bending at the bottom of the bank. On Oxnam water, a similar fracture of the greywacke strata may be seen, and on a much larger scale; but on that account it is not so obvious, as the whole contortions cannot be easily taken into one view. Near Crailing house, they dip north at an angle of 85°., whilst near Upper Crailing mill (about two miles higher up the river), they dip south at an angle of 80°; and in the intervening parts of the river, they present extraordinary bendings and twistings. Near South Dean and

near Abbotrule, the strata may be seen dipping in opposite directions, not far from each other, affording indications of contortions on a still more gigantic scale.

It is an interesting fact, as tending to indicate the direction of the forces which produced these contortions, that the greywacke strata in Roxburghshire, as elsewhere, crop out in lines which run nearly due east and west by compass.

One place where I observed the greywacke strata cropping out in a different direction was in Liddesdale, above Hermitage Castle, at a place called the Grains. Its strike there is north-east by east; but this was a mere local aberration, which was the more obvious, from the strata there dipping at the small angle of 40° to the horizon. In the burn behind Melrose, which flows from the Eildons, the greywacke strata run north-west by west, and rise to these hills. This is the strike of the beds also on the Jed, $\frac{1}{4}$ mile from Cleslipeel, and on the Carter burn at Sykehead.

It is a consequence of these gigantic foldings of the greywacke strata, in an east and west direction, that all the principal valleys in that formation run in the same direction, as is well shewn by the course of the rivers Ewes, Teviot, Ale, and Borthwick, which are the principal rivers in the district.

No fossils, except some morsels of black vegetable matter, have been discovered in the greywacke strata of Roxburghshire. Sometimes they exhibit on their surface a curious concretionary structure, which has been by some, though I think quite erroneously, attributed to organic causes.

The only metallic veins I have observed in this formation consist of hematite or red oxide of iron. They are in some places very abundant, as below Cowdenknows (on the Leader river), where they may be seen in the channel, from half an inch to an inch in thickness, filling up the natural joints of the rocks. In some places, these hematitic veins form, at their out-cropping on the surface of the greywacke strata, fantastic figures, as on the Jed at Cleslipeel. There is no doubt that this red oxide is largely distributed through the greywacke rocks.

I understand that small portions of galena were formerly found in the greywacke strata, at Langholm Bridge.

2. The next class of rocks to be described is the *Old Red Sandstone formation*, though I am unwilling to assert, that they form a class independent of, and distinct from, the one which I am next to describe, viz., the Coal Measures. On the contrary, there are strong reasons for believing, that both these sets of rocks, though their outward and visible characters are very different, belong to the same epoch, and have only been made to assume distinct appearances by local causes, to be afterwards alluded to. At all events, there is no grand line of demarcation between them, such as, in other cases, is indicated by the interposition of beds of conglomerate, or by striking differences of dip.

For the sake of distinctness, I will here treat of these two sets of rocks separately, and in the order generally followed.

The Old Red Sandstone rocks are most generally of a dull brick-red colour. In a few localities, there are strata nearly pure white, and, still more rarely, there are strata of a yellow colour.

The texture of the stone is soft, and seldom compact. The picturesque character of the valley of the Jed, enclosed within steep and lofty cliffs, for many miles of its course, is mainly attributable to the facility with which that river and its tributaries cut through the old red sandstone strata. But, though this is the general character of these rocks, they in many places become hard enough to form good building stone.

The lowest member of this formation, when visible, is almost every where seen to be a conglomerate, or bed of pebbles cemented by sand and clay, highly impregnated and reddened, by oxide of iron. This conglomerate is found in very many places on the upturned edges of the greywacke, as may be seen at Jedburgh, both above and below the town, in the river; in Hassendean burn, a few hundred yards below the village of that name; in the Wauchope and Catlie burns, and other places.* It was the first of the places just mentioned, which furnished to Dr Hutton the strongest of his proofs in support of his views as to the elevation and disruption of rocks by volcanic action, and the formation of new rocks out of the ruins of the former set. As Dr Hutton's description of the section is still perfectly applicable, I cannot do better than quote the following passage from his celebrated work on the Theory of the Earth.† In describing the vertical and the horizontal strata of the Jed, he refers to "a certain pudding-stone, which is interposed between the two, lying immediately *upon* the one and *under* the other. This pudding-stone (he adds) is a confused mass of stones, gravel, and sand, with red marly earth. These are consolidated or cemented in a considerable degree, and thus form a stratum extremely unlike any thing which is to be found either above or below.

"When we examine the stones and gravel of which it is composed, these appear to have belonged to the vertical strata or schistus mountains. They are in general the *hard and solid parts of those indurated strata*, worn and rounded by attrition; particularly sand or marl-stone consolidated and veined with quartz, and many fragments of quartz, all rounded by attrition. In this pudding-stone of the Jed, I find also rounded *lumps of porphyry*, but have not perceived any of granite. This, however, is not the case in the pudding-stone of the schistus mountains, for, where there is granite in the neighbourhood, there is also granite in the pudding-stone. From this it will appear, that the schistus mountains or the vertical strata of indurated bodies had been formed, and had been wasted and worn in the natural operations of the globe, *before* the horizontal strata were begun to

* These places are indicated on the map by red dots.

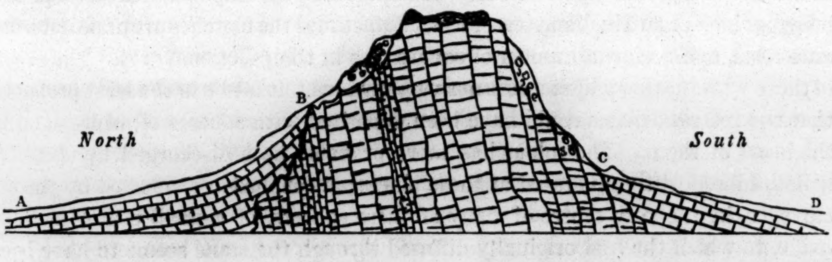
† Vol. i. p. 436.

be deposited in those places: the gravel formed of those indurated broken bodies worn round by attrition, evince that fact."

At every place where I have examined the conglomerate of this formation—whether in contact with the greywacke or not—I have found that Dr HUTTON's remark is true, viz. that the pebbles composing it consist chiefly of greywacke strata, and partly of other rocks, such as porphyries, which will be afterwards shown, from other evidence, to have been previously existing. In the Catlie Burn, the pebbles consist chiefly of greywacke, quartz, and porphyries.

The place where I observed the conglomerate in the largest masses, is near Earlston. There are escarpments of it there, several hundred feet thick. The lowest parts of the county are, generally speaking, those in which the conglomerate beds are thickest and coarsest.

In some places, the red sandstone formation is seen resting on the greywacke rocks without any conglomerate interposed, as, for example, at South-dean manse. The lowest stratum there is a dark red clay, with streaks of yellow in it; and a little farther off, this turns into a soft yellow sandstone. The following woodcut represents an interesting junction of the old red sandstone rocks and the greywacke strata, on the river Jed, about half a mile below the North Lodge of Edgerstone.



AB and CD are strata of Old Red Sandstone, resting on the nearly vertical strata of greywacke. The length of the section is about 300 yards, and the height 25 yards.

At a few places in the county, I have seen the Old Red sandstone rocks, if not resting contiguously on the porphyritic or felspathic rocks, to be afterwards described, at all events lying so close to the latter, as to leave no doubt that they have been deposited upon the latter. In these cases, however, which all occur in the higher parts of the county, near the Cheviot Hills, there was no conglomerate.

The white strata of sandstone are peculiar, not only for their perfect whiteness, but also for that quality which geologists have termed saccharine. Yet they are *not very* crystalline in their texture, but are soft and fine-grained. These white sandstones are worked at the top of a hill between Minto and Belshaes (where it rises a little towards a trap-hill west-north-west of it), and on the north side of

Ruberslaw (where the stratum is 12 feet thick), and rising towards the hill at an angle of 15°. At both of these places, there are the usual red strata, lying over as well as under the white beds. I understand that beds of similar white sandstone have been quarried at Pinnacle on the Ale-water, in South-dean parish, and also in Jedburgh parish, at Tudhope and Ferniehirst.*

The yellow variety of sandstone, existing at least among the old red rocks, occurs at only two places known to me, viz. at St Boswell's Green and Kirklands. At the former place it is quarried. I understand it occurs also at Bedrule.

These red rocks, which prevail so extensively through Roxburghshire, are almost everywhere horizontal. They preserve their horizontality even at the sides of trap hills, some of which (as, for example, the Dunion, Ruberslaw, Peniel Heugh) being entirely surrounded by them, look like black rocky islets in a red sea.

There are few places where the strata in this formation exhibit any considerable dip. On Jed-water below Edgerston, the strata dip towards the north at an angle of 25°; on the south side of Lilliard's Edge, they dip to the south-west at an angle of 30°; at Plewlands quarry, in the parish of St Boswell's, they dip south at an angle of about 20°; to the north of Hunthill, they are nearly vertical—owing to a local dislocation.

There are sometimes on these red sandstone rocks curious spots and blotches of a white and bluish-white colour. The spots are generally spherical, the spheres being an inch or two in diameter, or less. Sometimes the blotches are of no determinate form, and occupy a number of square feet in their sectional area. The origin of these white spots and patches is not very obvious. It is not in the least probable, that the red sandstones could have been deposited with spheres of white sand in the heart of them. The colour has more probably been discharged by chemical action, subsequently generated at these places; a conclusion confirmed by the occurrence, in the very centre of many of these spheres, of a small pea of metallic oxide, to which the iron originally diffused through the stone seems to have been transferred.

The spherical white spots now referred to, must be familiar to every one acquainted with the old red sandstone formation in other parts of Scotland. I believe that they are not confined to this class of rocks, and in particular, that they occur likewise in the new red sandstone. But though the phenomenon is common, I do not think the cause of it has ever been distinctly explained. I shall therefore venture, in the second part of this memoir, to throw out some suggestions on that point.

* Tudhope quarry is three-fourths of a mile north of Jedburgh, whilst Ferniehirst quarry is on the south side of the same valley. The rocks are, at both places, nearly horizontal; and being on the same level, as well as of the same colour, which is not a common one in the district, it is not improbable that they are portions of the same stratum, which originally stretched across the valley, before it was scooped out.

In September or October 1840, I had the good fortune to discover in these red rocks, at two places pretty far apart, bones and scales of fossil fish,—that kind which has been found in such abundance at Clashbennie in Perthshire. The place where I first found them (in company with the Rev. Mr Aitken of Minto) is at the head of Wauchope Burn, on the east side Windburgh Hill. The next place was at Plewlands, in the parish of St Boswell's. Those found at the former place I submitted to the inspection of AGASSIZ, who pronounced them to belong to the species *Holoptichius nobilissimus*, and considered them clearly to indicate that the rocks containing them belonged to the Old Red Sandstone formation. I discovered scales of the same fish also on the banks of the Jed, about a mile to the east of Southdean manse, and traces of them in Sunlaws quarry, on the Tweed, opposite to Roxburgh.

Since this discovery in 1840, more remains of Holoptichius have been discovered, and in rather an interesting situation. They occur about ¾ mile to the north of Jedburgh, at the place called Tudhope, where, as already noticed, a quarry of *white* sandstone was formerly worked. No scales have yet been found in the quarry itself. They were observed in one of the stones of which a dyke in the immediate neighbourhood was built, but the stones of which were taken from this quarry. This discovery of *Holoptichius* in rocks, apparently not near the bottom of the series, but among their highest members, and in a rock which has not the prevailing red colour, furnishes strong additional evidence, in confirmation of the opinion, that the whole of these rocks in the vale of the Jed, of the Ale, and of the Teviot, belong to the old red sandstone formation.

There have been also sent to me, by Mr OLIVER of Langraw, a few specimens of fish-scales, found by him in the Wauchope Burn, in the course of last year. Some of these scales are of a much smaller size than any which I had fallen in with, and the markings on them are quite different from those on the larger scales.

I observe that Mr DUFF, in his Sketch of the Geology of Moray, mentions (page 29) that, besides the large scales of the Holoptichius, which abound in the red sandstones there,—"smaller scales often occur: but they have not as yet received much attention." Both the larger and smaller scales figured by Mr DUFF, correspond in size and markings with those which occur in Roxburghshire.

It is proper to add here, that small portions of lead (sulphuret of galena) have been found near Abbotrule, in a mottled red and white sandstone, which is among the lowest of the old red sandstone formation. I do not know whether the metal occurs in veins or in concretions, having searched the place for it unsuccessfully. But Mr Oliver of Langraw has lately sent to me some small nodules which are apparently in their natural form. He states that they are found in the channel of the burn, which cuts through the sandstone in question, and which, from its friable character, easily decomposes. It will be observed, that this locality is not far from the trap of Bonchester Hill, and a basaltic dyke, to be afterwards particularly noticed.

3. The rocks to be next described are those which, for distinction sake, may receive the well known term of *Coal-Measures.* They consist chiefly of sandstones —grey, white, and red; of earthy marls—green, grey, and light-brown; of black shales; and of limestones—grey, reddish-brown, lilac, and black. It is among these rocks that seams of coal are occasionally found.

It will be seen from the map, that it is on the eastern and western extremities of the county, that this class of rocks exists in any abundance; though, as will be afterwards explained, patches of them do occur in intermediate spots. In these intermediate spots, much money has been fruitlessly expended in searching for coal. Trials have also been made in other places, where there was no appearance whatever to justify such attempts; as, for instance, close to Maxton Manse, on the west of it, where there is nothing but old red sandstone.

It has been stated, that the rocks now referred to appear generally to lie *over* the red rocks already described. It should be added, that in some places I have observed red rocks, of similar appearance, also lying above them. For instance, in Dinlee Burn (one of the feeders of Hermitage Water in Liddesdale), a yellow coal sandstone grit may be traced within two or three feet of a greywacke hill, from which it slopes or dips at an angle of about 8° or 10°. Over these, are strata of red sandstones and shales, very soft and friable, and not exactly conformable with the subjacent coal-measures. But the exact line of junction is indiscernible. So also in Laidlehope Burn, above Winshiel-Know (in Liddesdale), the soft red rocks may be seen lying above black coal shales, and conforming with them. On the Ale also, close to Kirklands House, a yellow coal sandstone may be seen at the river side, overtopped by the red rocks.

Though, in most places, the coal-measures of Roxburghshire lie over and rest upon the soft red rocks, they, in a few places, may be seen resting directly on or very near greywacke. Thus, in the Black Burn (a tributary of the Jed), I found the grey gritty sandstone of the coal-measures lying on the vertical edges of the greywacke formation. In the Carter Burn, at Sykehead, as well as in the Edgerston burn (about a mile north of the Carter toll-bar), the coal sandstones of Northumberland may be seen within 100 yards of the greywacke, and exhibiting a southern dip.

That the rocks which I have designated Coal-Measures really deserve that name, is evident from the fact, that they contain all the fossils which are characteristic of that formation; and, in some places, very valuable seams of coal, which are worked. In Liddesdale, coal is now worked very extensively at Rowan Burn. Four seams exist there, which are respectively in width 5½ feet, 9 feet, 6 feet, and 2 feet. Coal was worked, also, formerly at Byre Burn, on the Esk, about two miles below Langholm, where the principal seams were two in number—one 2 feet 7 inches thick, the other 5 feet 10 inches thick. Coal was formerly worked also at Lawston, on the river Liddel (where, however, the seams do not exceed 14 inches thick); and several thin seams have been proved by borings to run

along the south margin of the county, from Castleton eastward to the Carter. At the Rowan Burn colliery, a beautiful set of drawings has been made by Mr GIBSON, manager there, of the vegetables, fish-scales and teeth, as well as of shells,* which are frequently found in the shales and fire-clays immediately above and below the coal seams. At Maxwell-heugh, near Kelso, a great abundance of fossil vegetables, peculiar to the coal-sandstones, have been found, of which specimens nay be seen in the Kelso Museum. At Hunthill, where there is abundance of black coal-shales, though surrounded by red rocks, I found the teeth and spines of fish, which appear the same as those of the Berwick and Edinburgh coal-fields, and which are generally considered to be the *Megalychthis*. I have found there also the well known bivalve shell *Spirifer*, which abounds in carboniferous strata. At this last place, trials have been made at different times during the last fifty years, for coal; and Mr BELL, the present proprietor, renewed these attempts some years ago, though without success. In the sinkings made for this purpose, a series of black shales, thin limestones, and grey sandstones, were gone through, clearly indicative of the coal-formation. In the shales, nodules of clay-ironstone occur, filled with coal vegetables.† At the Forrester's house, near Hilton hill, about two miles north of Ancrum, a coal-seam, 6 inches thick, was found in sinking a well; and the portions excavated for that purpose burnt well in the fire.‡

* I saw some specimens of *Lingula* at Rowanburn in the fire-clay and shale lying about the pit-mouth. This shell is very common in the Mid-Lothian coal-shales.

† As great doubts are still entertained by many persons of the relative age of the dark-coloured strata of Hunthill, and the red rocks which surround this spot, I may mention, that the two sets of rocks may be seen, if not in junction, at all events within a few yards of each other, in the glen on the west side of Hunthill House. In 1839, I examined the place, at the request of some of the principal inhabitants of Jedburgh, who, on public grounds, were desirous of learning the probability of coal being found there, with the laudable view of starting a subscription to assist the proprietor in boring and sinking for it. It was then that I discovered the fossils above mentioned, which left no doubt in my mind as to the class of rocks prevailing at Hunthill, though, as they appeared to underlie the Carter limestone, I discouraged any expectation of finding a workable coal-seam. On this occasion, also, I observed that the red rocks in the glen just referred to, appeared to dip under the shales and limestones; though, from the quantity of grass and brushwood then covering the ground, no line of junction was discernible. I have been informed that last autumn (1842) Mr ADAM MATHESON, millwright, Jedburgh, and who possesses an ardent taste for Geological researches, made a minute inspection of the spot, for the purpose of clearing up the above point, and traced the red rocks up to the coal-measures, beneath which he and Mr JEFFREY, writer in Jedburgh (who accompanied him), distinctly saw that they dipped. He informs me, that only about 4 feet above the red sandstone strata, there is a bed of limestone about 2 feet thick, in three layers, in quality exactly resembling the limestone worked on the Carter at Meadowcleugh. About 300 feet above this limestone bed at Hunthill, a coal-seam 3 inches thick occurs.

‡ Sir W. SCOTT of Ancrum informed me (in 1840), that he ascertained this from the person who had dug through the coal in sinking the well.

The general dip of these coal-measures is, near Kelso, towards the east, and at a small angle; on the Carter (in Liddesdale); and at Hunthill, towards the south. The Rowan burn coal-seams lie a great way above the beds of workable limestone to be afterwards described, which crop out a couple of miles to the north and east of that colliery,—the whole strata there having a general dip to the south and west. Even at Byreburn (where coal was formerly worked), and which is situated about a mile north of Rowanburn, the seams are still many fathoms above the limestone. In the west part of Liddesdale, however, the whole strata take a bend, which accounts for the non-appearance of any workable seams of coal farther east than Lawston: For, a little to the east of Rowanburn, the strata change from a southerly to a westerly dip, and thus strike across the river Liddell into Cumberland, where they again resume their southerly dip, and preserve it, with some inconsiderable exceptions, eastward along the English border, all the way to Wooler, at the east end of the Cheviots. Indeed, I consider that the coal-measures near Berwick, and which run along the south bank of the river Tweed for about 10 miles, dipping to the south, belong to the same class of rocks, and to the same epoch, as those just described prevailing in Liddesdale. In the Berwick coal-field, as in Liddesdale, there is a large body of workable limestone, consisting of exactly the same number of workable beds, viz. *eight*, and of pretty much the same thickness. Above that deposit there are also, in both coal-fields, workable coal-seams. It is true that, in the Berwick coal-field, there are below the limestone beds other seams of coal, which, though they have their equivalents in Liddesdale, are not the same either in number or thickness; a variation, however, which is not to be wondered at, considering the distance between the two districts.

I consider, then, that no workable seams of coal will be found in Liddesdale west of Lawston—a remark which I offer with reference to the attempts now proposed to be made for discovering coal in Liddesdale. The thick limestone beds which lie below all the workable coal-seams, run chiefly on the south bank of the Liddell, from Penton Linns eastward. They do come a little farther north at the junction of the Liddell and Hermitage; and, in consequence of this, some thin coal-seams are brought within the borders of Scotland, at the head of Tweedenburn and Harden-burn. Near Windburgh, it is true, there is a thick bed of limestone worked, which is eight or ten miles within the Scotch border, so that there might be supposed to be space enough for the existence of coal-seams lying over it,—but of which there is only one known, a few inches thick, a good distance above the limestone. However, it is, in the *first* place, uncertain whether this bed of limestone belongs to the series just referred to; and, in the *second* place, even though it were, there is not cover enough upon it to include or "*bring on*" the coal-seams in question. In the *third* place, a large portion of the district intervening between Windburgh and the English border, is occupied by a range of greywacke hills.

The following is a section of the strata at the Carter Lime-Works, as given to me by the overseer there.

	Ft.	In.
Limestone, ...	1	8
Sandstone in two or three beds (of unknown thickness),		
Shale, (Do.)		
Coal-seam, ...	1	4
Various beds of shale and sandstone, about .	600	0
Limestone (now worked), ...	14	0
Shale, sometimes containing a thin coal-seam, .	3	0
Limestone, ...	3	0
Beds of shale and sandstone, ...	14	0
Limestone, ...	2	0
Sandstone and thin beds of limestone, . . .	?	

The coal-seam mentioned in the preceding section, is now worked near the Carter Lime-Works. On one of the coal-pits there, I found a slab of sandstone covered with marine fossil shells, such as *spirifer*, *productus*, &c. The same seam was formerly worked at Kerryburn, not far to the west. It was about 12 or 14 inches thick.

As the connection between the Liddesdale and the Berwick coal-fields is, for several reasons, important to be determined, I may here mention the places nearest to the border, where coal-seams are or have been worked. At Lewisburn in Northumberland, there are two seams, one said to be 1 ft. 5 in., and the other 2 ft. 2 in. thick, separated by 6 fathoms of shale, sandstone, and thin beds of limestone. Over these coal-seams, at a distance of about 10 or 12 fathoms, there is a thick bed of limestone, which crops out near Keildor Castle. At Plashets (still farther east) there is a seam said to be 6 feet thick. At Blackhope there are two seams, one 4 feet, and the other 2 feet thick, each having a stone in the middle. At Whitelee, about a mile to the east of the Carter Lime-Works, there is a bed of limestone 6 feet thick, which lies 17 or 18 fathoms above the Carter coal. Between this limestone bed and another of equal thickness which crops out at Ruken, there are two coal-seams, the one 2 feet, and the other 3 feet thick. This last-mentioned limestone is about 50 fathoms above these seams.

The coal-seams now described continue eastwards by Rothbury, Eglingham, Chillingham, and Doddington, to Belford and Berwick.

In confirmation of the opinion, that the same set of rocks extends from the east coast of Northumberland to the neighbourhood of Liddesdale, two other facts may be mentioned. The limestone beds which crop out at Falstone (fifteen miles south of the Carter) run eastward, parallel with the coal-seams. One of these limestone beds at Falstone contains galena, and to such an extent that it was once worked.* Between Belford and North Sunderland there is a limestone bed

* At Roanfells, on the north side of Liddesdale, a quantity of lead was found, and a company was formed to work it. In ancient times some metal must have been smelted there, as heaps of slag and cinders are met with on the muirs.

on the same strike or outcrop, which in like manner contains galena. It is probably a prolongation of this bed still farther westward which, near Alstone Moor, affords so large a supply of lead. The other fact is, that the same vermicular looking fossils, indicating an animal two or three feet in length, found in the slaty sandstones of Haltwhistle,* have been found by me in a sandstone rock of precisely the same character, on the sea-coast at Scremerston, between Holy Island and Berwick.

Some more special notice is deserving of the limestones found in the class of rocks now under consideration. There are three kinds, viz., carbonate of lime or ordinary lime-rock, Chert limestone, and magnesian limestone.

(1.) The first kind is that worked at Meadow-cleugh on the Carter, at Limekiln Edge, between Hawick and Castleton, at Lariston, at Harelaw Hill, and at Gilnockie Tower on the Esk.

At Harelaw Hill the rock is about 14 feet thick. It is compact, and of a bluish colour. There are, however, several other beds of limestone, one of which is said to be above 20 feet thick.

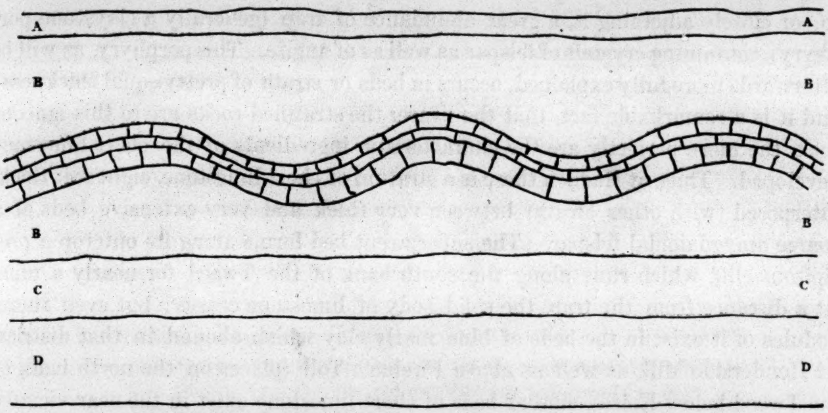
At Lariston the rock is of a bluish-grey colour, and several yards in thickness, divided, however, by one or two beds of shale or fire-clay. It abounds with marine shells, such as the *productus* and the *modiola*, which last shell I have also seen in the old Greenholm quarry. This bed of limestone is thought to lie above the Carter limestone.

The limestone at the places just mentioned, and most of the others where it is worked, contains numerous casts of the *productus*, *orthoceras*, *encrinites*, and other marine *mollusca*. The quarries at Limekiln Edge and the Carter are, however, exceptions to this remark, which may perhaps be owing to changes produced on their texture by large masses of trap-rocks adjoining them, to be afterwards described.

At Limekiln Edge, the bed worked is about 12 feet thick. It presents some remarkable undulations, which have nothing to correspond with them in the strata above or below. These undulations are seen also in the Berwick coal-field; and I understand from Lord Greenock, that he has seen them near Cambo in Northumberland, about twenty miles south of Cheviot, through which district the same limestone beds undoubtedly run. The occurrence of these undulations between horizontal beds have not, I believe, been explained, and I confess my inability to offer any plausible hypothesis.

* Geological Society's Transactions.

Section from Limestone quarry at Limekiln Edge. Depth 20 feet, length 60 feet.



A A, Stratum of gravel.
B B, Blue marly clay in horizontal layers, containing undulatory strata of limestone.
C C, Limestone 4 feet thick.
DD, Limestone 6½ feet thick.

The only analysis of this class of limestones which I have obtained, is of that worked at the Carter, made by the present Professor Gregory of Aberdeen. The following shews the constituent elements of three specimens :—

Carbonate of lime . . .	80	90	90
Magnesia, ...	3	trace.	5
Insoluble matter (sand), . . .	17	7	5
	100	97*	100

(2.) The chert limestone occurs at Robert's Linn (on west side of Windburgh), Windshielknow, Kerchester, Bedrule, Smailholm, and Sprouston.

At all these places, the rock is accompanied by sandstones, marls, and shales, which appear to me to belong to the coal-measures.

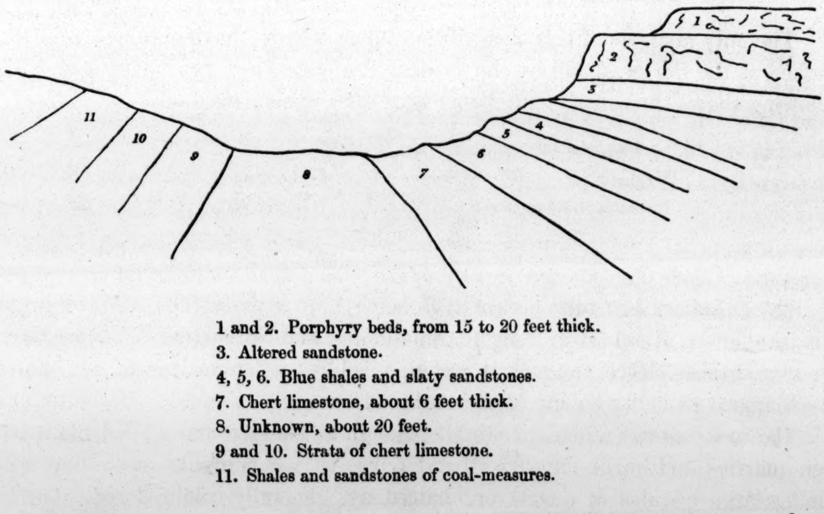
The rock has not much lime in it, though at Hadden and at Bedrule it has been quarried and burnt for agricultural purposes. It is highly crystalline, containing large nodules of quartz or chalcedony, generally coloured red. On an analysis of the Hadden limestone, there was found to be (out of 100 parts) 50

* It is observed by Mr Gregory, "that this slight deficiency is probably owing to a little water, which most limestones contain."

per cent. of carbonate of lime, 44 of magnesia, 12 of peroxide of iron, and 4 of silica.*

At all the places where I have seen this chert limestone, there is contiguous to, or closely adjoining it, a great abundance of trap (generally a claystone porphyry), containing crystals of felspar as well as of augite. This porphyry, as will be afterwards more fully explained, occurs in beds or strata of pretty equal thickness; and it is a remarkable fact, that the nearer the stratified rocks are to this igneous rock, the more perfectly are the characteristic ingredients of the chert limestone developed. Thus, at Hadden there is a stratum of chert limestone, eight feet thick, interposed (with other strata) between very thick and very extensive beds of a coarse amygdaloidal felspar. The superjacent bed forms along its outcrop a precipitous cliff, which runs along the south bank of the Tweed for nearly a mile. At a distance from the trap, the solid body of limestone ceases; but even there, nodules of it exist in the beds of blue marly clay which abound in that district. At Henderside Mill, as well as above Fireburn Toll (places on the north bank of the Tweed below Kelso), similar beds of chert limestone exist in the near vicinity of trap.

At Bedrule Hill, there are several beds of this limestone, the position of which is shewn in the following section.

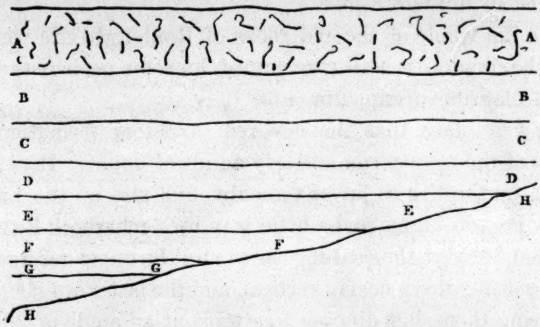


1 and 2. Porphyry beds, from 15 to 20 feet thick.
3. Altered sandstone.
4, 5, 6. Blue shales and slaty sandstones.
7. Chert limestone, about 6 feet thick.
8. Unknown, about 20 feet.
9 and 10. Strata of chert limestone.
11. Shales and sandstones of coal-measures.

The foregoing section extends for about 200 yards. The greenstone porphyry here, as at Sprouston and Hadden, forms beds nearly horizontal. Its outcrop at Bedrule runs very uniformly above these carboniferous strata, for about a mile.

* Analysis by Dr R. D. Thomson. (Mag. of Nat. History, by Loudon, No. 29.)

At Roberts Linn, the following section illustrates the position of the chert limestone.



A A, Bed of porphyry.
B B, Stratum of red clay, streaked with thin white lines, parallel to the porphyry.
C C, Stratum of red clay, streaked with thick blue lines, parallel to do.
D D, Bed of chert, pretty compact.
E E, Bed of chert, mixed with blue clay.
F F, Bed of gravelly red clay.
G G, Ditto.
H H, Channel of burn.

This section is about 30 feet high, and about 100 feet long.

Below Winshielknow, the trap in contact with the chert limestone is not (as in the cases just mentioned) a plateau or horizontal flow of trap, but a dyke 30 feet wide which cuts vertically across the strata, running in a northerly direction. It has tilted the strata on each side of it.

(3.) The only place where I noticed beds of true Magnesian Limestone, is in Tweeden Burn, on the south side of Liddesdale. There were several strata, a few inches thick, of a buff yellow colour, and presenting cavities in the way usual in this kind of limestone. There are thin beds, precisely similar, on the banks of the Tweed, both above and below Coldstream.

Another rock (if rock it can be called) belonging to this class, which here requires notice, is Gypsum. It exists in considerable abundance on the north bank of the Tweed at Kelso; also about a mile above Floors Castle, and near Birgham, on the confines of Berwickshire. It has been found too on the north bank of the Eden, about a mile above its junction with the Tweed. Both the white and the red varieties are found at all these places. The red exists in nodules or concretions;—the white is in the form of veins, more or less vertical, and has evidently been formed subsequently to the red, for it intersects the red gypsum. This mineral occurs only in beds of blue marly clay. Another place where gypsum is said to have been found is Archerbeck Burn, in Liddesdale. If this be true, it is probably in the new red sandstone formation, which will now be noticed.

4. The formation or class of rocks just mentioned, I mean the *New Red Sandstones*, there are some reasons, though they are not, in my opinion, conclusive, for supposing to exist in Roxburghshire.

At one time the whole of the red rocks of Roxburghshire were assigned to this class, and the opinion is still entertained by some geologists; nor is it altogether devoid of plausible arguments.

(1.) In the *first* place, that the new red sandstone formation exists on the western borders of the county can scarcely admit of doubt. It extends from the plain of Carlisle, up the Esk as far as Canonby, and also up the Liddell to about 200 yards below Penton Linns, and a little way up Archerbeck Burn. At Penton Linns, the division betwixt the coal-measures and the upper red rocks is very palpable; the former being there nearly vertical, and the latter abutting against them, and partly covering them, but dipping westward at an angle of 30°. As the formation thus reaches into Liddesdale, it was not unnatural to suppose that the same formation existed, likewise, on the eastern borders of the county. It is well known, however, to geologists, that the appearance of this formation in the plain of Carlisle, is owing to an enormous sinking of the strata, which took place in that part of England; and, accordingly, its eastern boundary is marked by a fault, which divides it from the carboniferous rocks of Northumberland. This fault shews itself on the Liddell, about 200 yards below Penton Linns, where the new sandstone rocks are seen with their edges tilted up against the limestones and other beds of the Canonby coal-field, and in such a way as very clearly to indicate that there has been a general sinking of the newer formation.

(2.) In the *second* place, there is a very remarkable horizontality in the red rocks of Roxburghshire, which seems to indicate a comparatively recent period for their deposition. And this circumstance becomes the more striking, when it is found that, on the other hand, there are many localities bordering on this district of country, where the coal-measures are inclined at considerable angles. Thus, at Bedrule Hill, where, as has just been shewn, the limestones, shales, and yellow sandstones are vertical, the red sandstones immediately adjoining in Huntly Dell, are perfectly horizontal, and indicate no signs of disturbance. Generally speaking, it cannot be doubted that the red rocks present fewer deviations from the horizontal, than the coal-measures. Must they not, then, have been deposited at a subsequent period?

(3.) Farther, it appears that, in several parts of Liddesdale, red rocks have actually been seen lying above coal-shales and sandstones.

These are some of the grounds on which it has been or may be maintained, that the new red sandstone formation prevails in Roxburghshire. Nor am I inclined to deny, that possibly some rocks more recent than the coal-formation may exist. That the *Old Red* formation exists, is clear from the discovery, in various places, of the fossil fish characteristic of that formation, and from other facts already alluded to. That the *Coal-Measures also* exist, is equally certain;

and that occasionally red rocks are seen lying above these, I think can also be affirmed. But there is no reason to believe, that these several classes of rocks form separate and independent formations. They appear rather to be all members of the same formation, deposited, however, under not exactly the same circumstances. The appearance of red sandstone strata among, and even above, the carboniferous strata, may be explained by supposing, that the causes which had previously led to so large and constant a deposition of red sandstones, though diminished, had not altogether ceased. At various places in the county, the old red formation can be seen passing into the coal-measures, by a blending and intermixture of the strata characteristic of both. Thus, in Mellenden Burn (two miles south-east of Kelso), there are beds of red shales and calcareous marls and sandstones, including beds of conglomerate which contain porphyry pebbles. In Sunlaws quarry, on the Teviot opposite to Roxburgh, there will be seen red sandstone strata, overlaid by blue and brown marly strata.

II. *Unstratified or Igneous Rocks.*

These I divide into three classes,

1*st*, Felspathic rocks.

2*d*, Tuff or amygdaloidal rocks.

3*d*, Augitic and hornblende rocks.

Of these three classes, the first is by far the most extensive and remarkable; in proof of which, it is only necessary to mention, that the Cheviot and Eildon Hills, as well as many others in the county, belong to it.

In each of these classes, two epochs of eruption are apparent, judging by the effects produced on the contiguous stratified rocks.

In describing these several sets of igneous rocks, I shall endeavour to indicate their ages by a reference to this test.

1. The Felspathic Rocks present, generally speaking, various shades of a yellow, and sometimes reddish-brown colour. Occasionally they present purple and lilac hues. They form rounded and dome-shaped hills, very different from the elongated and occasionally precipitous greywacke hills.

(1.) Following the division above explained, as to the age of these rocks, I observe, that the Cheviot and Eildon Hills appear to have been formed at an era prior to the deposition of the old red sandstones, as these stratified rocks are, in many places, some of which will be immediately mentioned, seen close to the felspar rocks, without exhibiting any change either of dip or of texture.

Cheviots.—It is impossible to describe all the varieties, they are so numerous, and so complex in themselves, of porphyry which abound in these hills. I am not aware of there being any greenstone or basalt among them. The rock consists almost entirely of clay and felspar, exhibiting in general an earthy, seldom a compact or crystalline structure. The latter variety seems to occur only in the very central parts of the range, composing several entire hills near Hownam, Attonburn, and Yetholm. The rock of these hills has a dark resinous appearance, and exhibits sometimes a conchoidal fracture. When struck with the hammer, it rings like metal. It is occasionally striped with iron-shot veins of quartz, which afford a pleasing contrast with the black resinous lustre of the rock. It does not easily decompose, and has been used with advantage as a building stone.

The Cheviot porphyries, which present an earthy structure, are generally filled with veins and nodules of quartz. These nodules are often reddened with iron, and, when large, receive the popular name of jasper. Such masses are extracted on the west bank of the Jed, about a mile to the west of Edgerstone, not far from Shaws farm-house. The colours of the earthy porphyries of Cheviot are brown, lilac, purple, grey, and inclining to red. I have never seen any porphyries of the brick-red colour, common in the Eildon hills.

That the Cheviots were thrown up before the deposition of the red sandstone formation, is evident from several circumstances. (1.) At Linton Church, near Morebattle, the conglomerate of the old red sandstone may be seen almost in contact with the Cheviot porphyry, and perfectly horizontal. The pebbles of the conglomerate are chiefly porphyritic. (2.) On Jed water, about two miles west of Edgerstone, and opposite to the farm of Shaws, there are sandstone strata which appear to lie above the old red sandstone conglomerate existing on that farm. These sandstone strata are within twenty yards of the Cheviot porphyry, and unaffected by it. Moreover, they contain rounded pebbles of Cheviot porphyry. Besides being seen so close to the igneous rock, they can, higher up the river, be traced to within a few yards of the greywacke strata, which there run E. by S., dipping at an angle of 86° to the south. At this place there is little or no iron in the greywacke, which probably explains the brownish-yellow colour of the sandstones just referred to. The greywacke strata are here, and farther down the river, of exactly the same yellowish colour. (3.) At Cherrytrees, near Yetholm, the old red sandstone strata are quite horizontal, and quite close to the compact felspar porphyry, of which the hill there is composed, and seem to have been in no way affected by it. (4.) At Blakelaw there is a thick bed of conglomerate, composed of porphyry and greywacke pebbles, and close to a hill of claystone porphyry, which has apparently not altered it.

Eildon Hills.—The westermost hill, on which the Cairn stands, consists of a very hard clinkstone, having a grey basis, and small crystals of felspar interspersed through it. It strikes fire with steel. On the eastmost hill, the rock, though much the same, is not quite so hard. I have specimens of this rock, con-

taining imbedded portions of greywacke,—a fact of itself sufficient to shew, that these hills were thrown up after the greywacke strata were deposited.

These hills are remarkable for the columnar ribs of flesh-coloured felspar which occur on their south-west side, opposite to Bowsden. Some of them exceed 30 feet in length. I do not know any other place in Great Britain, where this species of trap exists in the form of such gigantic crystallization.

Bemerside Hill, which consists of a yellow or buff-coloured felspar, belongs apparently to the same epoch. In this rock, where quarried on the south side of the hill, I observed conchoidal fractures on a very large scale. Some of them form elliptic figures fully 15 feet in diameter. It may be supposed that in igneous rocks, such an arrangement of matter as these conchoidal surfaces indicate, may be readily explained by the process of cooling. But similar appearances, and on nearly as large a scale, are not uncommon in sedimentary rocks, producing what the quarrymen call "*yokes*," or hard concretions.

At Easter Softlaw and Frogden, the felspar assumes the form of a compact fine-grained clinkstone, of a purple colour. This seems to be about the northern limit of the old felspathic rocks of Cheviot.

At Windburgh, the rock is clinkstone, and of a still darker hue. The old red sandstone can, at several places on the *east* side of this hill, be seen within a few feet of the igneous rock, and perfectly horizontal.

On the west and south-west sides of Minto Crag, which is clinkstone of a dark colour, there is a yellow slaty sandstone within 10 yards of it, quite unaffected.

In Ancrum Park (Sir W. Scott's), a bed of claystone porphyry (containing large crystals of felspar) may be seen in a burn below the dog-kennel, overlaid by slaty horizontal strata, which appear to consist partly of felspar, derived probably from the decomposition of the subjacent beds. Over these the red sandstones are lying undisturbed and unaltered.

(2.) In regard to felspar rocks of a later date, I would, in the first place, refer to an elevated mass or sheet of felspar porphyry, stretching across the Teviot and the Tweed from the south by Springwood Park to Mackerston. The texture of the rock is there coarse and friable, and not nearly so crystalline as the trap-rocks last described. There is, however, a vein in it (to be seen in the channel of the Teviot above Springwood Park summer-house) much more hard and fine-grained. This vein is in one part red, and in another grey, in its colour. It is visible for about 20 feet, running by compass west 4° north. It is from 6 to 8 inches wide.

This flow of felspar porphyry has produced remarkable effects on the strata adjoining it, both in the Teviot and in the Tweed. The coal sandstones have evidently been made to undergo great changes in their internal structure. Near the porphyry they are highly crystalline, and the calcareous matter has separated from the siliceous, in a way altogether unusual in this rock, except in such situations. The trap appears to have flowed among and between the stratified rocks, and thus partakes of their dip,—as may be well seen near Roxburgh and Sprouston.

In the Tweed, about $1\frac{1}{2}$ mile below Mackerston House, on the north bank, the marls and sandstones of the coal formation are hardened in a somewhat similar, though not in so striking a manner.

These sheets of porphyry have in all probability flowed down from the Cheviots, but at a period subsequent to the elevation of the Cheviot proper. Accordingly it is found, though in a less friable state, at Hightown, about a mile to the south of the Teviot. In a quarry of it south of this village, I noticed a vein of compact red felspar running north-west, cutting through the more shivery rock. I picked out a quantity of earthy copper-ore from the sides of this vein.

The felspar porphyry (already described) at Sprouston, Kerchester, Hadden, and Carham, lower down the Tweed, belongs evidently to the same epoch, and has produced effects precisely similar on the strata of sandstone and marl which occur there on both banks of the Tweed. At Hadden, near Sprouston, and at Sucklawrig, near Mackerston, the trap has a green earthy basis, having numerous plates of brown mica disseminated through it.

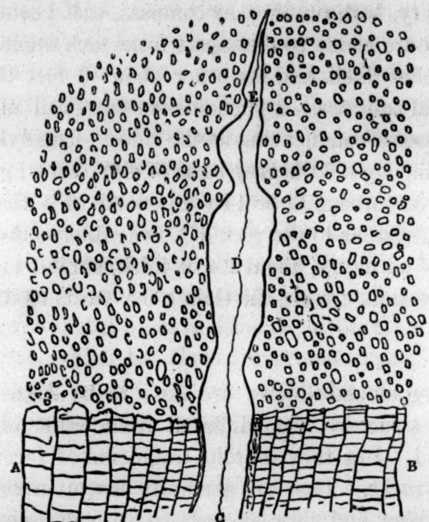
It rather appears to me, though I am by no means confident, that the Black Hill of Earlston, sometimes called Cowdenknows Hill,* also belongs to this later epoch. It is a felspar porphyry, the colour of the rock being, like that of the Eildons, brick red. The red rocks reach to within about 200 feet of the top of this hill, and dip from it at a considerable angle.

There is a small patch of felspar precisely similar to that composing Cowdenknows Hill, on the west side of the Leader, near Clack-mae, as shown on the map; and there are some remarkable dykes, also of the same rock, which, in various parts of the county, may be seen traversing the old red sandstone strata. I shall mention some of the places where I have observed these.

Just below Chapple (on the Leader), a felspar-dyke of a greyish-red colour, and about 18 inches wide, cuts through the old red sandstone conglomerate, in a direction east by south and west by north. It corresponds exactly with the strike of the greywacke strata at this place.

Below Carrolside (also on the Leader) the edges of the greywacke strata stand up about 6 feet above the channel of the river, and are covered by the conglomerate of the old red sandstone, which here forms a bank not less than 200 feet high. At this place a dyke precisely similar in texture and colour to the one above noticed, and about 6 feet wide, is seen shooting up from between the greywacke strata, and piercing the conglomerate. The greywacke strata here are very nearly vertical, and run east by north. The dyke runs in the same direction. The following section is meant to illustrate this interesting meeting of rocks.

* On the top of this hill are the remains of a vitrified fort, consisting of two ramparts. The rock was well adapted for being fused, from the quantity of alkali it contains,—a quality of which the manufacturers of these forts seem to have been well aware, as the stones in all vitrified forts are of this description.



AB are the greywacke strata, having the dyke CDE interposed between them. In some places the dyke is contracted in width. It does not rise to the top of the conglomerate, and it narrows as it rises. A vein of quartz, chlorite, and some other minerals, runs down the centre of it, which is common enough in trap dykes, and is supposed to have been formed in the process of cooling.

In the Dinlee Burn, one of the tributaries of Hermitage Water, there is a similar felspathic dyke, about 15 feet wide, rising out of the greywacke, and cutting across the superincumbent red sandstones. It runs, however, in a north-north-west direction. The strike of the greywacke there, is east-by-north and west-by-south.

The dyke at Winshielknow, already referred to, belongs to this epoch of felspathic eruptions.

About a mile below Galashiels, there is a dyke of yellow felspar, which runs east and west, following the strike of the greywacke.

At the foot of Easter Burncleuch, there is also a dyke, about 3 feet wide, running in a north-north-west direction.* In a quarry of greywacke west of Galashiels, on the turnpike road, a dyke of porphyritic claystone, about 18 inches wide, may be seen running east and west. Mr Kemp of Galashiels also informs me that a porphyritic-dike, about 100 yards wide, runs from the Eildon Hills to the hill south-east of Gledswood House. I have noticed a mass of porphyry in the river about half a mile south of Old Melrose.

In the Allan Water, about half a mile above Fairy Dean, there are two ver-

* These two dykes I have not myself seen. They are noticed in a report by Mr Farey made out for the Duke of Buccleuch, dated 1816.

tical dykes of porphyry, both running, by compass, east ½ south between the greywacke strata. They are about 20 feet apart from each other. The southmost of them is about 18 inches thick, and the other about 3 feet thick. The latter is disposed in horizontal columns,—a phenomenon now well understood to be the effect of a cooling process commencing at the sides. The dyke last mentioned is a felspar-porphyry, having a dark-grey basis of clay, with large white crystals of felspar imbedded in it. About 12 or 14 yards north of it, there is a knoll of claystone-porphyry, very similar to the rock of Cowdenknows and Gledswood Hills.

Another dyke of porphyry, about 2 feet thick, similar to the above, and running in the same direction, crosses the Gala a few yards to the west of Galashiels Bridge.

2. Tuff or Amygdaloidal Rocks, are next to be described, having an appearance as well as structure, very different from either of the two classes of rocks just mentioned. But they contain fragments of various sizes, apparently derived from these rocks. They are generally brown in colour, and of various shades, sometimes lilac and lightish red. This rock derives its characteristic structure from containing, besides the fragments just alluded to, almond-shaped concretions, of all sizes, supposed to have been originally bubbles of gas in the erupted lava, subsequently filled with various chemical precipitates.

Amygdaloid or tufa, apparently thrown up since the deposition of the sandstone formation, occurs on the north-west shoulder of the Eildon Hills (where it is extensively quarried),—at Holm House, nearly opposite to Dryburgh,—in the three green hills of Minto (including the one at Standhill),—at Dinlee Burn, and above Windshielknow (where beds of amygdaloidal-porphyry are interposed between the strata of red sandstone),—in Ancrum Park (where there is a similar bed),—and at Ancrum Craggs, where there has been a considerable outburst.

The age of the Minto Hills can be shewn pretty distinctly, in the *first* place, by the rapid dip from them of the stratified rocks. On the west side of the westmost hill, and close to the amygdaloidal rock, there is a small-grained conglomerate, or very coarse sandstone grit, of a brown or yellowish colour (apparently a member of the coal-measures), dipping at an angle of 60°. In the quarry behind Minto House, the strata, even at a considerable distance from the hills, dip at an angle of about 40°. In the *second* place, the sandstone strata in contact with and adjoining these hills, are much harder than usual. In the *third* place, the trap of these hills contains fragments of greywacke-slate and of Eildon porphyry.

At Ancrum Craggs, in like manner, fragments of Eildon porphyry, greywacke, and sandstone are found in the trap.

Besides the amygdaloidal trap on the north-west shoulder of the Eildon Hills, a breccia, or fine-grained conglomerate, occurs there, which is apparently igneous; the enclosed pebbles consisting of ancient porphyry, greywacke, and a variety of other substances, probably much altered in their appearance and texture by heat.

The amygdaloidal tufa of Holm House, which is about a mile to the south-east of the Eildons, appears to be much the same sort of rock as the tufa just described. The Holm tufa presents a wall or face, which runs north-west and south-east, and seems to run across the Tweed, forming a rapid. It effervesces slightly with acids. Where most compact, this rock contains white and yellow crystals of felspar. It contains also angular fragments of clinkstone and ancient porphyry, and probably greywacke. This tufa may be traced for a considerable distance up the burn which here flows into the Tweed. On examining the yellow sandstone rocks, which are close to it on the south side of it, there are appearances, in the structure of them, which indicate that they have been acted on by heat.

At Moorhouselaw, about 2 miles south-east of Maxton, there is a sheet of amygdaloidal porphyry, which has flowed over and among the red sandstones. There is, in this porphyry, great abundance of chlorite and other minerals, formed probably by chemical precipitation.

3. The Augitic and Hornblende Rocks are next to be described.

(1.) Among the oldest of this class, must be placed the well-known hills called Ruberslaw, Bonchester, Dunion, and Peniel. The rock at Ruberslaw is greenstone, containing steatite. That on Bonchester is basalt, containing crystals of augite.

Around the hills just named, the red rocks prevail, and, up to within a short distance of their summits, remain on all sides pretty nearly horizontal. At Peniel Heugh, they are to be seen within 50 feet of the summit.

There is a range of greenstone and basaltic rocks between the Fly Bridge and Smailholm, which are to a considerable extent covered by the red rocks—also here horizontal. They bear the name of Black-craigs and Black-dykes, derived probably from the dark colour of the basalt. The farm-steading of Sandyknow is built on horizontal strata of red sandstone, and within 20 yards of the greenstone rock, which seems to have in no respect affected the sandstone. These trap rocks appear to continue eastward by Nenthorn to Stitchell and Hume Craigs in Berwickshire. At Stitchell the basalt occasionally exhibits red stains, whether caused by iron or red felspar I do not know. It contains also some beautiful crystals of black glassy hornblende; also of several other minerals, which are unknown to me. One of these specimens I shewed lately to my friend Mr Jameson Torrie, who states that it contains a morsel of mineral pitch, having opal imbedded in it.

To the west of Peniel Heugh, there are several protuberances of basaltic and greenstone porphyries, which form with it a connected range. There is a patch on the east side of the turnpike-road near Ancrum North Lodge. Another mass occurs a little to the west of Ancrum House, at the Castle Hill, and at Scaw. The stratified rocks are seen at the place last mentioned, within a few feet of the trap, without being at all affected by it. To the west of Kirklands House, greenstone again occurs; as also about 2 or 3 miles farther west.

At Wooden Hill (Eckford parish) there is a considerable mass of basalt, full of elongated crystals of dark glassy felspar.

Beneath Maxton Manse, there is a fine-grained greenstone, approaching to clinkstone, which does not appear to have affected the adjoining sedimentary rocks.

(2.) Greenstone and basaltic rocks, of a more recent epoch, occur in two forms, viz. in dykes, and in hills, or aggregate masses. On the Carter, there is a large mass of Greenstone lying at a considerable distance above the limestone worked there, and which may be traced for about a mile along the top of the hill. Windburgh Hill consists, on its west side, of fine-grained greenstone, which has there formed itself into columns nearly vertical, and has affected the adjoining coal strata and limestone. At Greena Hill, in Carby Hill and in Tweeden Burn (all in Liddesdale), there are considerable masses of basaltic porphyry, which have upraised the coal-measures surrounding them.

Below Maxton Schoolhouse there is a fine-grained greenstone, which has hardened as well as tilted up the sandstone strata on all sides of it. About a mile above this, on the south side of the Tweed, opposite to Merton House, there is a mass of basaltic greenstone (called Craigoer), which has evidently hardened the sandstone strata. I at one time considered this a dyke, running north-east and south-west; but as it is composed partly of vertical columns, I am now inclined to think that it is an overflow. The sandstone in its immediate neighbourhood has lost its red colour, and become yellow, as near the Holm, before referred to. There is a good deal of greenstone and clinkstone along the south bank of the Tweed, between Craigoer and Maxton, which has hardened the sandstone strata near it.

Dykes of basalt or greenstone are not numerous. There is one which I have traced for about 26 miles, passing through Hawick at its north end, and a little to the south of Edgerstone at its south end. It is, or has been, worked at the following places, beginning at its north end, viz. at a place 1½ miles north of Hawick, Miller's Knowe (1½ miles south of Hawick), Orchard, Ormiston, Kirkton, on muir west of Ruberslaw, Hallrule Mill, Falside, Roughlie, Rink. It may be seen beyond this last mentioned point crossing the Edgerstone Burn, and also the Kale Water near Hindhope.* At this last point it enters the coal-measures of Northumberland; so that even within this county, it intersects the greywacke, the porphyry, the red sandstone, and the coal formations in its course.

Its course is not very regular, especially when within the limits of the greywacke formation. Its general direction towards the north, when viewed from the Miller's Knowe, by the line of its bearing apparent on the surface, is north 63° west. On the muir west of Ruberslaw, its line may be traced pretty uniformly

* The best maps of the county do not, with any sort of correctness, indicate the position of the Cheviot Hills, or even the situation of the farm-houses existing among them. I found it, therefore, very difficult to lay down the dyke in this part of its course.

for about a mile, and gives a direction of north 60° west. Its more general direction, indicated on the map, is north 54° west.

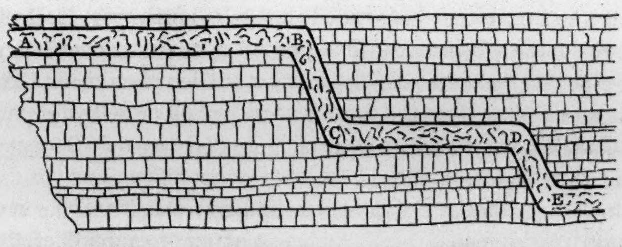
I shall proceed now, however, to give a more particular account of the bearing and width of this dyke, at every part of its course where it is visible, stating at the same time the nature of the rocks which it intersects. The importance of these details, if not here self-evident, will be seen in the second part of this memoir.

Commencing with the south end of the dyke, where it enters the county among the Cheviot hills, I have to mention that its course may be traced down the face of the hill to the Kale Water, which it crosses between Upper and Nether Hindhope. It may be distinctly followed along Eccop Hill, passing not far from the top of it on the north side. Here the Rink quarry (to be afterwards particularly mentioned), situated on the Newcastle and Edinburgh road, where the dyke is worked, and distant from this point about three miles, bears by compass north 55° west. This part of the dyke exhibits on the surface of the muir, huge horizontal columns. Proceeding about two miles west, we find the dyke crossing in that interval several burns, and forming in most instances rocky barriers in their channels. It has been recently quarried on the west side of Edgerstone Burn, which is about half a mile from the Rink, and there the dyke is 36 feet wide, its walls running in a direction north-west by west.

In the part of its course just described, the dyke traverses first the coal-measures, which here stretch over from Northumberland, and reach nearly as far as Eccop Hill. It then enters the porphyry formation, which continues to the Edgerstone Burn just referred to, where it enters the greywacke formation.

At the Rink Quarry, the dyke takes several bends, which are well shewn in the extensive workings to which it has there been subjected. The following woodcut will explain these.

Horizontal Section, shewing course of Basaltic Dyke through Greywacke Strata.



The course of the dyke is indicated by the letters ABCDE,—the distance from A to B being about 100 yards,—from B to C 8 yards,—from C to D 20 yards,—from D to E 6 yards.

The dyke has been quarried from A to E, patches only being left here and there, and especially at the angles, where the rock proved not to be good for road-metal. It will be observed, that it runs most frequently parallel with and between the strata of greywacke, and that it occasionally cuts across them. Its width in the Rink Quarry varies from 16 to 30 feet.

Between the Rink Quarry and the Jed, there are several quarries on the line of the dyke, and which, from the Rink Quarry, bear N.W. 2° N. The rock is not perceptible in the channel of the river; but its course up the face of the brae, on the opposite side, is very perceptible. The progress of the dyke to the northward I shall point out, from a very distinct account of it furnished to me in writing by Mr OLIVER of Langraw, who, at my request, travelled along that portion of its line to be now described. Mr OLIVER states,* that "at Roughlee Nook, the dyke appears to be about 20 feet wide; but where, as in this instance, there has been no section by water or digging, it is very difficult to ascertain the width. There appeared here to be, on each side, 4 or 5 feet of a dark-coloured mass, compact and heavy, but not crystallized like the material of the dyke proper. This is on the border of the porphyritic formation. At Falside, the dyke, 30 feet wide, runs through red sandstone, which is also changed, where in contact, into a material similar to that described above, gradually assuming its natural character as it recedes. The adjoining strata do not seem at all deranged. From Falside, the next point, near Abbotrule, bears nearly north-west by north. Here the dyke is intersected by a burn, and its side exposed for 30 or 40 yards along a bank. The material on each side of the dyke is a red and white (mottled and streaked) sandstone, which at some points is changed into a hard dark-coloured stone, somewhat resembling ironstone, but which gradually regains its natural features as it recedes from the dyke. In other places, quartzy pebbles, without any other very considerable change in structure, indicate the action of heat. There is no discernible change in the direction of the adjacent strata, which, however, are highly inclined, dipping to west by north, at an angle of about 20°. Width of dyke about 20 feet; direction, as far as seen, west north-west; but between this point and Hallrule Mill, the direct line runs nearly west by north. At Hallrule Mill, the dyke cuts through the sandstone, without, so far as can be seen, effecting any great changes. From this, to the next point of view at Glen planting on Caver's estate, the course is due west. Here the dyke is seen for 200 or 300 yards running west north-west, and is 18 feet wide, when suddenly it changes to 30 feet for a short distance, and then suddenly reverts to its previous dimensions (18 feet), and at the same time changes its direction to west south-west. The dyke at Glen Quarry has been quarried out to a considerable extent, and to a depth of about 30 feet —the sides, nearly perpendicular on either hand of the dyke, being left standing.

* It is proper to premise, that the bearings given by MR OLIVER are *true*, and not *magnetic*.

Here are seen to advantage, the effects on the adjacent materials, which, in immediate contact, are converted into a very hard and dark-coloured stone, with some crystals and seams of felspar. This quickly changes to a lighter colour, and, at the distance of 5 feet, consists of a red shaley sandstone, approaching to what we call dent, and rapidly crumbling down to red earth, where exposed to the atmosphere. From Glen Quarry to Tofts Hill, the bearing is west; and from that to Kirkton, west by north. The dyke is seen nearly all the way across the Tofts Hill, which is composed of greywacke, through which the dyke passes *nearly at right angles to the direction of the strata*. The course of the dyke is jagged and irregular, the irregularities on each side bearing a striking coincidence, and, in some places, especially at Kirkton, when the metal is taken out, if the sides were brought together, it is evident that they would fit exactly. The greywacke does not seem to have undergone much change beyond semifusion and distortion for a very short distance, with now and then some of the matter of the dyke injected into its fissures; width ranging from 10 to 25 or 30 feet. At Kirkton, the dyke (15 feet) suddenly takes a south-west direction, which it maintains whilst in view, about 100 yards."

"From Sunnyside, where it is 27 feet wide, the dyke runs, for a few hundred yards, west by north. It is again seen at a short distance, in the bottom of a deep hollow; and from the last point to this, the course is west, when it changes, and runs for 200 or 300 yards west by south, width 24 feet. The next point, perhaps 260 yards, bears west north-west. Next point, 200 yards, west north-west, 21 feet wide. Next point, 200 yards, west by north, width about 22 feet. From this it bears west a short distance, when it changes to west by north; in which direction it continues upwards of a quarter of a mile, when it abruptly turns, and runs about 300 yards north north-east; at least, the next point that I can discover bears in that direction; and I thought I could make out the angle it formed in changing to north-west by north, in the direction of Miller's Knowes. At Miller's Knowes, the dyke curves from north-west by north to north-west, width from 15 to 18 feet."

"Here my survey terminates; but I know that the dyke runs across the common haugh at Hawick, and from the old workings at Miller's Knowes, bears about west by north. It is seen not far to the north of Wilton Lodge,—at a point somewhere between Whitehaugh and Wilton Burn,—a short distance to the north of Mabenlaw and Whitecleughside, and at a point nearly a mile from Borthwick Shiells."

Mr Oliver adds, that the general strike of the greywacke strata, is *south-west by west*, by true bearings. At the Miller's Knowe, therefore, the dyke which, by true bearings, runs *north-west by north*, must cut across these strata at exactly a right angle,—just as in the Rink quarry before described. The general course of the dyke, however, is, as already stated and shewn, oblique to the strike of the

greywacke strata, forming, indeed, very nearly half a right angle with them. If, therefore, it occasionally takes a course at right angles to the greywacke strata, it must, in order to preserve its average direction, also occasionally take a course parallel with them; and, accordingly, it has been shewn that this is the case. Nor is it difficult to understand, why this irregular course should have been taken, if the dyke only fills up a rupture of the earth's crust. This rupture would follow the lines of least resistance,—and these, it is plain, must be either parallel with, or directly across,—and not obliquely across the strata. In the less compact formation of red sandstone, there would not be the same difficulty of rupture; and hence within its precincts, the course of the dyke is not so irregular as in the greywacke formation.

This dyke varies in composition, not merely in different parts of its course, but even at any one spot, according as the specimens are taken from the sides or the centre. At the sides the trap is fine granular, almost approaching to clinkstone, and occasionally it is vesicular; whereas in the centre the texture is coarse but compact, and highly crystalline. The crystals are sometimes pretty large, consisting chiefly of quartz, and more rarely of glassy felspar. These differences of structure can be readily explained by the difference in the rates of cooling in the different parts of the dyke.

On the south bank of the Tweed, a little above Merton House, there is a mass of greenstone, which has upraised and hardened the contiguous sandstone strata, and a portion of them may be seen entirely enveloped in the trap. This rock has all the appearance of a dyke, though of this there is (as I have already mentioned) no certainty, in consequence of its not being traceable for any great distance.

A little below the Manse of Castleton, on the south bank of the Liddell, there is a mass of greenstone about 20 or 30 yards wide, which has upraised the strata, and appears to be a dyke, running about west-north-west. At Larriston lime quarry, a similar dyke may, I understand, be seen running in the same direction. It is marked on the map.

There is one other subject which ought here to be noticed, as common both to stratified and unstratified rocks. I allude to the *joints* which intersect them.

This is a point which of late has been attracting a good deal of attention, and not more than it seems to deserve. But it is one which can be properly worked out, only after an immense accumulation of observations, and a careful classification of them, which in this county I have not been able to make, and which I regret the more, as the observations I *have* made are very encouraging.

At the Limekiln edge, the principal joints form fissures no less than 5 inches in width, and are filled with a fine yellow clay. The minor joints at right angles to these, form fissures about 4 inches wide. The former run north 55° east, the latter north 35° west. In the limestone of Penton Linns (near Canonby), the prin-

cipal joints run about north-east, the others about north-west. In the coal-seams at Rowanburn (as Mr Gibson the manager informed me), the principal joints (or backs as the colliers term them) run about due north, and the others due west. Mr Gibson states, that these backs and cutters are independent of several large faults which intersect the coal-seams, and which appear to be of subsequent origin. He thinks also, that these joints are all independent of the dip and rise of the coal-seams.

In the greywacke, I have observed joints at only a few places. At Carolside Bridge, there are numerous parallel joints intersecting the flesh-coloured greywacke strata, in a north-north-west direction; and a similar system of joints prevails in the light blue strata at Clackmae. At Langholm Bridge, there are numerous joints traversing obliquely the greywacke strata in a west-north-west direction, and mostly filled with spar, which occasionally contains lead.

There is thus a remarkable uniformity in the direction of the structural joints of the stratified rocks,—a direction apparently quite independent of their dip, and formed at a date subsequent to their deposition.

I may here also take notice of a vein on the south bank of the Tweed, opposite to Birgham, intersecting horizontal strata of clay and marl, and varying in its width according to the nature of the strata passed through. In those marly strata which contain a good deal of lime, the vein is from 2 to 3 inches wide, and consists of crystallized carbonate of lime. In the other beds of dark red or brown clay, which have no appearance of lime in them, the vein becomes a mere crack of about ½ inch in width, and has no mineral contents.

III. Lastly, I have to notice the Post-tertiary and Diluvial Phenomena, in so far as at all remarkable in Roxburghshire.

(1.) The oldest of the post-tertiary deposits is what has been termed Boulder clay, because characterised by containing, interspersed through it, large boulders or rounded blocks of stone. This deposit may be seen on the banks of the Leader at different places, and also near Sprouston. It does not appear to exist in the higher parts of the county.

At Sprouston freestone quarry, there is a good section, shewing a bed of boulder-clay from 2 to 8 feet thick, lying upon the sandstone,—then a bed of fine clay or silt free from pebbles, from 1 to 2 feet thick, lying over the boulder-clay, —and, lastly, a bed of small gravel, from 4 to 5 feet thick, immediately under the soil. The boulders are all rounded, and consist of greywacke, porphyry, and basalt.

In regard to the existence of boulders on the surface, there are not many places where they are in any abundance; though it is more than probable, that, before agricultural improvements commenced, the whole county had been covered with them.

Rounded blocks of grey granite occur in several parts of Liddesdale, as in the fields and moors near Castleton Manse, where I saw several from three to four feet in diameter. On the east bank of the Esk, about two miles below Langholm, granite boulders, of the red as well as grey variety,* some of them very large, are to be seen. A number also occur in the Gill Burn, which flows into the Liddell above its junction with the Esk. These granite blocks are lying on the greywacke-formation, as well as on the coal-measures. Now, the nearest known hill of granite is Criffel, which consists almost entirely of a grey granite, precisely similar to that composing the boulders in question. Criffel hill is situated about twenty miles to the west of these boulders. The next nearest place where granite occurs *in situ*, is in Kirkcudbrightshire, at Loch Doun, which is at least sixty miles distant.

A pretty large boulder of greywacke was noticed by me, about 200 feet below the summit of Ruberslaw. It rests on the red sandstone strata, and very near their highest level. No greywacke rocks occur *in situ* nearer than three miles to the westward, between which place and Ruberslaw there is low ground, at least 800 feet beneath the level of this greywacke boulder. Moreover, the greywacke rocks, at the place above alluded to, where they occur *in situ*, do not rise to so high a level as that of the boulder in question.

To the north-eastward of Ancrum House about one mile, there is an immense accumulation of trap-boulders. They appeared to me to be composed of the basaltic porphyry, which exists at Kirklands and Castle Hill, situated about two miles to the west-south-west.

To the east of Cowdenknowes Hill, many large blocks of felspar porphyry, consisting of the same kind of porphyry which forms the top of that hill, are strewed over the muirs, resting on the old red sandstone strata.

In the burn on the north side of Toft's House, about $\frac{3}{4}$ mile east of Edgerstone, I found several irregular blocks of greywacke, resting on a reddish-purple porphyry rock. The nearest point where greywacke exists *in situ*, is about half a mile to the west, between which, however, and these blocks, there is a porphyry hill several hundred feet high. There is no greywacke to the south or east, which are the only quarters from which any glacier could have descended, according to the existing levels of the country.

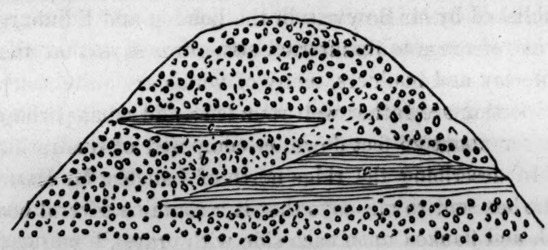
Near the sides of the old road which runs from Jedburgh to Crailing, a great abundance of basaltic boulders may still be seen. There is no hill, situated to the east or south, which could have produced them. The basaltic hills to the west and north-west, are those from which they must have been, in all probability, derived.

Nothing is so remarkable in this county, as the uniform manner in which the steep side of a hill faces to the *west*, and the accumulation of gravel and other

* This fact is taken from FAREY's Report before mentioned.

loose materials exists on the *east* side of it. This phenomenon, which the late Sir James Hall so well denominated Crag and Tail, prevails in Liddesdale as well as in Teviotdale, and, therefore, on both sides of the summit-level of the country. In Liddesdale, examples are afforded in the basaltic hills of Carby, and another up the Tweeden Burn, of which I have forgotten the name, if it has one. In Teviotdale, there are still more remarkable examples afforded by Bonchester, Dunion, Peniel Heugh, and Castle Hill near Ancrum; and also, farther north, by the Eildon Hills, Bemerside Hill, Cowdenknowes Hill, and the basaltic ranges near Smailholm.

There are in this county a number of those remarkable accumulations of gravel and sand, which have of late become objects of increasing interest, on account of their resemblance to moraines of glaciers. These accumulations are sometimes disposed in the form of isolated mounds, and sometimes of long ridges, which last are called Kaims by the country people. The most distinct of these is at Liddell Bank, between the river Liddell and the turnpike road from Castleton to Canonby. The ridge is about half a mile in length, about 200 feet wide at its base, and from 50 to 60 feet in height. It forms pretty nearly a straight line, running north-east by east. It is not quite parallel to the course of the river, its eastern extremity being farther off from it than its western extremity. The ground on which it has been deposited, slopes towards the river, and, of course, therefore, the ridge does not form a level or horizontal line. A considerable burn, called the Mere, joins the Liddell a little to the west of this gravelly ridge, flowing from the eastward; and the ridge is situated on the high ground between the two valleys of the Liddell and Mere. At its upper or eastern extremity, the height of the ridge above the adjoining ground diminishes gradually, and is finally lost in the side of a pretty high hill. The relative position of this hill and the ridge is such, that if a stream or rush of waters had passed over the country from the north-eastward, the ridge in question would have formed a ridge on the *lee* side of the hill. The situation and direction of this ridge are indicated on the map by a blue streak.* The accompanying section is taken from one part of the Kaims, where it has been quarried,



apparently for sand or fine gravel. *a* is large gravel, *b* is fine gravel, *c* is sharp sand,

* It has been found impossible to introduce this mark into the accompanying map, on account of the smallness of the scale.

disposed in nearly horizontal layers. Among the gravel, I observed pebbles of red and grey granite, as well as fragments of sandstone, shale, and coal.

Another set of ridges of the same kind occurs about $1\frac{1}{2}$ mile north of Kelso, on the road to Stitchell. Their sides are steeper than the one just described, but they are not so high, and do not form for such a distance an unbroken line. A small stream runs along one side of the longest of these ridges. There are several other ridges of gravel in this neighbourhood, which have given the names of Kaimknow and Kaimflat to farm-houses near them. Several pits have been opened in them, for the sand and fine gravel contained in them, disposed in horizontal beds, some of which are about 15 feet long.

A similar ridge of gravel, about 50 or 60 feet high, occurs between Ormiston and Eckford, on south side of Teviot, running nearly half a mile in a west-north-west direction, and nearly parallel with the Teviot in this part of its course. There is another to be seen on the south side of Jedburgh.

In the neighbourhood of Galashiels, there are a number of knolls and ridges, which have by some persons been represented as the remains of glacial detritus. Within the policy of Gala House, there are several of both kinds pretty distinct, though on no great scale. Mr KEMP, in a written account of them which he sent to me, says, " there is a quarry in one of the largest, which shews it to be composed, at that part, of well rolled coarse gravel, mixed with much sand, but not at all stratified." I visited these knolls, but unfortunately did not see the quarry which is here alluded to. I remarked, that more than one rivulet was running along or near the base of these gravelly knolls and ridges.

Besides these knolls and ridges in Gala park, there are others, no less remarkable, lower down the valley. One of them, about half a mile east of Galashiels, is at right angles to the course of the valley, and runs for about 200 yards. It has already been designated the *terminal moraine of the Gala glacier!* There is another still longer, situated immediately to the north of Langlee House, where Captain RUSSELL ELLIOT resides. It runs parallel to the valley, and has been dignified with the title of *a lateral moraine.* Mr KEMP, in an account of the first of these ridges, published by Mr BOWMAN in the London and Edinburgh Philosophical Magazine,* in reference to its internal structure, says that the greater part " is composed of clay and boulders, many of which are quite sharp and angular, but the greater portion are rather well rounded: and what perhaps is worthy of notice, the top, for about 25 feet down, is composed of unstratified gravel and coarse sand." In describing the ridge north of Langlee, Mr KEMP, in a written account which he has sent to me, says that it " contains several beds of sand distinctly stratified, and flanked upon each side with gravel."

Whether these accumulations of sand and gravel are really the remains of glaciers, will be considered in the second part of this Memoir.

* Vol. xvii. p. 339.

There are, in many places, indications that the rivers, in this country, flowed formerly at a much higher level than they ever now reach. On the north side of the Tweed, for about a mile above its confluence with the Teviot, there is an extensive flat, about 70 feet above the ordinary level of the river, extending back probably half a mile from the river, and there bounded by an abrupt bank, which runs for some distance parallel with the river. On this elevation, flat, or terrace, Floors Castle stands; as also, a part of the town of Kelso. The terrace, at its side nearest the river, has a steep face or front, about 15 or 20 feet high, at the foot of which there is another and lower terrace, intervening between it and the Tweed.

At Castleton, there is a steep bank, from 50 to 70 feet high, which runs on the north side of the town for about two miles nearly parallel with the Liddel, and which, at some former period, has evidently been the north bank of the river, but which is now, for a considerable distance, more than a mile distant. The base of this cliff is from 30 to 40 feet above the ordinary level of the river.

It is proper here to take some notice of those curiously shaped stones in the valley of Allan Water, known by the popular name of Fairy Stones. They are most commonly in the form of flattened spheres, and, though generally separate, they are sometimes united together. They consist of a brownish-white clay, hard in the mass, though easily scratched with a knife. They effervesce very briskly with acids, and they appear from their colour also to contain a small proportion of iron. They are found in the channel, but more frequently on the west bank of the river, at the edge of the stream.

Various opinions have been offered, to account for these stones; some supposing that they are formed, like stalactites, by the dropping of water; others, that they are fragments of some hard rock, worn by aqueous attrition. My own opinion is, that they are mere concretions in finely laminated clay, of which there is a large bed on the west bank of the river. But deferring till next part of this Memoir, the grounds of this opinion, I may only here mention, that the bed of clay from which these fairy stones are derived, is overtopped by a mass of gravel, the weight of which, aided by the infiltration of water, causes constant slippings into the river. The clay thus exudes or is squeezed out into the stream.

The clay is extremely tough and plastic. It resembles exactly the clay found near Berwick, on the left bank of the Tweed, where stones of the same lenticular shape are found. In both places the clay is finely laminated, and equally tenacious, indicating originally deposition in still waters.

In the channel of Kale water, near Morebattle, stones, having characters in many respects similar to those just described, occur, though of a very different shape. They are not spherical but elongated—sometimes as long as 14 to 18 inches, and with a transverse diameter of only 3 or 4 inches. They have precisely the same colour as the fairy stones of Allan Water, and present like them laminæ of strati-

fication. They seem also to be derived from a bed of clay on the west bank of the river.

Before concluding this part of my memoir, I think it right to take notice of some other phenomena, which have lately been brought into notice by the industry of Mr Kemp of Galashiels. He was the first person who drew attention to a number of terraces, on the sides of hills in the neighbourhood of that town; and, on examining the relative levels of these terraces with an instrument, he found almost no case in which a terrace on one hill did not correspond in level with one or more terraces on other hills. Mr Kemp considers that he has discovered no less than fifteen or sixteen terraces, at different levels, and maintains that they have had the same origin as the parallel roads of Glenroy. The height is represented as being about 1300 feet, and the lowest 500 above the sea; so that there is, on an average, about 50 feet of perpendicular height between each terrace.

I regret not having had an opportunity of examining fully these phenomena; for though I am by no means convinced of the correctness of Mr Kemp's conclusion, or of the facts on which he relies, neither is there any thing, on the other hand, which satisfies me that he is mistaken. In fact, I had myself some years ago been much struck, when on Ruberslaw, with a terrace near its top, on the north side, which appeared to correspond in level with one on the Dunion and another on the Eildon Hills. But it was only with a pocket spirit-level that I made the observation, and considering the distance of these hills from one another, a very small error either in the instrument or in the observation would (independently of refraction) cause a considerable difference in the levels deduced.

Notwithstanding, however, the difficulty of ascertaining whether these terraces were exactly on the same level, it would have been a circumstance strongly indicative of their supposed origin, had there been no abundance of such marks on other parts of the same hills. If on twenty hills in the same district there were terraces all very nearly on the same level, and on no other parts of these hills, it would have been difficult to have resisted the conclusion, that they had all been simultaneously produced by a common cause. I soon found, however, that upon almost every hill-side, there were many such marks,—an observation fully confirmed by Mr Kemp, who describes no less than fifteen or sixteen terraces, distant from each other only forty or fifty feet. I was thus compelled to seek for other evidence of their origin.

I proceeded to examine the terraces themselves; and observed, that whilst many of them appeared to be perfectly horizontal, several of them had a decided slope. Indeed, I observe from an account of them by Mr Kemp, with a perusal of which he has favoured me, that "in almost every case, whether these shelves are of greater or less extent, their extremities are rounded over, by bending down hill for some distance; so that, after repeated examinations, we were at last obliged to abandon the idea, of water alone having run out those terraces." Mr

Kemp then goes on to suggest, that icebergs may, by bumping on the sides of the hills, have produced the phenomena. But I quote the above passage, as shewing the opinion of an accurate local observer, that few or none of the terraces in question are quite horizontal.

At the same time, it is possible to suppose, that accumulations of gravel and sand, formed on the beach of a sea, may, by subsequent denudation, have been worn down in some places so as now to present a surface sloping either *from* the hill, or *along* its side. On the whole, however, the evidence derived from horizontality was so doubtful, that I could not venture to place much reliance on it.

The next point to which I attended, was the nature of the materials composing these terraces. But I made little progress in this inquiry, as the interior of them is seldom exposed. From the slight insight, however, which I did get into the structure of some of them, it appeared to me that they were not sea-beaches, at least of the character or comparatively modern date which has been suggested. I found that on Ruberslaw, Dunion, Cowdenknows, and the Eildon Hills, these terraces were composed chiefly of red soil derived from decomposed strata of old red sandstone; and that, in fact, they indicated the upper limits of this formation. In the small burns and sheep-drains which intersect the terraces on these hills, soft strata, chiefly horizontal, are to be seen,—in almost all cases of a deep-red colour; and, on the north side of the Eildons, containing occasionally a brown-coloured and gritty-coal sandstone. It is, therefore, not improbable, that these shelves have been formed so far back as the time when the sedimentary rocks just alluded to were deposited, the land being then at least 1100 or 1200 feet lower in level than at present. On that hypothesis, but on that only, is it possible to explain the fact, that the upper limits of this formation should be manifested by terraces of its debris all nearly, if not exactly, on a level.

So far, then, I am inclined to admit that there is evidence existing on the Roxburghshire hills, of the sea having formerly stood at a far higher level than at present. This evidence depends, however, in my humble opinion, entirely on the fact of the terraces in question being the upper limits of the red sandstone formation; and therefore it indicates terraces only at one particular level. I can see no evidence to shew successive levels, at which the sea reposed so long as to form other beaches, though of these Mr Kemp thinks he has discovered above a dozen.

I must add, however, that even this evidence of a single sea-beach or sea-level, is not altogether free from doubt; for it is not yet to my mind matter of absolute certainty, that the upper limits of the sandstone formation, as shewn on the Roxburghshire hills, do all coincide in level. My present impression certainly is, that they do coincide, at least so near, as to afford strong presumptive evidence of a common origin. But it would be desirable to test more rigidly the accuracy of this observation.

Of course, also, if the sea stood at the height of these upper limits of the red sandstone formation, it may be expected that, even in places where that formation was not deposited, some marks should also have been made, and should still be visible on the hill-sides. The marks there may not be so distinct, for very obvious reasons; but still there should have been some abrasion of the greywacke and porphyritic hills, similar to what occurs in Glenroy. Mr Kemp will say that such marks do exist in the neighbourhood of Galashiels; and I by no means deny this. On the contrary, it appeared to me, when visiting the locality in company with Mr Kemp, that on Galashiels Hill, Buckholm Hill, Williamlaw, Meigle Hill, and Appletreeleaves Hill, there are marks of shelves, which are on nearly, if not exactly, the same level with the upper limits of the red sandstone formation.

On the whole, therefore, I am strongly disposed to think, that there yet remain, on the hill-sides of Roxburghshire, visible marks of the sea having stood at a level 1100 or 1200 feet higher than at present, and of its having continued at that level for a very long period. But I see no sufficient evidence of any lower levels at which the sea was stationary before reaching its existing level.

Part II.

Having, in the preceding parts of this Memoir, described the leading geological features of the county of Roxburgh, I shall now advert to the inferences, of a cosmological character, which these facts seem to authorize or render probable.

1. The first indication of important changes is afforded by the greywacke strata,—which, after being, like other sedimentary rocks, deposited horizontally, or nearly so, have been, as if by lateral pressure, pressed and squeezed together, so as to become vertical, with numerous foldings upwards and downwards, in alternating order. In consequence of having been thus compressed, they have formed valleys and ridges, or chains of hills, all running in the same direction, and which direction throughout the whole extent of the greywacke formation is, with few exceptions, east and west by compass.

Now, it can scarcely be doubted, that these effects have been produced by the operation of some force or forces of vast extent, and which could not, in its operation, have been confined to this particular district. The greywacke rocks of Roxburghshire form only part of the range which runs through Berwickshire to St Abb's Head, and through Dumfriesshire and Kirkcudbrightshire to the Irish Sea,—a range everywhere characterised by ranges and valleys, running east and west, and by a corresponding strike of the individual beds. It is well known that the greywacke and clay-slate system of Cumberland, as well as that of Perthshire, present characters precisely similar.

These greywacke strata of Roxburghshire were, therefore, in all probability,

elevated and squeezed into their present condition by forces, which acted over a considerable portion of the earth's crust.

Whether they acted every where contemporaneously, so that the greywacke hills, in the south of Scotland, were raised at the same instant, as those of Cumberland and Perthshire, may be doubtful. For there is no reason why the same force might not act at different places, at several successive periods. But there is certainly strong reason for thinking, that it was the same force which acted on the Grampians, the Lammermuirs, and the Cumbrian chains; (1.) Because they consist of rocks, of apparently the same age, having been all deposited before the epoch of the old red sandstone formation; (2.) Because the effects on all these greywacke chains of hills are precisely similar.

What that gigantic force was, or could have been, which produced effects so remarkable for their extent and their uniformity, is a question too difficult and too general to be entered on here. One theory is, that these greywacke rocks were raised by the effects of subterranean heat. Another theory is, that the internal nucleus of the globe is, from excess of heat, in a molten and liquefied state; and that the temperature of this nucleus diminishes faster than the crust, so that, as the nucleus contracts in size, or even changes in form, the external crust, in order to accommodate itself to what it rests on, must be broken up, and occupy smaller space than before.

Without entering upon the discussion of these two theories, I may observe, that if, as the former implies, the greywacke strata were upturned by volcanic action, it is reasonable to suppose that there would have been large outbursts of volcanic rocks among these strata. But, so far from this being the case, the greywacke formation in the south of Scotland, and especially in Roxburghshire, is, generally speaking, entirely exempt from igneous rocks; and where igneous rocks do exist, as in Ayrshire and among the Lammermuir Hills, the adjoining greywacke formation has lost many of its ordinary characters, as, for instance, the east and west strike of its beds. Indeed, it is not easy to imagine how igneous or volcanic action could have operated, so as to produce the remarkable parallelism of chains and strata which distinguishes this ancient formation. The outbursts of igneous rocks seldom or never form continuous chains, at least of any extent; but, according to the theory now alluded to, one would have expected to have found a central axis of igneous rocks stretching across the island, having on each side the greywacke strata which it had been the means of raising up.

On the other hand, if, from the refrigeration and contraction of the earth's nucleus, its crust became rent and broken, it is easy to see how, through these rents, portions of the molten nucleus might have squirted up, and formed those hills of granite and other ancient igneous rocks which occasionally occur in, and on the outskirts of, the greywacke formation. The effects, therefore, which such a

cause may be supposed to have occasioned, seem to accord well enough with the actual phenomena.

If, then, the convulsive movements which the greywacke system has undergone, be attributable to changes in the earth's nucleus, these changes must have occurred in certain lines, so as to have produced the remarkable uniformity of direction and parallelism which prevails among the ranges and strata of greywacke. But this is a question which lies still beyond the depths of modern philosophy.

2. The next important change in this part of the island seems to have been the eruption of the felspathic rocks which form the Cheviots, the Eildons, and those other hills, shewn in the first part of this Memoir as belonging to the same epoch with them.

It is quite manifest that these felspathic rocks, generally speaking, were erupted long before the great mass of the greenstones and basalts appeared. The same remark holds true in most other districts, as there cannot be a doubt that the felspar rocks of St Abb's Head, Soutra Hill, the Pentlands, the Ochils, and of the hills near Comrie (Perthshire), burst out long before the augite rocks of these several districts.

Whether these felspathic rocks were erupted at the same time with, or immediately after, the elevation of the greywacke strata, is uncertain. At all events, they were not erupted previously, for in many places these felspathic rocks are seen intersecting the greywacke strata, and in some instances containing portions of greywacke rock. Moreover, those places where the strike of the greywacke strata deviates from its east and west bearing, are mostly to be found near the Cheviot and Eildon porphyries. At the same time, the outburst of these igneous rocks seems, to a very considerable extent, to have been controlled by pre-existing vertical strata of greywacke. Thus, most of the felspathic dykes run east and west. Even the larger outbursts, as the Eildon Hills, exhibit, generally speaking, an elongated shape, of which the greater axis runs in the same direction.

It would appear that felspathic rocks, after their first great outburst, continued to be erupted, though in a gradually diminishing degree; for we have seen, that a number of felspathic dykes intersect the conglomerate of the old red sandstone.

3. The next circumstance deserving of attention is, that all the geological convulsions just described, happened when this district was under the waters of an extensive ocean.

(1.) The depressions of the greywacke formation produced by the causes above alluded to, as well as the hollows among the outbursts of felspathic rock, were gradually filled up with the debris of these several rocks exposed to the action of submarine currents. In this way beds of conglomerate, consisting of

rounded pebbles, both from the greywacke and felspathic rocks, were formed; and beyond, as well as above these conglomerate beds, strata of sand and mud were deposited in greater or less abundance.

In regard to the deep red colour by which these conglomerate and other sedimentary beds are characterized, it is pretty plain that it has been derived in some way or other from the greywacke formation. That the red oxide of iron (in the state of a peroxide) prevails largely among the greywacke strata, both in the form of veins of hematite, and diffused through the substance of the rock, has been already shewn. The disintegration, therefore, of these rocks, would produce extensive beds, impregnated with iron; and as such a sediment would be heavier than pure sand or clay, the beds formed by it would be mostly deposited in the immediate neighbourhood of the greywacke hills.

This last inference is proved to be correct from this circumstance, that the red sandstone strata are most abundantly developed, and most deeply tinged along the flanks of the hills. It is at a considerable distance from them, that we find the yellow and white beds of sandstone beginning to make their appearance; and it is still farther off, before we reach the shales, marls, and sandstones of the coal-measures, which present little or no intermixture of iron-shot strata.

This seems to be the most proper place for adverting to the probable origin of those white spots and blotches which are occasionally seen in the old red sandstone formation. Dr FLEMING has suggested that they are owing to the presence "probably of some vegetable or animal organism, the decomposition of which exercised a limited influence on the colouring matter of the surrounding rock."

Being desirous of testing, by chemical analysis, the soundness of this explanation, I requested my friend Dr MADDEN of Pennicuick to do me the favour to examine one of those white spheres, and at the same time a portion of the red stone adjoining it. This experiment he very readily undertook, and the following is the result.

	In Spot.	In Red Sandstone.
Silica,	67.4	63.40
Alumina,	9.8	3.92
Carbonate of lime,	7.8	7.8
Iron,	(Protoxide) 2.5	(Peroxide) 12.76
Phosphate of iron and alumina,	3.4	0.3
Alkalies,	5.2	6.85
Moisture and loss,	3.9	4.97
	100.	100.

Dr MADDEN, in sending this analysis to me, observes, that, "although it does not shew the difference to be so great as I at first imagined, still I should think, from the result, that some powerful de-oxidizing agent had been at work, as it has so completely changed the condition of the iron. In fact, both this and the

alumina were in the spot in some curious condition, rendering them very difficult to separate, so that probably their numbers are not so correct as they might be."

From the foregoing analysis, it appears (1.), that there is in the spot treble the quantity of alumina which is in the adjoining red stone; (2.) that there is in the spot, less than half the quantity of iron which is in the red stone; (3.) that the iron in the spot is in the state of a protoxide, whilst in the red stone it is a peroxide; and, (4.) that there is phosphoric acid in the spot, whilst there is none in the stone.

There are two ways of accounting for the difference between the spot and the red sandstone. The peroxide of iron, which prevails through the general mass of the rock, may never have impregnated the white spot, owing to the presence in it of some body which had the power of repelling it; or the peroxide of iron may have subsequently, as Dr MADDEN suggests, become deoxidized.

In reference to this last theory, it may be observed, that if the abstraction of oxygen from the iron be ascribed to the action of some body previously existing in the heart of the stone, does the analysis above given indicate what that body is? Dr MADDEN infers that there has been "*some* powerful de-oxidizing agent at work;" but, as he does not surmise what this agent was, it is to be presumed that he had been unable to discover it. Dr FLEMING, as above mentioned, suggested that the decomposition of animal or vegetable matter might have decolorized the stone; and, in corroboration of this opinion, I may state, that I am in possession of one specimen, where there is a scale of a Holoptichius in the middle of the white spot. Now, as this scale consists to a great extent of phosphate of lime, it may be supposed, that, on the decay and decomposition of part of the scale, the phosphoric acid would combine with a portion of the iron in the peroxide, and convert it into a protoxide, which is generally of a whitish-grey colour. But whilst in this way the change in the state of the iron may be accounted for, what reason can be given for the other differences between the spot and the stone,—the larger supply of alumina in the former, and the smaller quantity of iron?

Farther, it is deserving of observation that it is only in one case out of a thousand, that any foreign body is discernible in the white spots. And if it is assumed that a foreign body, such as a fish-scale, be the sole cause of the spot, why should its form not correspond with that of the fish-scale, instead of being an exact sphere, which is the form universally exhibited?

I cannot help thinking that the formation of these white spots belongs to the same class of phenomena as the blanching process which takes place along the sides of fissures or cracks in the old red sandstane rock. On each side of the crack there will be found, on breaking off a portion of the rock, to be a ribbon of a greenish-white colour which fringes the red stone. It appears to me that this

is clearly a chemical process, by which the red dye has been to a certain depth discharged,—or, in other words, the iron changed from a peroxide to a protoxide. If this process could be discovered, we should in all probability have a clew to the problem of the white spots.

I may observe, that along these cracks in the red sandstone, there is often a large development of metallic incrustations, having a dendritic form. The iron seems as if it had been withdrawn from the general mass of the stone through which it was diffused, and that thereby the stone was restored to its original white or greenish-white colour. Appearances similar, or at least analogous, to these, are common in the red sandstone rocks, when in contact with or near trap rocks, which had risen through them. The red rocks, in such a situation, acquire a brown and sometimes even a yellow colour;* and on examining with a microscope the structure of the stone, particles of iron are found in a state of crystallization, instead of being equally diffused through the whole mass.

If heat has produced these last mentioned effects, it may equally have produced the similar change which has taken place at the sides of cracks; assuming that these cracks were formed during the desiccation of the rocks, by the influence of subterranean heat.

In several specimens, now before the Society, of these spherical spots, there is a metallic-looking pea in the centre, which would seem to indicate that the iron previously diffused through the spot, had become aggregated into the centre. If, as in the cases just referred to, heat was capable of making the iron separate from the general mass of the stone and form metallic incrustations, might it not have produced the analogous effect in the spot?†

* Two places, not far from one another, where these effects may be observed, are on the right bank of the Tweed, one opposite to Merton House, at the Craigoer rock, and the other opposite to Dryburgh, at the Holm House.

† When this part of my Memoir was going through the press, I wrote a note to Dr MADDEN, stating shortly the views expressed in it. From his answer I make the following extracts, as containing some important suggestions:—

"I have just received your note, and, having considered its contents, would offer the following observations. The idea that suggested itself to me at the time of the analysis was, that the deoxidizing agent producing the white spots, must, in all probability, have been a portion of *organic matter in the act of decomposition*,—this may have been a fish bone or scale, or any other organized body; there are, however, certain objections to this view of the matter, which I will now state.

"1*st*, If the spots were produced by the decomposition of any substance imbedded within its mass, the effect would be produced with greatest effect in the immediate neighbourhood of the decomposing body, and this effect would gradually diminish in intensity as the distance increased; whereas, in the spot, there is an abrupt transition from the deoxidized to the unaffected mineral.

"2*d*, As the intensity of effect would be proportioned to the decomposing mass, and as the distance to which the effect was produced would likewise be proportional, the exterior of the spots should possess a shape either exactly or nearly similar to that of the organic body inclosed; whereas, the spots in question are, without exception, nearly spherical.

(2.) Allusion has just been made to the coal measures, as being separated from the greywacke formation by the old red sandstone group of rocks. I have explained, in the former part of this Memoir, that I do not consider the coal-measures of this district as forming, with reference to the old red sandstone, a separate and independent formation. On the contrary, it appears to me, for the reasons already given, that the two merge into each other by insensible gradations.* It is evident indeed, that, supposing the materials of both groups of rocks to have been derived from the same quarter, they would arrange themselves precisely as they are found to exist, viz., first the sediment loaded with peroxide of iron, and afterwards the finely comminuted clays which afterwards constituted the shales and limestone.

In regard to the old red sandstone rocks of this district, I may farther observe, that they appear to present the same general characters, and even the very varieties,

"3*d*, The fact of the spots being annularly stratified, the rings being in most cases distinct, and easily separable by cleavage, militates somewhat against the hypothesis of the creating cause being placed in the centre, because it is generally found that annularly stratified masses grow by deposition upon a central nucleus; whereas, when a central substance influences a surrounding mass, *previously deposited*, a section generally exhibits *radiations* in place of *rings*.

"With regard to your other suggestion, it is exceedingly probable that the iron was brought in contact with the calcareous sand, in the form of a solution of protoxide, and that the protoxidation was an ulterior effect, possibly of heat. I do not, however, see exactly what state of things could exist so as to prevent the protoxidizing of particular spots, and, at the same time, to change so materially their structure. Some very interesting experiments have suggested themselves to me, by which I fancy we could arrive at a somewhat satisfactory conclusion respecting their origin and formation. I cannot, however, as yet promise to undertake these experiments."

I had suggested, in my note to Dr MADDEN, whether clay or sand, deposited in water which held protoxide of iron in solution, would not, on exposure to heat, acquire a red colour, like common bricks or house-tyles when put into a kiln? The only difficulty is, to explain how, in particular spots, the peroxidation of the iron was prevented or subsequently neutralized. But if organic matter of any kind, (such as fish-bones or scales), containing phosphoric or carbonic acid, existed in these spots, then their organic matter would become gradually decomposed, and the acid being set free, would combine with a portion of the iron to form a protoxide, and thus discharge the red colour.

So also, in regard to the cracks and fissures, on each side of which there is a ribbon of a white or greenish-white colour,—may the peroxide originally, in that part of the stone, not have combined with the carbonic acid of the air and water, permeating these cracks, and produced similar effects?

* LORD GREENOCK, to whom, as a member of Council, this Memoir was referred for examination, has, in reference to this point, written on the manuscript the following remarks: "According to Miller, who quotes the opinion of Agassiz, the remains of Holoptychius are characteristic of the *upper* beds of the old red sandstone, the inferior beds being distinguished by different organic fossils, viz., the midstone or cornstone formation, by the Cephalaspis, and the lower by Ptericthys, Coccosteus, Diplopterus, &c., each formation having its distinct group. Therefore, the remains of Holoptychius only having been as yet noticed in Roxburghshire, is a strong confirmation of Mr Milne's views in respect to there being little, if any, difference in age between the two descriptions of sandstone which he has noticed as existing in that county; scales, &c. of Holoptychius being likewise met with in the coal formation." G.

by which they are characterised in other parts of Scotland. 1°. In Fife and Morayshire the formation is described as consisting of yellow, grey, and dark-red beds, the first of these being the highest, and the last the lowest in the series. It has been seen that these varieties, and in the same order, characterize also the old red sandstone formation of Roxburghshire. 2°. In Fife this formation is overlaid by the coal-measures, just as in Roxburghshire. 3°. The remains of the *Holoptychius*, and of a smaller fish much akin to it, which characterize the old red sandstones in the North of Scotland, have been found among the red rocks of Roxburghshire, in several localities. It is also not a little remarkable, that these fossils should be found throughout Scotland, characterizing only the red and yellow beds,* but not the intermediate grey beds.

(3.) It has been assumed in the remarks above made, that the strata of the coal-measures in this district have been derived from the finer debris and sediment afforded by the greywacke and felspathic rocks. It is well known that these ancient rocks contain, in general, all the elements which are necessary to form beds of sandstone, shale, limestone, and magnesia.† The porphyritic rocks of the Cheviot do in many places contain lime (at least they effervesce with acids‡), as also great abundance of silica and alumina. At the same time it is difficult to perceive how these different substances, brought simultaneously, and forming a common sediment, could have been deposited in separate beds of pretty uniform thickness, and of great extent. It seems more natural to suppose, that all the particles of these different substances, mechanically suspended, were deposited promiscuously in one common mass; and that some movement of the particles afterwards took place, probably according to their respective chemical affinities. Some of the elements which now occur in the strata, of course, were held by the water in solution, as, for instance, carbonate of lime and carbonate of magnesia; and it is not difficult to conceive how these may have been precipitated according to circumstances.

Thus, the extensive beds of chert limestone, the thin beds of magnesian limestone, and the nodules and veins of gypsum or sulphate of lime, which occur (as was shewn in the first part of this Memoir) only near great sheets of porphyritic trap, probably owe their origin to the great and long-continued heat in those places where they occur. It is well known to geologists, that the frequency with which gypsum and magnesian limestone or dolomite are associated, has long been matter of speculation,—a circumstance which, as Sir HENRY DE LA BECHE observes,

* For an account of the Fife fossils, see the Rev. Mr ANDERSON's Memoir, published in the Highland Society's Transactions; and of the Moray beds, see Sketches by PATRICK DUFF, Esq.

† DE LA BECHE, Manual, p. 450.

‡ The porphyry at Plewlands effervesces very briskly, and must contain a large quantity of lime.

"has not been satisfactorily accounted for."* In the work now quoted from, Sir HENRY refers† to the opinion of VON BUCH, that the dolomite of the Alps and some other places is an altered rock, and has been acted on by the augite porphyries, which contain magnesia, and from which, therefore, the magnesia may have been derived. If the magnesia has been derived only from the porphyry, it is not easy to understand the transmission of it to beds at a distance from the porphyry. It seems to me more natural to suppose, that the water diffused through the sedimentary deposits held magnesia, as well as other substances, in solution; and that, by an excess or long continuance of heat, a precipitate was caused, which would be diffused through the beds, and act as a cement to the particles of silex and alumina, and other substances which had been mechanically deposited. The same remark I would apply to the beds of chert at Hadden, Bedrule, and other places, containing nodules of chalcedony and of lime, which can scarcely be doubted to have been chemical precipitates. In like manner, the nodules of red and the veins of white gypsum which occur in the marl strata, may be easily supposed to have been thus formed.

The abundance of lime, in one form or other, existing in the sedimentary rocks of the district, is very extraordinary; and appears to be due to some other cause, than the mere wearing down of the Cheviot porphyries. For though, as already mentioned, these porphyries occasionally effervesce with acids, the quantity of lime thus indicated, bears no proportion to the quantity existing in the old red sandstone and carboniferous formations. There are few places, where the sandstones of both sets of rocks do not effervesce. The red sandstone at Lochton, about two miles east of Kelso, yielded on analysis 25 per cent. of lime.‡ The well-water at Eccles Manse, in Berwickshire, shewed, out of 100 parts, 57.75 of sulphate of lime, and 29.75 of common salt. It seems to me, therefore, that the waters in which these sedimentary rocks were deposited, and which continued to saturate the sediment out of which they were formed, must have contained in chemical solution a large proportion of lime.

Allusion was made in the first part of this Memoir to the existence of "*yokes*" or large concretions, not merely in the igneous but also in the sedimentary rocks, and especially the sandstones. The formation of these harder portions seems due to chemical action of some sort, excited probably by heat.

I am disposed to ascribe to the same cause the formation, at least in many instances, of beds of homogeneous matter. It is difficult to imagine how strata, which sometimes extend uninterruptedly over large tracts of country, and possessing a remarkable uniformity of thickness, could have been formed by the mere

* Manual, p. 478. † Ib. p. 475.

‡ Analysis by Dr THOMSON, given in Loudon's Mag. of Nat. History.

mechanical deposition of different kinds of sediment. I cannot help thinking that substances of the same nature, and having chemical affinities, have afterwards arranged themselves into beds. I have, however, no evidence to adduce in support of this view, and I offer it as little better than a conjecture to account for a problem in geology, which, it appears to me, has not yet been solved. But it has always appeared to me that geologists have taken too little into account the important effects which might result from chemical action continuing for a long period to modify the arrangement and character of the sedimentary strata.

4. After the deposition of the red sandstone and carboniferous rocks, another outburst of igneous rocks took place,—of all kinds. The amygdaloid and breccia of the Eildon Hills, of the Minto Hills, of Bedrule, of Ancrum Crag, and Wooden Burn, then flowed up, as well as those great coulées of porphyry already referred to.

Many, indeed most, of these *newer* volcanic rocks are in contact with, or in the immediate neighbourhood of, igneous rocks of a much *older* date. But this is just what might have been expected, as those parts of the earth's crust, once burst through, would continue to be weak points, and afford less resistance than others to the expulsion of volcanic matter.

Besides those eruptions of trap, which now form hills and coulées, there belong to the epoch now referred to, the greenstone and basaltic dykes. It is a curious circumstance that these dykes all run very nearly parallel to one another, viz. about west-north-west by compass; and that this also is the direction of all the principal dykes in Northumberland and Durham. Further, it is deserving of remark, that the Hawick dyke, which I have traced for above 26 miles continuously, and at its south end crosses the Cheviot range of hills, appears to coincide with one or other of the basaltic dykes running into the sea on the Northumberland coast. Mr Wood,* in his account of the rocks on the shore between Berwick and Newcastle, speaks of a basaltic dyke near Howick running N. 58° W.,† and which, whilst it agrees in direction with the Hawick dyke, seems to cut through the Cheviots not far from the place where the Hawick dyke runs. If the conclusion to which these circumstances point, be verified by a farther examination of the course of this dyke, it will then be found to stretch in one unbroken line for at least 50 miles, and without at either end shewing any signs of cessation.‡

* Transactions of Newcastle Natural History Society, vol. i. p. 308.

† Mr WOOD's statement is N. 83° W., which, it is presumed, are *true* bearings.

‡ Mr ADAM MATHESON, millwright, Jedburgh, already referred to for his geological zeal, has lately afforded additional proof of this, by actually attempting to trace the dyke from the Scottish Border through Northumberland to the sea. Having intimated to me his intention of setting out on this voyage of discovery, and asked me for instructions, I sent him out a map, compass, and other necessary implements. He writes me, that he hired a horse at Jedburgh, and set out from Hindhope along the line which, at that

These dykes appear to indicate the production, by some cause or other, of very extensive cracks in the earth's crust, which were afterwards filled up by igneous matter injected from below. But for such cracks, the whin dykes described in the former part of this Memoir would not have existed.

I am aware that on this point there may be difference of opinion; as in geological treatises these dykes are generally explained on the supposition that the igneous matter, by being forced up, produced the crack. But in opposition to this view, I submit,—(1.) That if the igneous matter was forced up through the sedimentary stata, where there was no previous rupture, the edges of these strata would, in almost all cases, have been turned upwards on each side of a dyke. Now, this is not the case in any of the dykes of Roxburghshire; and though I have seen elsewhere, cases where the strata were inclined upwards to the dyke, and also where they have dipped down towards it, these rare cases can be explained by subsequent vertical movements of the strata on one side or other of the dyke. (2.) I have to observe, that if igneous matter was erupted through strata, where there was no previous fissure, it would all flow out at the place where it first got vent, and would never form a narrow dyke only 20 feet wide (which is the average width of the Hawick basaltic dyke), and intersecting the country in a line very nearly straight.

On these two grounds, it seems to me perfectly clear, that at or shortly before the eruption of the greenstones and basalts, some great convulsions took place, by which the earth's crust was rent and ruptured, just as it had been at a former period, and that through these rents portions of the earth's molten nucleus were again ejected.

If the theory of Elie de Beaumont, before referred to, be well founded, that the elevation of mountain chains, and the protrusion of the ancient trap rocks, is caused by changes in the shape or volume of the earth's nucleus,—then the same theory seems sufficient to explain the production of rents, and the injection of these rents with igneous matter at a later epoch. Assuming that these last convulsions may be accounted for by changes in the earth's nucleus, it is deserving of remark, that these changes were not precisely similar to those which had previously occurred; and, moreover, that the substance of the nucleus itself must have undergone some alteration. At least, the matter erupted from it is extremely different,—there having been felspathic rocks in the *first* instance, green-

place the dyke appeared to run in. He has returned the map to me, having marked on it the places where he recognised the dyke. From his account, it appears to run by Clennel, Borrowden, Whittle, Dibden, Framlington, and Acklington. This last point is about seven or eight miles from the sea,—and beyond it Matheson did not proceed in his search. Though the dyke is reported by him to present a very variable direction and width—its average direction and width seem to agree with its character in these respects in Roxburghshire. The dyke at Howick, mentioned by Mr Wood, cannot therefore be the Hawick dyke, though it runs parallel with it, and about twelve miles to the north.

stones and basalts in the *second*. Moreover, whilst, during the former period of eruption, the lines of fracture were invariably east and west, on this last occasion they were west-north-west and east-south-east. Nor is this last direction merely that of the trap dykes; for it has been seen that there is one set of fissures or joints in the rocks themselves, and which pervade all the strata, from the oldest to the newest, running in much the same direction. If any explanation of these combined and complex phenomena is to be sought, on the hypothesis before referred to, we may suppose that the shape of the earth's nucleus changed, so as to become, immediately below this part of its surface, more convex than before, and to form a sort of ridge running in a west-north-west direction. The pressure of this bone of the nucleus on the outer skin, would have a tendency to produce fractures or cracks immediately above it, and in lines parallel with itself; whilst through these cracks molten matter would gush out, and form both dykes and coulées. At the same time, so much heat would be communicated to the whole of the rocks, both stratified and unstratified, composing the earth's crust, that chemical affinities would be called into action, the matter of these rocks would begin to re-arrange itself, and thus multitudes of minor cracks in these rocks would be produced, which would approximate to a west-north-west direction,—that being, in the circumstances above described, the line of greatest weakness.

5. The next important epoch, in the history of those convulsions to which this district, in common with the rest of the island, was subject, is connected with the formation or deposition of the clay, gravels, sands, and boulders which cover the rocks. To this class of phenomena much interest attaches, not merely from the general and abstract difficulty of explaining them, but also from the attempt which has recently been made, supported by great zeal and talent, to account for them all by glacial action.

Now, I freely admit that the problem is exceedingly complex, and that, therefore, every attempt to solve it should receive due consideration. Nor do I pretend to say, that any explanation of all the phenomena presents itself, which is quite satisfactory even to my own mind. But, whatever the true theory may be, of one thing I am satisfied, that glaciers could not have transported the boulders, or produced the remarkable accumulations of gravel and sand which occur in this part of Scotland.

(1.) What cause can be suggested for the transportation of the numerous boulders strewed over Roxburghshire, and especially the blocks of granite which occur in Liddesdale?

In the previous part of this Memoir, when noticing the situation of the boulders, to whatever species of rock they belong, I shewed that the parent rocks were, in all cases, to the westward of the boulders.

In some of these cases, the situation and relative levels of the parent rocks and the boulders are such, that there is, on that account, no impossibility in sup-

posing transport by a glacier. But in other cases, and these the most frequent, such a hypothesis is altogether inadmissible.

Take, for instance, the case of the granite boulders of Liddesdale, which are found strewed all over the country, between the Carter on the east and Canonby on the west.

These blocks, it is certain, have, by some means or other, been brought from the westward. The only places, in this part of the island, where granite, either red or grey, is known to exist, are in Cumberland, Kirkcudbrightshire, and Galloway. The nearest of these places is Criffel; and it certainly appears to me, on comparing the granite boulders of Liddesdale with the granite of Criffel, that they are identical.

If this be the case, there is an end of the Glacial Theory, as affording either a probable or possible explanation of the phenomenon. For, in the first place, who ever heard of a glacier 40 miles long,—that being the distance of Criffel from the upper part of Liddesdale? Moreover this glacier, in order to have transported Criffel granite to the hills round Castleton, and near the Carter, must have moved inconsistently with the natural levels and drainage of the country, these being from Criffel, generally speaking, towards the south and south-west, and not towards the east. A glacier which transported granite blocks from Criffel to the hills of Liddesdale, besides having been 40 miles long, must have crossed the valleys of the Nith, Annan, Esk, and Tarras rivers, as well as the high ridges separating them;—it must have done so, without having any lateral barriers to retain and guide it;—and, lastly, it must have moved *up* the valley of the Liddel for at least 15 miles of its course.

Discarding, then, the glacial theory as quite insufficient to account for the transportation of the granite boulders of Liddesdale,—and of several other of the cases noticed in the first part of this Memoir, are there any other means of transportation which can plausibly be assigned?

Before offering any suggestions on this point, I beg here to allude for a moment to another general feature of the district, as tending to throw some light on the question,—a feature well known to prevail in many other parts of the island. I allude to the fact, that almost all the hills present precipices of bare rock towards the west, and tails of gravel on the east;—a phenomenon, as already mentioned, first prominently noticed by the late Sir James Hall. This, by the way, is one of those things which the glacial theory not only fails to explain, but which is entirely at variance with it; for if it is alleged that the hills were bared by glaciers, the precipitous sides should always be towards the highest part of the country from which the glacier descended; so that, in Liddesdale, they ought to be towards the east, instead of being, as they all are, towards the west.

The phenomena now adverted to shew, I think, pretty clearly, that, at a comparatively recent period in the history of the earth, there was some vast rush

of waters from the westward, which bared most of the hills on that side, leaving or depositing on their opposite or lee sides, vast accumulations of sand and gravel, containing, in many cases, large fragments of rocks.

The question, then, is, could such a rush of waters have transported from Criffel to Liddesdale the rounded boulders, some of them 3 or 4 tons in weight, which are now to be seen there?

In answering this question, I refer to the Geological Manual* of Sir Henry De La Beche, for the following facts, which are the more valuable, as not adduced by him in support of any theory, but are given merely as illustrations of the power of water to move heavy bodies. He says, that at "Plymouth, during the severe gales of November 1824, and also of the commencement of 1829, blocks of limestone and granite, from 2 to 5 tons in weight, were washed about on the Breakwater like pebbles:—about 300 tons, in blocks of these dimensions, being carried a distance of 200 feet, and up the inclined plane of the Breakwater. A block of limestone weighing 7 tons was washed round the western extremity of the Breakwater, and carried 150 feet. Two or three blocks of this size were washed about." I may be permitted to add, that, having visited Plymouth a few months ago, I was shewn by Mr Stewart, who has charge of the Breakwater, several blocks, from 7 to 10 tons in weight, which, in the storm of January last, had been moved from 15 to 20 yards.

These effects of aqueous action become less surprising, when it is considered that a granite block of 5 tons weight in air, weighs only about 3 tons in salt water; and, moreover, that the power of a current to move a solid body increases as the square of its velocity. So that, as Mr Hopkins has observed in a paper recently read by him in the Geological Society, "if a current of ten miles an hour would move a block of 5 tons, a current of twenty miles an hour might, under similar circumstances, move one of 320 tons." Mr Hopkins, in the paper just alluded to, has given an account of the boulders in Yorkshire, which have been transported from Cumberland, and has adverted to the different theories by which that transport is accounted for. He gives it as his opinion that these boulders, as well as the whole mass of diluvium with which they were associated, were transported whilst the country was under sea; an opinion which I had myself very confidently embraced and advocated, long before I had heard of Mr Hopkins' views. He explains in his paper, not only the efficacy of currents of a given velocity to move and transport blocks, but the mode in which such currents may have been produced; and, finally, he does not "hesitate to affirm the entire adequacy of such a cause to transport all the erratic blocks derived from the Cumbrian mountains, and therefore to conclude, that such has been the agency by which that transport has actually been effected."

* Page 82.

In regard to what might have caused a current of 10 or 12 miles an hour, which seems sufficient, according to Mr Hopkins, and according to the facts also related by De La Beche, for the transportation of boulders, various suggestions have been offered. The most probable cause seems to be, a submarine eruption to the west of Great Britain, of sufficient magnitude to produce a great wave. At the time of the Lisbon earthquake in 1755, the focus of which is supposed to have been about 100 miles to the westward, a wave was produced, which, when it broke on the coast of Portugal, was from 40 to 60 feet high. This wave reached the British Islands in about six hours, having travelled at the enormous rate of about 150 miles an hour; and on entering the different harbours in the south shores of England and Ireland, broke the moorings of almost all the ships at anchor.

It is related by Sir Woodbine Parish, that during the earthquake which destroyed Callao in 1678, the sea, after first retiring, rose with such violence as to carry "three ships, about 60 or 100 tons,"—"*over* the town, which then stood on a hill."

"In 1746 Callao was again destroyed by an earthquake-wave, vast heaps of sand and gravel occupying its position. All the ships in the harbour, except four, foundered. One of these, a man-of-war, was found in the low ground of the Upper Chicara, opposite to the place where she rode at anchor, and near her the St Antonio. Another of these vessels rested on the spot where before stood the Hospital of St John, and the ship Succour was thrown up towards the mountains."*

These accounts of the enormous size of the waves, which are formed during many of the South American earthquakes, are fully corroborated by Mr Caldcleugh's account of the earthquake, by which Talcahuano was destroyed in 1835. There were several waves which then rolled in upon the land, apparently exceeding 40 feet in height.

With reference to these cases, however, it deserves to be remarked, that they prove only the power of moving water to *move* blocks,—not to *transport* them, at least for any considerable distance. The wave of translation referred to by Mr Hopkins, and as described in Mr Scott Russell's valuable papers on waves, produces only a momentary current at the place affected by the wave. There is a forward propulsion of the aqueous particles only for a short distance, corresponding with the width of the wave. The wave travels on, leaving behind it, the particles so moved. In like manner, a body immersed in the water, even of only the same specific gravity with it, would be pushed forward but a short distance, and left there. Of course, therefore, a block of granite, though it might readily be moved, would not be transported more than a few feet by any single wave, however great in size or rapid in motion.

* Abstract of Geological Society's Proceedings for December 1835.

But whilst I think it fair to notice these circumstances, as creating in my mind some difficulty in understanding how an earthquake-wave can transport boulders, I admit that the opinion of such mathematicians as Mr Hopkins and Mr Scott Russell, who concur in ascribing a transporting power to such a wave, is deserving of all reliance. The views of the former I have already stated; and to shew the views of the latter gentleman, I may be allowed to quote the following passages from a correspondence I have had with him on the subject. "The valuable application," says Mr Russell, "which Mr Hopkins has made of our knowledge of the laws of the great wave of translation, is in perfect accordance with all the phenomena I have examined in my observations on this class of waves. In the first place, Mr Hopkins' mode of genesis of the wave, is identical with a method of genesis which I have adopted in experiment, viz., the upheaval of a considerable surface on the bottom of the channel. Suppose a depth of ocean of 400 feet: then, according to my experiments, the velocity of transmission of the wave would be 77 miles an hour. But if the wave were of a height of 50 feet above this level, the velocity of the wave would be increased to 84 miles an hour. The velocity of translation of such a wave could attain a maximum of 27 miles an hour. This represents the current of the particles of water tending to move an obstacle at the moment when the highest part of the wave is passing." "It is perhaps important to observe, that the *transporting* power of such a wave will be greatly facilitated by encountering a gradual shallowing and contracting of the firth or channel into which it enters, as in the case (viz. the granite boulders of Liddesdale) which you have applied this force to explain.

"There is an additional view of this subject I may suggest to you, viz., that on a hard or rocky surface, the chances are much in favour of the *large* block. The tendency to crush an opposing obstacle, increases with the weight, or is as 2^6; this being of a given size, both for the little and the large boulder. Further, there is a gain of moving force, which is as the distance from the centre of gravity of the block from the obstacle. This again gives us an increased chance in favour of the large block as 2 : 1. Hence, on hard ground, the chances of motion, and of continuing in motion, are greatly in favour of the large block."

On writing Mr Scott Russell, to suggest whether his wave of translation, though it should move the block, would not then pass and leave it behind, I received an answer from which I quote the following passage:—"I see some difficulty in getting the large boulder into motion, but little in keeping it going, after it has set out. If you start it by a gentle slope, then the water would have nothing to do but give it way. A hard and tolerably even bottom, like a level stratified rock, would greatly facilitate the locomotion; and as to its being bedded in mud or earth, why, the rushing of the waters past the boulder would soon clear that away. Besides, the presence of a column of water, or a wave 20 feet high, would be more than equal to the whole weight of such a boulder as you describe;

so that the height of the impinging column would certainly be ample for its propulsion. Reckoning your granite boulder at say 5 feet diameter, a column of water 15 feet high would give a dynamical action on it, greater than its entire weight in air, and of course equal to its propulsion on any surface,—2.5 is, I think, about its specific gravity."

Mr Scott Russell thus distinctly concurs with Mr Hopkins as to the transporting power of a wave produced in the way above supposed; and if these opinions are well founded, then there seems no difficulty in explaining how the granite boulders of Liddesdale were brought from Criffel.

But, in addition to *waves* in the ocean, produced by submarine earthquakes, there must have been *currents*, which, if of sufficient rapidity, must, as it appears to me, have been still more effectual in accomplishing the results in question. That currents, and of great force and extent, existed in the ocean which, at the epoch of these boulders, covered the district, is proved by the bared western faces of the hills, and the residuum of diluvial debris on their east sides; and it will by-and-by be shewn, that these debris could have been spread over the entire country, by no other cause than currents of water. The origin of these currents —all from the westward—it is certainly difficult to account for; but the fact of their existence seems indubitable, and also of their having had such a force and velocity, as is quite sufficient to have transported boulders.

(2.) The next phenomena to be noticed are the accumulations of clay, gravel, and sand.

In the former part of this paper, I mentioned that the only place where I had observed the well-known boulder clay, which is so largely developed in Mid-Lothian, is in the neighbourhood of Kelso. The deposit there is filled with huge blocks of basalt, greenstone, greywacke, and porphyry rocks, of which none exist in the neighbourhood *in situ*. The blocks, by their rounded forms, indicate very plainly that they have been rolled from a distance; and as no rocks of the same characters exist towards the eastward, there is great probability, if not absolute certainty, that the blocks in question, as well as the clay in which they are imbedded, were, by the force of water, transported from the west. The clay is not stratified, and there are no layers of sand in it. Moreover, the imbedded boulders are not deposited according to size or weight. Judging from these circumstances, I should say that this boulder clay must have been deposited by waters of great power and violence.

Above this boulder clay there is a deposit of gravel and sand, which forms a skin, as it were, over the whole country. It is to be seen distinctly covering the boulder-clay at Hadden. The pebbles do not generally exceed the size of the fist. They are, in every place which is known to me, rounded and apparently water-worn. There are occasionally extensive beds of sand, which alternate with the layers of gravel.

This accumulation of sand and gravel, is in all respects so different from the boulder clay, that it must have been deposited under very different circumstances, (1.) The boulder clay is almost uniformly the lowest deposit. (2.) It is never stratified. (3.) It contains much larger fragments of rock than the superincumbent beds of gravel.

From these data I infer, that the boulders and clay were transported by tumultuous waters, whilst the gravels and sands were deposited in waters comparatively tranquil, though affected by currents.

Now, here the question occurs, Whether the sands and gravel, just alluded to, were deposited during the rush of waters which bared the west faces of the hills? I should be inclined to think, that they had been spread over the country previously to this event; and that the effect of that violent and universal rush of waters, must have been to sweep away a great proportion of these superficial deposits, leaving undisturbed only those on the east side of hills, and, perhaps, adding to the deposits there. In confirmation of this last remark, reference may be made to the enormous accumulation of gravel, on the east sides of the Cheviot hills (near Palinsburn and Wooler), of the Galashiels hills, and of Lamberton hill in Berwickshire.

If, then, previous to the rush of waters from the westward, the country had been overspread with sand and gravel, this would indicate that it must, at all events, down to the date of that occurrence, have been under the waters of a sea, which transported sand and gravel from great distances. In no other way is it possible to account for the extensive beds of sand and gravel, often stratified, which occur in many parts of Roxburghshire, and particularly in Liddesdale, in situations far above and beyond the reach of rivers. In some places (as at Maxton and Plewlands), pebbles of gneiss have been found in cutting drains, which must have been brought at least 80 or 100 miles from the west or north-west. We have seen, that, when the red sandstone rocks were being deposited, the waters of an extensive ocean prevailed to a height of at least 1100 or 1200 feet above its present level. The existence of gravel-beds as well as boulders, at much about the same height, indicates, that, down to a very recent geological period, there had been no change in the relative levels of sea and land. *When* the sea did retire to its present level, is quite a separate question; the solution of which in no way affects the soundness of the views above suggested.

It may, however, be asked, How it is possible, consistently with these views, to explain the origin of the remarkable knolls and ridges described in the first part of this Memoir, and for the production of which it has been thought necessary to invoke the aid of glaciers? It may be conceived how the waters of an ocean charged with sandy sediments, and rolling along gravel, should form beds more or less horizontal. But how can they form narrow ridges of sand and gravel,

from 40 to 50 feet high, and running for half a mile in nearly a straight line (as is the case in Liddesdale), or for nearly two miles in a curved line, as in the case of Dogden Moss in Berwickshire ? *

It is with the view of obviating these apparent objections, that glaciers have been suggested; as the agent which has produced the remarkable knolls and ridges just referred to,—and which is said to be in Switzerland at this moment giving rise to phenomena exactly similar.

But here, as it humbly appears to me, lies the fallacy of the explanation. I do not believe that the accumulations of debris formed by glaciers, are exactly similar to the knolls and ridges in this country above referred to. In outward form they may be similar. Farther, they may often be in situations, precisely analogous to those occupied by the moraines of Switzerland; though, assuredly, they are also as often in situations, where no moraine, terminal, medial, or lateral, is ever seen in Switzerland. But, in the internal structure of these gravel heaps, and in the nature of the materials composing them, there appears to be a total and entire dissimilarity.

All the knolls and gravelly ridges which I have seen in the border counties, contain stratified beds of sand and fine gravel, which seem to me unequivocally to demonstrate, that they were deposited by water; and by water which, judging from the form and nature of the pebbles, must have rolled them from a great distance. Now, it would appear, that moraines have a totally different structure. There are in them no stratified beds of sand or gravel; and the fragments are generally angular. Thus, AGASSIZ states (I quote from a very good abstract of his views lately published by Mr MACLAREN, a Fellow of this Society), that "The materials of moraines *are not stratified*, but huddled together in confusion. The fragments are generally *somewhat* rounded by mutual attrition; but some are *angular*. They may be distinguished from the banks of gravel formed at the margin of lakes, by their internal structure."† To the same effect, Mr CHARPENTIER, in an article published in the last number of Professor JAMESON'S Journal, says, "The sedimentary deposits, *whether stratified deposits of pebbles, sand, or clay*, are, in my opinion, not the *erratic* formation (formed by glaciers), but diluvium, that is to say, a sediment whose materials have been conveyed and deposited by *water*." On the same page, he adds, that "erratic deposits can always be distinguished from the diluvium, by the frequency of well preserved *angular* debris."

If, then, according to the admission of the two greatest advocates of this Theory, the accumulations of gravel formed by glaciers are characterized by frequency of "angular debris;" and if, as they also admit, stratified beds of

* For an account and surface-plan of Dogden Moss Kaims, see Paper by me, published in the Transactions of the Highland Society for 1836.

† Glacial Theory, by Mr MACLAREN, p. 14.

pebbles and sand could only have been deposited by water, then it cannot be doubted by any one, who looks at the internal structure of the ridges of Liddesdale and of Kelso in Roxburghshire, or of Dogden Moss, and of Dunse, in Berwickshire, that they must have been formed by aqueous and not glacial action.

These kaims, I may here observe, are not of rare occurrence, either in this country or abroad, though, considering their very singular appearance, it is remarkable how little the origin of them has, till lately, been speculated on. I have mentioned two remarkable examples in Berwickshire, in addition to those in Roxburgh; and, probably, many persons here may have seen the one at Campend,* about two miles north of Dalkeith. There is another to the south-west of Arniston, near the Moorfoot Hills. In the State of Maine, in North America, there is a ridge provincially termed *Horseback*, which Dr JACKSON, the State geologist, says "consists of sand and gravel, built up exactly like the embankments for railroads, the slope on either side being almost 30°, while it rises above the surrounding lowlands to the height of 30 feet, its top being perfectly level, and wide enough for two carriages to pass abreast." In the same district, there is another horseback described as running for no less than six miles, and elevated about 15 feet above the swamps on each side. The horsebacks of New Limerick and Houlton, in the United States, are much more elevated, some of them being (as is said)† 90 feet above the adjoining places.

In Scania (a province of Sweden), a number of similar ridges prevail through the country, a description of which is given in Mr LYELL's Bakerian Lecture on the rise of land in Sweden.‡ He says that, near Stockholm, "remarkable ridges of sand and gravel are seen, called in Sweden Sand-oasar. These oasars are immense banks of sand, from fifty to several hundred yards broad, and from fifty to more than one hundred feet in height, which may often be traced in unbroken lines for a great many leagues through the country, but are breached occasionally by narrow transverse valleys. They usually run in a direction from north to south; generally terminate on both sides in a steep slope, and are sometimes so narrow at the top, as to leave little more than room for a road. As they afford excellent materials for road-making, a great many of the highways in Sweden are carried either along the summit or base of these ridges, so that the traveller has many opportunities of observing their form and structure. In places where they are composed of large rounded boulders, of about the size of a man's head, no stratification is observable; but where, as is more usual, they consist of gravel and fine sand, *they are invariably stratified*, in the same manner as sand and gravel in the beds of rivers. I shall offer, in another place, some speculations

* This word is probably a corruption for Kaim-end,—as Kaim is the term by which these elongated ridges are universally designated in the south of Scotland. The ridge here referred to has been, in several parts of Mr LAING's farm, opened, both for gravel and for sand. Its length is about half a mile.

† HITCHCOCK on Deluges, Part II., p. 103.

‡ London Philosophical Transactions for 1835.

on the probable origin of these ridges; and I have merely alluded to them now, in order to explain the position of some *fossil shells* which I am about to describe."

"They (the shells) occur at Solna, about a mile to the north-west of the city, at the foot of one of the great ridges of sand and gravel before mentioned; a ridge which, passing southward, traverses the city of Stockholm, and is said to have afforded fossil shells in the large pits at the Skantstull, in the southern suburbs. These pits lie between the church of Solna and the public cemetery of Stockholm. Both in the pits and in the adjoining ridge, the gravel and sand is stratified, and in general no organic remains can be discovered in them; but in the pits, a little below the level of the road, there are some layers of loam mixed with vegetable matter, where shells occur in abundance."

Since, then, sea-shells occur, if not in the heart of these ridges, at all events at the base of them, and in gravel manifestly of contemporaneous origin, it is impossible to doubt that, at least in Sweden, these gravel and sand banks have been formed at the bottom of the sea; and, accordingly, Mr LYELL had, in the year 1834, no doubt that these *oasars* were of marine production.

Such being the character of these gravelly ridges, it seems not a little bold and inconsistent in CHARPENTIER to lay down the proposition (I quote his words), that "*Oasars are Moraines*, some having been formed by the oscillations to which the great glacier was subjected during its retreat, others by the ice which remained on elevated mountains and table-land, long after the low regions had been freed from it."*

On the contrary, it humbly appears to me, that these oasars of Scania, like the horsebacks of America, and the kaims of Scotland, composed as they all are chiefly of rounded pebbles and beds of sand, must have been formed by water, and cannot have been detritus, either transported on the surface of, or pushed forwards by, glaciers.

Mr POGGENDORFF, in an account of these oasars recently published in his Annalen, mentions a circumstance which can leave no doubt as to their origin. He says that they "always exhibit at their northern extremity, and only there, a fixed standing rock; a phenomenon which, on the assumption of a violent flood from the north, has led to the conclusion, that it was these very rocks which, by affording shelter from the flood, gave rise to the accumulation of the narrow and far-extending alluvial hills."†

Perhaps I may be allowed to mention, that, when I examined several of the kaims in Roxburghshire and Berwickshire, it was in company with a valued friend of AGASSIZ, and an able supporter of his Glacial Theory. Whilst he pronounced at least two of these gravel ridges, viz. that in Liddesdale, and the

* JAMESON'S Edinburgh Philosophical Journal for October 1843, p. 73.

† Ibid. vol. xxiii. p. 72.

other at Dogden Moss, to be, the one a lateral and the other a terminal moraine, he admitted the force of the objections to that theory, founded on the rounded form of the pebbles, and the existence of sand in the heart of the ridges; but he contended, that, in these respects, the moraines in question had been altered by the action of water, probably derived from the melting of the glaciers themselves; and he instanced the formation of small ponds in the glacial valleys of Switzerland, along the sides of moraines, in which ponds, layers of sand, and even of small pebbles, are frequently found. But admitting that there may be beds of sand and fine gravel occasionally formed along the sides of moraines, would this explain the existence of such stratified beds in the moraines themselves?

Another objection to this theory, and which seems to me equally decisive with the existence of stratified beds in them, is suggested by the character of the rock composing a large portion of the gravel, in these beds.

In the Liddesdale moraine, many of the pebbles (to the extent of fully 8 per cent.) are granite. If, then, the pebbles have been brought by a glacier descending the valley of the Liddell,—and this is the only valley by which a glacier could have flowed past or reached the alleged moraine of Liddell bank,—there should be granite rocks in the higher parts of that valley. But, as already mentioned, there is not a trace of granite in those parts,—the nearest place where granite exists, being about 25 miles to the west, from which, on account of the levels and character of the country otherwise, no glacier could have reached.

We must look, then, for some other cause or causes than glaciers, for the transportation of these granite pebbles, and the formation of the knolls and ridges containing them; and I proceed now to offer the views which have occurred to me, as to these probable causes.

(1.) One cause, by which I believe a very large class of diluvial phenomena may be explained, is aqueous action on pre-existing beds of sand or gravel.

Assuming the country to have had spread over it, at least in many places, a thick covering of sand and gravel, and which must, as already shewn, have been deposited in a sea standing at a level 1100 or 1200 feet higher than at present, what was the effect of the rising of this land to such a height, by which all these sands and gravels became exposed to atmospheric influences?

In the *first* place, the emergence of the land, unless it was gradual, must itself have caused the beds of sand and gravel to be cut up by the force of the retiring waters, in a very remarkable manner. Their action would vary in force and direction according to the nature of the materials and the pre-existing levels. Wherever there was a depression of surface, there the waters would act most powerfully, and thus form deep cuts or gutters in some places, and high ridges in others. Suppose what is now a valley (as that of the Liddell) to have been, when under the sea, filled up with sand and gravel,—then, if the

above views are correct, there would, on its emergence, be deep gutters nearly parallel, cut through the whole of it. In the *central*, because the lower parts, the largest portion would be scooped away, and along a line corresponding with the general slope. At the *sides*,—gutters and ridges would be formed nearly parallel to these sides, if the range of hills composing them did not rise high; but, if otherwise, the ridges and banks between the hollowed channels, would be slightly inclined away from the sides, and converge towards the lower part of the valley. On the complete emergence of the land, the central parts of the valley would, of course, be occupied by a river, which would gradually undermine and carry off what gravel and sand had been left there. The lateral ridges and mounds at a distance from the river, would continue undisturbed except by the minor influence of rain and rivulets.

Suppose now the case of *two* valleys with a ridge between them, both nearly filled up as before with sand and gravel. On the waters rushing off by a rapid rising of the land, deep cuts, and high banks between the cuts, would be formed, as above explained,—the middle of the valleys being the places where the least quantity of gravel would be left, and the sides being the places where the gravel banks would be most undisturbed. Then at the end of the ridge dividing the two valleys, the sand and gravel would be little affected on the *lee*-side of the hill, so that, in such a situation, it is easy to conceive how a ridge or bank should be left, having a direction corresponding with the average direction of the two valleys which had guided the rush of water on each side of it.

It appears to me perfectly possible to explain in the way now suggested, many of the banks and ridges of gravel which exist in Roxburghshire, and, in particular, those at Liddell Bank, and near the Elland or Allan Water, described in the first part of this Memoir. The one at the place last mentioned, though now in some places broken down into a series of knolls, has originally been parallel, or nearly so, to the general axis of the Gala valley, and had extended for about three quarters of a mile in length. It is evident that if, when the land emerged from the sea, its surface had anything like the form which it now has, the great rush of waters must have been on the north and south sides of this bank, so that in their progress eastward, the waters must have had comparatively little effect on the gravel accumulated there. Then as to the Liddell Bank ridge of gravel, if the waters rushed off by a sudden rise of the land, which is the case supposed, that ridge being situated at the west end of the hill or range of hills which divides the Merse and Liddell valleys, the detritus there must have been protected by the hill, and thus would form an elongated bank, its upper end inosculating with the east side of the hill.

These effects, however, would follow only on the assumption, that the land rose with sufficient rapidity to produce a rush of waters.

(2.) If, however, this was not the case, then it remains to be seen what would be the influences to which the beds and banks of sand and gravel, forming till now the bottom of the sea, would be exposed.

Here, however, there is a preliminary inquiry necessary. In estimating the effects of the atmosphere and meteoric agents just adverted to, some consideration must first be had of the form and shape of the beds and banks on which they were to operate. Now it may be admitted, that the sand and gravel would in general be spread pretty uniformly over the bottom, though, of course, where submarine hollows or valleys existed, the greatest quantity would be deposited there. But whilst this would in general be the case, it is well known that the bottom of the sea, especially where currents prevail, presents in many places narrow banks with steep sides, and which, according to the course of the currents, are either in straight or in curved lines.

It is very well known, that all around Great Britain, and particularly along its southern and eastern shores, banks of sand and of gravel (or shingle, as it is sometimes called) are formed by submarine currents. Sir HENRY DE LA BECHE, in his Manual, describes two of these off the coast of Devonshire;—and any one who reads his description of them, cannot fail to be struck with the strong resemblance which they bear, in form, size, and materials, to many of the banks of sand and gravel now existing on the surface of the land. "The sea," to use this author's words, "separates the Chesil Bank from the land for about half its length, so that, *for about eight miles*, it forms a shingle *ridge in the sea*. The effects of the waves, however, on either side, are very unequal: on the western side, the propelling and piling influence is very considerable; while on the eastern, or that part between the banks and the mainland, it is of trifling importance." Unfortunately, neither the height of this ridge or bank above the bottom of the sea, nor the slope of its sides, is given. But if the woodcut in illustration of the description be correct, the sectional dimensions and shape accord completely with those of the Roxburghshire and Berwickshire kaims.

The other case described by the same author, is known as the Slapton Sands. Sir HENRY describes these as composing, "at the bottom of Start Bay, and for the distance *of about five or six miles*, a considerable bank, *principally composed of small quartz pebbles*." "This bank," the author adds, "protects and blocks up the mouths of five valleys;" so that we have here what AGASSIZ would describe as a terminal moraine, extending across the vomitaries of five glacial valleys. Sir HENRY mentions, that, in November 1824, a breach was made by the sea, through this protective barrier, and that it "continued open for nearly a year, becoming gradually smaller. The complete restoration of the sands," he adds, "was hastened by throwing a few bags filled with shingles into the gap, upon which two or three gales soon piled up a heavy beach." The upper portion of this bank is described as being in some places above the level of the sea; for Sir HENRY observes, that "the old bank (that is, I suppose, before the breach was made in

it,) must have remained undisturbed for a long period; for vegetation had become active on it, as we see by those portions which remain uninjured, where turf and even furze bushes have established themselves upon the shingles." The description of this bank, as in the former case, omits the height and slope of its sides; but a woodcut is given, which shews these to have been considerable.

On the east coast of England, and particularly off Essex, there are great numbers of narrow banks, composed chiefly of sand, both straight and curved. The most remarkable of them are laid down on the ordinary sailing charts.

There is the Gunfleet, about three miles from land, about fifteen miles long, a quarter of a mile wide, and dry at low water. Its sides are steep, and close to them, the depth of water at low tide varies from four to seven fathoms. This bank is situated between the estuaries of the rivers Crouch and Black Water.

A still more remarkable sand-bank for length and narrow width, lies farther to the seaward than the Gunfleet. It is situated between the estuaries of the Thames and the Medway. Its upper part is called, on the charts, "Oaze Edge;" its middle part "Knock John;" and its lower part "The Sunk." It is altogether about thirty miles long. The greater part is dry at low tide, whilst on each side there is from four to eight fathoms, and which rapidly deepens to ten and twelve fathoms.

Parallel with this long sandy ridge, there are twenty or thirty smaller ones, all laid down on the charts.

These are examples of straight ridges of sand. The following is an instance of one which is curved. It is situated off Reculver, in the Isle of Thanet, and is known by the name of the Horse. It would form a complete ellipse, but for a break in one small portion of it. The longer diameter of the enclosed basin is about a mile and a quarter in length, and its shorter diameter half a mile. At low tide the bank is dry, and is less than fifty yards in width, and there is from ten to eighteen feet of water close on each side of it. There is also round a great portion of this bank, an outer rampart, of similar shape, at a distance of 200 yards.

These various cases compel every one to assent to the truth of the following general proposition of Sir HENRY DE LA BECHE (with which he concludes his account of shingle-beaches), "That if the present continents or islands were elevated above the present ocean level, shingle-beaches would be found to fringe the land, but not to extend far seaward."

As it thus appears that ridges are formed by submarine currents, composed partly of pebbles and partly of sand, having a considerable height and steepness of sides, and extending for several miles, sometimes in straight and sometimes in curved lines, may not the kaims of Berwickshire and Roxburghshire have been formed in the same manner? That they have been formed by water, in some way or other, is unquestionable. That the waters in which they were deposited, were in the ordinary state of the ocean, and not in a state of debacle, seems

to be probable, from the structure of the shingle ridges described by De La Beche, and from the impossibility that sand should be deposited in waters that are very tumultuous.

Perhaps I may here be permitted to refer, in support of the above views, to the case of a gravel bank or ridge, never hitherto described, which is situated on the coast of Northumberland, about four or five miles south-east of Belford. It is about three miles from the shore, and it runs nearly parallel with the shore for about four miles. It is composed chiefly of large rounded pebbles, of all descriptions of rocks, derived chiefly from the neighbourhood,—though there are some, the origin of which I have not yet traced. Sand in stratified beds also abounds, and in such quantities as to be worked. Its sides are steep, sometimes exceeding 50°; and in several places its top is from 40 to 50 feet above the adjoining grounds. This remarkable ridge is, at its base, about 120 feet above the level of the sea, and affords, in my opinion, one out of many proofs, that this part of our island has, at a very recent geological period, risen out of the sea. It appears to me, in short, to be one of those shingle banks, described by De La Beche as having been formed by submarine currents, and with which he says this island would be found to be fringed, were it elevated above the ocean level. In confirmation of this opinion, I may add, that, at the north end of this ridge, viz. at Waren Mill, there is a hill of greenstone, from which have apparently been derived many of the rounded blocks and pebbles occurring in the ridge, and which are most numerous towards the north end of it. It seems probable, that the greenstone hill in question has been the means of forming an eddy on the south side of it, in consequence of which a tail of gravel and sand was there deposited. Accordingly, there is no appearance of either sand or gravel, in any form, on the north side of Waren Hill.

I may only farther observe, in regard to this Belford gravel ridge, that it is utterly impossible to account for its formation by glaciers, as there is no great valley, not even a mountain, or any considerable hill, within 20 miles of it; and, moreover, it is situated at a level far below that to which any glaciers are pretended ever to have reached, in this country.

I am not aware that the above view of the matter which I have ventured to suggest, is backed by the opinion now entertained by any of our great geological authorities, and therefore I offer it with distrust. I am glad, however, to find, that this view was at one time, though it may not be now, entertained by Mr Lyell, at least in regard to the production of the Swedish *oasars*, which I have shewn to be identical in form and structure with the kaims of Scotland. Mr Lyell, since he became a convert to the Glacial Theory, has most probably adopted M. Charpentier's explanation of the formation of these oasars; and, accordingly, he has recently attempted to show, that the mounds and banks of gravel, clay, and sand in Forfarshire, have been produced by glacial action. I may not therefore be able to

found upon Mr Lyell's authority, in support of the opinion which I have just expressed, though advocated by himself at a former period. But it is a circumstance in favour of that opinion, that it was the conclusion to which he came, after examining the kaims of Sweden. The following passage occurs in his Paper before referred to :—" The occurrence of layers of marl containing littoral shells, as above described, in the midst of a stratified ridge of sand and gravel, is opposed to the theory of those geologists, who refer the formation of such ridges to a violent flood or debacle rushing from the north. The perfect preservation of the shells at Upsala, and the repeated succession of thin alternating layers of gravel, sand, and loam, which are seen almost everywhere, imply a gradual, and at times a very tranquil, deposition of transported matter. If I am asked for a more probable hypothesis in the room of that to which I object, I may state that these ridges appear to me to be ancient banks of sand and shingle, which have been thrown down at the bottom of the Gulf of Bothnia, in lines parallel to the ancient coast during the successive rise of the land ; or in other words, during the gradual conversion of part of the gulf into land. I conceive that they may have been formed in those tracts, where a marine current, flowing as now, during the spring when the ice and snow melt, from north to south, came in contact with flooded rivers rushing from the continent, or from the west, charged with gravel, sand, and mud. According to this view, these large Swedish ridges may be compared to smaller banks known to have been formed within the last five or six centuries on the eastern coast of England, at points where a prevailing marine current from the north meets rivers descending from the interior, or from the east."

But whilst I adduce, in support of my view, the opinion of Mr Lyell, at least as entertained in 1834, I know that its soundness must be tried, not by authorities of that kind, but by an accurate survey of facts. I merely found on his opinion, in order to bespeak, on behalf of the foregoing views, an attentive consideration.

(3.) But farther, and independently of the operation of submarine currents in forming elongated banks of sand and gravel, it remains to be considered what effects would be produced on the bottom of the ocean, on becoming exposed to atmospheric and other natural influences.

That the rain itself must act powerfully in washing away and carrying off sand and small gravel, cannot be doubted ; and this agent, trifling as it is, appears to me quite sufficient to have produced the almost innumerable mounds and knolls which, as already remarked, are to be seen near Palinsburn, Gala House, and Lamberton. It is impossible, indeed, to doubt that a thick and extensive bed of sand and shingle would, by this cause, after the lapse of time, be cut up into sections of various dimensions ; and when the channels or gutters thus formed reached any considerable depth, the loose materials would begin to be undermined, and separate mounds and banks would be speedily formed.

It is evident that no limit can be prescribed to the variety of forms which the

banks thus engendered may assume. They may be round, or oval, or elongated, according to circumstances.

But to the influence of rain, must be added that of rivers and rivulets, as capable of producing similar effects and in a more striking degree. And it is a circumstance strongly favouring the supposition of their being capable of producing the effects in question, that rivulets are found flowing along or near the base of many of the elongated gravel ridges, which have been compared to moraines,—as, for instance, at Gala House and Dogden Moss.

On the whole, therefore, it appears to me that it is not necessary to resort to glaciers, in order to account for the transportation of the boulders and gravel which have been strewed over the south of Scotland, or to explain the formation of knolls and elongated banks. It is, according to my humble opinion, quite possible to account for all the phenomena, by assuming that these boulders and gravel were transported by submarine action, and subjected to processes of re-arrangement, by subsequent aqueous action in the way just explained. Water, as the true cause, is suggested by the arrangement and nature of the materials, and is found capable of producing the required effects. Ice, as a cause, is negatived by the arrangement and nature of the materials, and is, moreover, in many situations, utterly inadequate to have produced any effects.

6. I have still to make some reference to the formation of the fairy stones found in Allan or Elland Water near Melrose, and other places.

One of the theories on the subject, and supported by, if not originating with, a Principal of one of our Scotch colleges, distinguished for his philosophical discoveries, is, that these stones are formed by the dropping of water, holding in solution earthy particles which cohere on its evaporation. But (1.) how does this account for the general sphericity of these stones? The process just described would form a columnar stalactite,—it never would form a spheroid. (2.), I have in my possession several specimens of greywacke pebbles studded all round with these stones. By the process above mentioned, one can understand how the dropping of water should produce a deposit on one side, but it leaves unexplained the formation of similar deposits on other sides of the same pebble.

Another theory, advocated by a writer in the Transactions of the Berwickshire Naturalists' Club, is, that these stones are formed into their spheroidal shape by the attrition of the current, and rolling on the rocky channel of the river. But there are many facts which shew the unsoundness of this theory. (1.) When these stones are most perfect, they are not in the stream of the river, but on the side of it, at the foot of the clay bank; and when picked up in the channel, at some distance from the clay bed, their characteristic shape is much obliterated. (2.) If the theory suggested be sound, similar stones should be found, not only in higher parts of the river, but in every other river whatever. Moreover, their like spheroidal form should be acquired, not by one kind of stone only, but various other

kinds. All these conclusions, to which the theory necessarily leads, is utterly inconsistent with observation.

A third theory has been lately propounded by Mons. PARROT, in describing " les pièrres d'Imatra," which, judging from his elaborate account, and still more from the beautiful lithographic figures he has given of them, I think are identical with the stones of both kinds mentioned in the first part of this Memoir, as found in Elland and Kale Waters. This author maintains that " les pièrres d'Imatra sont des mollusques petrifiées, sans coquèllis." He modestly declines, however, " to classify this new family of molluscs," leaving that labour to other zoologists; but he does not hesitate to name it, as one the existence of which can no longer be doubted, and the name he gives to it is *Imatra*, in honour of the place where it was first discovered.

This extraordinary theory is very zealously supported in a Memoir which extends to 130 quarto pages, and is illustrated by no less than sixteen plates, occupied partly with views of the locality, but chiefly with figures of the stones, of which there are nearly 100.* The stones so figured are identical in size, shape, and appearance, with those described in the present Memoir; and the chemical analysis given by Mons. PARROT, appears to be in entire accordance with the composition of the Roxburghshire stones. I have read with attention the arguments which he advances in support of his theory, that they are molluscous animals in a fossil state; but I confess that they have neither convinced nor influenced me. They have left only a feeling of surprise, that so extraordinary an inference should have been adopted on such slender evidence.

In the first part of this Memoir I expressed an opionion, that these fairy stones are concretions of clay produced by the homogeneous attraction of its particles. Though Mons. PARROT notices this hypothesis, and endeavours to combat it, I think his arguments altogether futile, and several of his facts not a little confirmatory of it. From the analysis which he gives of these stones of Imatra, the following are the proportions of the substances composing them :—

Carbonate of lime,	.49
Silica,	.19
Alumina,	.12
Oxide of iron,	.13
Sulphur,	.04
Water,	.01
	.98

Mons. PARROT gives also an analysis of the clay which contains these nodules, and which consists of sand .32, silica .37, alumina .13, and oxide of iron .15, =.97.

* This Memoir was published at St Petersbourg in 1840 by the Imperial Academy of Sciences.

He remarks on the total absence of lime and sulphur from the clay, as proving that the nodules which contain these two substances, could not have been formed in the clay itself; and this is his chief argument for the organic origin of the stones.

But this very diversity of chemical character, appears to me to explain the formation of the concretions. If carbonate of lime and sulphur are capable of exciting chemical attraction or repulsion, it is plain that these substances, originally existing in the clay, might easily produce concretions. Innumerable examples occur of the formation in this way, of nodules composed of iron pyrites, carbonate of lime, and many other substances, the particles of which must have separated from the general mass of matter through which they had been interspersed, and formed bodies variously shaped.*

Whilst in this way it would not be difficult, on ordinary and well-known principles, to account for the formations of these fairy stones in the interior of the clay bed, certain it is that they are also formed when exposed to atmospheric influence. Both at the Fairy Dean, and on the banks of the Tweed near Berwick, I have picked up small portions of the clay about the size and shape of a walnut, hard on the surface, but perfectly soft and plastic in the interior. They were evidently in an incipient state of consolidation and chemical arrangement, probably induced by evaporation and the action of the external air. One of these half-consolidated nodules I took home, and in a couple of days it became as compact, and nearly as well shaped, as any of the rest.

I have stated that these stones, besides being of a spherical form, more or less flattened, generally consist of laminæ, which are the same in character with the laminæ of stratification in the bed of clay. This circumstance affords additional proof that these stones are concretions formed by chemical action in the clay. In this respect they bear a very close resemblance to the calcareous nodules described by De La Beche as existing in the marl beds of Lyme Regis.†

I here conclude my Memoir on the Geology of Roxburghshire. Whatever may be thought of the description which I have given of its different formations, or of the views which I have offered in explanation of them, this much will be conceded, that it is a district containing many phenomena of novelty and interest, and the study of which is calculated to throw some additional light both upon the structure of the earth and on revolutions which have taken place on its surface.

* See Lyell's Elements, p. 76, 77, for examples, and an explanation of this concretionary structure.

† Geological Researches, p. 95.

Lists of Specimens illustrative of the foregoing Memoir on the Geology of Roxburghshire, and lodged in the Royal Society's Museum.

1. Greywacke, fine-grained and blue, from vertical strata running east and west by compass. Edgerstone Burn foot.
2. Greywacke, coarse and gritty, from a stratum about two feet thick, nearly vertical, running about east and west. From a quarry on turnpike road, two miles north-west of Galashiels.
3. Greywacke, fine-grained and blue, from Do. Below Edgerstone North Lodge, in channel of Jed.
4. Greywacke, coarse and brown. Edgerstone North Lodge, in channel of Jed.
5. Greywacke, coarse and brown. From channel of Jed, where crossed by Hawick trap dyke, near Rink.
6. Greywacke, curiously veined with iron, from channel of Jed, near Peel, in Southdean parish.
7. Another specimen.
8. Do.

8 A. Greywacke, with organic *forms*, produced by structure.

8 B. Pebble of old porphyry, from conglomerate of old red sandstone at Byreslees on the Ale Water.

9. Old red sandstone with white spots, from right bank of Tweed opposite Dryburgh.
10. Five specimens of red and white varieties of old red sandstone, from Denholm Hill, on the north side of Ruberslaw, where it is extensively quarried. The strata dip north at an angle of 8°.
11. Yellow sandstone, slaty, from Ancrum Park, overlaid by red sandstone strata. This yellow sandstone is composed either of disintegrated yellow porphyry, or it is one of the red sandstone rocks from which the colour has been discharged by heat. (See p. 473.)
12. Old red sandstone, distant about 50 yards from porphyry rock, and apparently unaffected by it.
13. Old red sandstone, with a stripe of greenish white. This white stripe is at the side of a crack or fissure in the rock. On the opposite side of the fissure, the red rock has a similar white stripe of the same width. From Jed river, opposite to Fairneyhirst.

13. Another specimen containing two stripes, the specimen having been between two fissures. One end was lately exposed to the heat of my kitchen fire, in order to ascertain the effect in changing the colour.

13 A. Specimens from the same locality, with the red colour nearly all discharged, from some change apparently in the chemical state of the iron.

14. Old red sandstone, with spherical white spots, from south bank of Tweed between Maxton Manse and school-house.
15. Old red sandstone, lying nearly horizontal over vertical strata of greywacke. From river Jed, a little below the north Lodge of Edgerstone. The junction of the two formations is shewn by the woodcut on page 437 of the foregoing paper.
16. Calcareous sandstone, from right bank of Tweed opposite to Dryburgh.
17. Yellow sandstone interstratified with red beds of Do., from right bank of Tweed below Holm House.
18. White sandstone, from Doveston Hill, about a mile north-east of Edgerstone north lodge. A similar stone got and quarried at Kilburn.

18 A. Conglomerate containing fragments of Cheviot porphyry, overlaid by the coal-measures at Millenden Burn. See page 449.

18 B. Pebble of Cheviot porphyry, found in the conglomerate marked 18 A.

18 C. Calcareous sandstone, lying above 18 A. at Mellenden Burn, and alternating with marly shales.

18 D. Conglomerate, from Sunlaws Quarry on Teviot, where the change apparently takes place from the old red sandstone formation to the coal-measures, p. 449.

18 E. Stratum of hardened clay, very common in lower parts of coal-measures, near the junction with old red formation. This specimen from Tweed above Floors Castle.

18 F. Specimens of red and white gypsum, from strata of marl and clay on left bank of Tweed, half way between Floors and Mackerston.

19. Limestone from Limekiln Edge, near Windburgh; extensively quarried. Apparently altered by heat from adjoining trap.

20. Do. from Do.

21. Do. from Carter Fell, on Cheviots; extensively quarried. No shells have been found in it.

22. Do. from Do.

22 A. Coal sandstone, Eccop Hill (Cheviots).

23. Coal sandstone ? brown and gritty, from a glen between Minto village and Minto Manse.

23 A. Marl, from Pinnacle Hill, opposite Kelso.

24. Coal sandstone ? from foot of greenstone hill, opposite to Ancrum Church.

25. Do. ? very gritty, from west side of Minto Greenhills, composed of tufa.

26. Another specimen.

27. Scales of Holoptichius, from old red sandstone, head of Wauchope Burn, east side of Windburgh.

28. Do. of Do., from old red sandstone at Plewlands Quarry, near Maxton school-house.

29. Do. of Do., of a smaller size, and probably belonging to a different species. From same locality.

30. Mould of a palate or other part of Holoptichius, from Jed river, near Peel, in Southdean parish.

Specimens of Sedimentary Rocks altered by heat.

31. Yellow sandstone, very near Craigoer rock of basalt, opposite to Merton. The same stratum at a greater distance possesses the usual red colour of the formation.

32. Coal sandstone slightly altered by felspar porphyry, from Teviot, below Heaton.

33. Marly sandstone of coal formation, altered by overflowing mass of porphyry. Robert's Linn.

34. Do. Do. Do.

35. Do. Do. Do.

36. Do. Do., from Bedrule Hill.

37. Do. Do. Do.

38. Do. Do. Do.

39. Old red sandstone, or some lower member of the coal formation, altered by felspar porphyry. From Liddesdale, four miles west of note of the gate.

42. Pitchstone porphyry, veined with iron, from near Hownam.

44. Jasper, from Jed, opposite Shaws, in the Cheviot porphyry.

45. Cheviot porphyry, from Tofts.

46. Do., from Letham.

47. Pitchstone porphyry. Hownam.

48. Amygdaloidal porphyry. Chatto.

49. Porphyry. Above Morebattle, on Kale Water.

50. Felspar, from quarry west of Maison-dieu, near Kelso.

51. Felspar porphyry, from between Kerseknow and Frogdean.

52. Basalt ? Easter Softlaw.

53. Clinkstone. On Tweed, below Maxton Manse.

54. Felspar porphyry. Plewlands Burn.

55. Felspar porphyry. Plewlands Burn.

56. Amygdaloid, from opposite Mackerston.

57. Felspar, from right bank of Tweed, above Mackerston.

58. Felspar porphyry, from left bank of Tweed, below Mackerston.

59. Felspar porphyry. Opposite Merton, and above Craigoer rock, on Tweed.
60. Basalt, from Craigoer rock, on Tweed.
61. Felspar, with brown mica. Sucklawrig, north of Mackerston.
62. Clinkstone. Woodenburn, near Kelso.
63. Red felspar porphyry, containing greywacke. Easter Eildon Hill.
64. Do. Bowsden Muir.
65. Clinkstone. Muirhouselaw clump.
66. Amygdaloid. Muirhouselaw onstead.
67. Trap tufa, from north-west part of Eildon Hills.
68. Yellow felspar, from the Holm opposite to Dryburgh.
69. Do.
70. Amygdaloid and tufa, from Do.
71. Tufa, from Bedrule Hill.
72. Felspar porphyry, from an extensive coulée on Do.
73. Felspar porphyry, from Do. Do.
74. Claystone porphyry. Ancrum Park.
75. Do., overlaying slaty and yellow strata. (See No. 11.) Do.
76. Greenstone porphyry, from Kirklands. Probably part of same mass or eruption as at Castlehill. There are brown coal? strata close to each horizontal.
77. Felspar or claystone porphyry, from Heaton onstead, at side of turnpike road.
78. Clinkstone from Windburgh.
79. Trap tufa, Ancrum Craigs.
80. Felspar from quarry south of Heaton.
81. Vein of compact red felspar from do. Vein runs north-west, and is about 4 inches thick. Of same quality as vein at back of Springwood Park garden, in channel of Teviot, which runs through felspar similar to No. 80 in a north and south direction. (See No. 91.)
82. Vein of copper from No. 80, united with No. 81.
83. Basalt from Hawick dyke, as seen at Halrule Mill, on Rule Water.
84. Basalt from Limekiln edge, which has flowed over limestone there, and is about 40 fathoms above it.
85. Greenstone from Cow rock, south-west of and near Heaton.
86. Do.
87. Felspar or claystone porphyry from Heaton Mill. (See Nos. 77 and 80.)
88. Clinkstone from Woodhead, on Ale Water, about a mile above Ancrum.
89. Greenstone from the Carter.
90. Do. Southdean Hill.
91. Felspar porphyry from Teviot, back of Springwood Park Garden. (See No. 81.)
92. Basalt from Smailholm Craigs, with opal.
93. Sulphuret of lead from Abbotrule.
94. Fairy stones from Allan or Elland Water, near Melrose.
95. Stones supposed to be formed from similar causes, found in the Kale Water, near Morebattle.

Fragments from Liddesdale boulders, collected and labelled by the Rev. Mr. Barton *of Castleton.*

1. In the bed of the river at the village in great abundance.

×2. Castleton, about a mile south-east of the Manse, so numerous that they resemble a quarry, and some of them so decomposed, that the earth surrounding them, upon being dug up with a spade, presents nothing but their elements.

×3. Tweedenhead, a little farther to the south-east of the former.

4. Powisholm, a very large block, blasted last year, on the west side of the Liddell, about 500 yards to the north of the Manse.

×5. Picked up in the bed of the river at the Manse.

×6. Between Belshiels and the Manse, in a dyke about 300 yards south-east of the latter.

×7. Near Newhouse, on which Mr Milne broke his hammer, about 150 yards from No. 6.

×8. Thorlieshope, about seven miles north-east of the Manse, very abundant for many miles, especially in the direction of the Carter.

9. Liddellbank, about seven miles to the south-west of the Manse; the most southern hill in the parish.

10. Greena, about half a mile to the north of the former.

11. South Burnmouth, on rising ground about a mile to the north of No. 10, in a piece of good land, very numerous,—ugly customers for the farmers.

12, 13, 14. At Ettleton ? and Burying-ground, very numerous.

×15. Kershope. The boundary between England and Scotland, to the south-east of the Manse.

16. Blackburn, about a mile to the north-west of the village.

17. Berrycleuch, to the south-west of Blackburn, in great abundance.

18. Tinnisburn, about two miles to the south of Berrycleuch, in superfluity.

19. The upper millstone of an old *querne*, dug out lately from the foundation of a house to the north of the Manse, on the other side of the river, about 100 yards.

Numbers 2, 3, 5, 6, 7, 8, 15, marked with a ×, are all on the east side of the Liddell. No. 5 is doubtful, being found in the bed of the river.

Numbers 1, 4, 9, 10, 11, 12, 13, 14, 16, 17, 18, are all on the west side of the Liddell, and, as it turns out, by far too numerous for the farmer.

Mr Barton, alongst with the foregoing specimens, sent to Mr Milne a letter, from which the following extracts are made:—

"You will observe that all the specimens from the east of the river, are to be found every where from Kershope, on that side of the river, to the northern extremity of the parish, and are traceable, as I am informed by shepherds, to the Carter, without any difficulty. At No. 8. and for several miles in circumference from it, they abound to nearly the summit of the hills. On the west side of the river, however, I have not yet discovered that they are so abundant, although you will perceive that they are very numerous, especially at Nos. 1, 9, 10, 11, 12, 13, 14, 16, 17, 18, nor that they are to be found so near the summit of the mountains as on the north side. At the same time, I may observe, that the soil to the south of the Manse, and at No. 2, consists solely of the debris of the grey and red granite, as was satisfactorily proved to me, who am no geologist, by a mason, who has some

knowledge of the subject, digging up several spadefuls and pointing out the component parts of the granite, and by convincing me that the hill in front of the Manse, from which the materials for making the road, along which you and Dr B. went on the morning you left, was almost entirely constructed of the same substances. To-day, for he is making some repairs on the Manse, he drew my attention to the decomposition of rocks, by crushing with his foot several blocks of weathered granite, and then comparing them with the adjoining soil, and fully demonstrated, to my satisfaction at least, that the two substances were precisely the same. You will also notice that red granite is found, and that the soil is composed, of a greater proportion of that than of the *grey*. You mention that Criffell is distant 20 miles from this, but if you say 40, from where I write, you will come nearer to the truth, and find that the valleys of Nith, Annan, Esk, Tarras, and Liddell intervene, and present many *barrier acts*, to the establishment of Dr B.'s theory.

" THOMSON has been very active in collecting specimens- If you want more, he or I will furnish them with great pleasure. I hope those sent will arrive in safety."

Fig 4

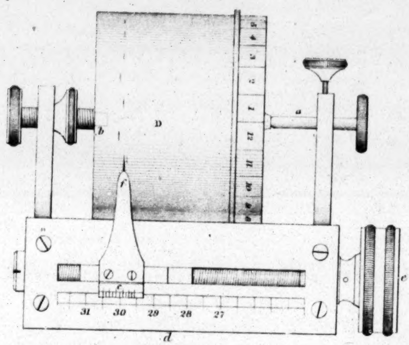


Fig 3

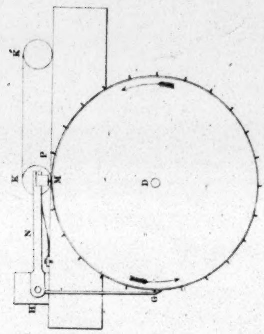


Fig 2

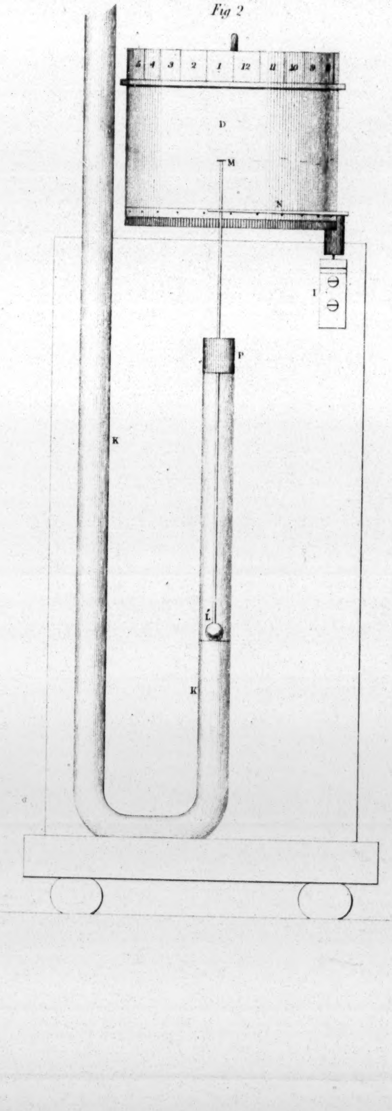
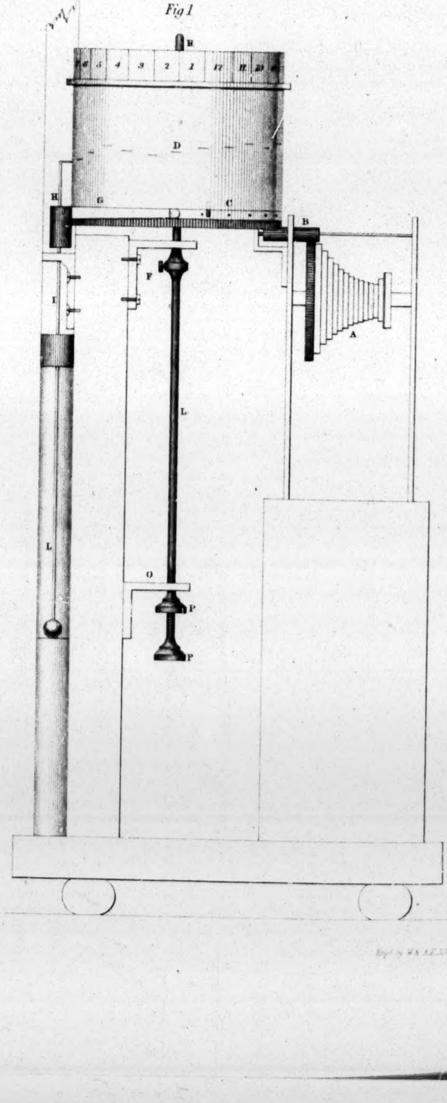


Fig 1



Engd by W & A.K. Johnston

XXXIII.—*Description of a New Self-Registering Barometer.* By ROBERT BRYSON, F.R.S.E.

(Read 2d January 1844.)

ALTHOUGH many proposals have been made to obtain a series of hourly meteorological observations by mechanical means, this desideratum has not, from various causes, been completely attained. The chief obstacle to be overcome, in such self-registering instruments, is the great amount of friction, which necessarily vitiates all the results, more especially in delicate instruments, such as the barometer and thermometer.

Dr ROBERT HOOKE was the first to propose a self-registered series of meteorological observations, by an instrument, which he quaintly called a *Weather-wiser;* but no further notice is taken of this contrivance than a short description in one of his tracts, bearing the date 5th December 1678, which would lead us to believe that the instrument was never used. The late ALEXANDER KEITH, Esq. of Ravelston also proposed a similar contrivance, a description of which is contained in this Society's Transactions, Vol. 4th. This contrivance, from the constant friction excited by the marker on the revolving paper, seems likewise to have been abandoned.

The barometer now to be described does not seem liable to the objection of the others. Fig. 1. exhibits a side-view of the barometer, with the clock-work which moves the cylinder on which the observations are registered. A is the fussee of a spring time-piece, placed between two brass frames, driving a pinion B, which passes through the frame, and is pivoted into a small cock on the backplate, for the purpose of allowing the large horizontal bevel wheel C to *pitch* easily into it. This wheel is, by the pinion B, made to revolve once during twenty-four hours; it carries twenty-four pins, placed at equal distances round its circumference, and is fixed to the spindle L, which works in two puppets F and O, the upper extremity being shewn at R, after passing through the cylinder D. PP are two milled nuts, which are used in adjusting the *pitch* of the wheel C into the pinion B.

D is a tin cylinder about 3 inches in diameter, having the hours marked from 1 to 12 A. M. red, and from 1 to 12 P. M. black. This cylinder has a brass tube soldered through its axis, which fits easily upon the upper part of the spindle L. To enable us to put this cylinder always on at the proper point, a small pin is fixed into the wheel, and may be seen in the figure near C. This pin fits into a small aperture in the bottom of the cylinder, and prevents any lateral motion which would change the marking of the hours; this steady pin prevents

entirely any chance of error when shifting the cylinder, as it will not reach the wheel unless the aperture at the bottom be coincident. I is a small puppet carrying a lever, which is raised every hour by the transit of each pin of the wheel C; the axis of this lever is marked H, and the end where the pins act G. The action of this lever will be better understood by reference to Figs. 2 and 3, where KK represents a bent tube filled with mercury, forming the common syphon barometer. Upon the surface of the mercury floats a spherical ball of ivory L′ attached to a flat steel rod, which passes easily through a nozzle on the open end of the barometer, marked P; it is also passed through a slit in the end of the lever, which is better shewn at P, fig. 3, which represents a section of the instrument. Here the float-rod M is bent at right angles, and in form of a knife-edge, so as to mark the cylinder D; K and K′ are the sectional parts of the barometer tube; H is the axis or pivot of the lever; N is the embracing arm which clasps the float-rod; G is the other arm of the lever, which is acted on by the transit of the pins. From this short description, it will easily be perceived that when the wheel D revolves in the direction of the arrows, each pin, as it passes the bent point of the lever at G, will cause the float-rod M to be pressed against the cylinder, which removes a small line of the white pigment covering the cylinder, and thus indicates the height at which the float stands in the open end of the barometer. When the pin has passed the arm of the lever G, it is forced into its former position by a spring which gives a jerk to the float, and removes for the next observation any adhesion of the mercury to the tube which may have been caused by moisture or otherwise. The operation of marking occupies about eight minutes, during which any change in the height of the mercurial column does not affect the float until it is released from the embrace of the lever by the passing of the pin, when the float is again free to rise or fall with every change of the atmospheric pressure, without any restraint or friction, until the coming pin again brings it in contact with the cylinder.

It is convenient to have seven cylinders, each marked with a day of the week; they are quite detached, may be removed, covered, and replaced, by any person totally unacquainted with the management of instruments. After the cylinders are read, they require merely to be streaked with chalk and water well levigated and applied by a camel's hair brush; and as the cylinders are japanned black, the slight mark made by the point of the float is very easily perceived, as it is a faint black mark on a white ground.

Fig. 4. is a representation of the *reader* for ascertaining the value of each hour's mark on the cylinder; D is the cylinder, with the various registered observations marked upon it; a moveable pivot *a* presses the cylinder against an opposite pivot *b*, which is mounted with two milled nuts, binding it fast to the upright through which it passes; these pivots allow the cylinder to revolve without any *end-shake*, which would vitiate the readings; *d* is the scale where

the values of the markings are indicated by the vernier fixed at *c*; *e* is the milled nut attached to the screw which guides the pointer *f* to the various lines on the cylinder.

When the barometer is first put in action, the scale is adjusted to its proper height by observations made at the same hours of a standard barometer. Should the register be found too high, the fixed pivot *b* is loosened and screwed out until the vernier and scale correspond to the height observed at that hour on the standard barometer, after which it is fixed, and the other observations will be found to correspond within the usual limits of discrepancies in barometric observations.

The following are the observations registered since the barometer was first completed, June 22. 1843. They were made in Princes Street, Edinburgh, at the height of 211 feet above the mean level of the sea.

All the observations are comparable with those at Greenwich and the Royal Society of London, as the clock is kept at Greenwich Mean Time.

HOURLY REGISTER.

HOURLY BAROMETRIC REGISTER.

1843	A. M.												P. M.											
	1	2	3	4	5	6	7	8	9	10	11	12	1	2	3	4	5	6	7	8	9	10	11	12
June 22	...	...	...	...	...	...	...	...	...	...	...	...	29.96	29.96	29.96	29.96	29.96	29.96	29.96	29.96	29.97	29.98	29.94	29.93
23	29.93	29.91	29.90	29.90	29.90	29.90	29.90	29.92	29.93	29.94	29.94	29.94	29.96	29.96	29.96	29.96	29.96	29.96	29.96	29.96	29.96	29.96	29.96	29.96
24	29.96	29.95	29.95	29.95	29.95	29.95	29.95	29.95	29.92	29.92	29.92	29.93	29.95	29.95	29.95	29.95	29.95	29.95	29.96	29.97	29.97	29.97	29.97	29.97
25	29.97	29.97	29.97	29.97	29.97	29.97	29.97	29.97	29.97	29.97	29.96	29.96	29.93	29.91	29.91	29.91	29.91	29.91	29.91	29.86	29.86	29.86	29.86	29.86
26	29.85	29.85	29.83	29.83	29.83	29.83	29.83	29.83	29.83	29.83	29.83	29.83	29.84	29.84	29.84	29.84	29.80	29.82	29.82	29.82	29.82	29.82	29.82	29.82
27	29.80	29.79	29.79	29.78	29.78	29.78	29.78	29.78	29.77	29.76	29.76	29.74	29.74	29.74	29.74	29.74	29.73	29.73	29.71	29.71	29.71	29.71	29.71	29.71
28	29.71	29.69	29.69	29.69	29.66	29.66	29.66	29.66	29.65	29.65	29.65	29.65	29.63	29.63	29.63	29.65	29.65	29.65	29.67	29.67	29.67	29.67	29.67	29.67
29	29.67	29.67	29.67	29.67	29.67	29.67	29.67	29.67	29.67	29.67	29.65	29.65	29.63	29.62	29.62	29.62	29.62	29.62	29.63	29.64	29.64	29.66	29.66	29.66
30	29.66	29.66	29.65	29.65	29.65	29.65	29.65	29.62	29.62	29.62	29.62	29.62	29.61	29.61	29.61	29.61	29.60	29.63	29.63	29.67	29.67	29.67	29.67	29.68
July 1	29.68	29.68	29.68	29.68	29.68	29.68	29.68	29.68	29.70	29.70	29.70	29.70	29.68	29.68	29.68	29.66	29.66	29.63	29.69	29.58	29.58	29.58	29.58	29.58
2	29.58	29.58	29.58	29.58	29.58	29.61	29.61	29.61	29.61	29.61	29.61	29.60	29.60	29.60	29.60	29.58	29.57	29.55	29.53	29.51	29.51	29.51	29.51	29.51
3	29.51	29.51	29.50	29.50	29.50	29.50	29.50	29.51	29.51	29.53	29.56	29.54	29.54	29.55	29.57	29.60	29.63	29.64	29.66	29.68	29.70	29.71	29.72	29.73
4	29.74	29.74	29.74	29.75	29.77	29.77	29.78	29.78	29.80	29.80	29.80	29.77	29.78	29.78	29.77	29.76	29.76	29.76	29.66	29.66	29.76	29.76	29.75	29.74
5	29.72	29.71	29.70	29.69	29.68	29.68	29.68	29.67	29.67	29.66	29.66	29.64	29.64	29.62	29.61	29.58	29.56	29.53	29.51	29.46	29.46	29.44	29.43	29.41
6	29.40	29.40	29.42	29.42	29.42	29.42	29.42	29.42	29.42	29.43	29.46	29.46	29.48	29.51	29.55	29.56	29.59	29.61	29.62	29.67	29.68	29.68	29.68	29.69
7	29.69	29.69	29.70	29.72	29.72	29.72	29.72	29.72	29.70	29.70	29.70	29.72	29.71	29.71	29.71	29.71	29.71	29.72	29.72	29.72	29.74	29.74	29.74	29.75
8	29.74	29.74	29.75	29.75	29.75	29.75	29.75	29.76	29.76	29.78	29.79	29.81	29.82	29.82	29.85	29.85	29.88	29.89	29.91	29.95	29.97	29.97	29.99	30.00
9	30.00	30.00	30.02	30.02	30.04	30.04	30.05	30.05	30.06	30.04	30.04	30.04	30.03	30.02	30.01	30.01	30.01	30.01	30.01	30.01	30.01	30.01	30.03	30.01
10	30.01	30.01	30.01	30.01	30.01	30.01	30.01	30.01	30.01	30.01	30.01	30.01	29.97	29.98	29.96	29.98	29.98	29.98	29.99	30.02	30.04	30.05	30.05	30.05
11	30.06	30.06	30.07	30.07	30.09	30.11	30.13	30.13	30.13	30.14	30.14	30.12	30.12	30.12	30.10	30.09	30.09	30.09	30.09	30.09	30.09	30.09	30.10	30.10
12	30.10	30.09	30.07	30.07	30.06	30.06	30.04	30.02	30.02	30.00	29.98	29.97	29.96	29.97	29.94	29.94	29.94	29.93	29.94	29.94	29.94	29.94	29.94	29.94
13	29.94	29.94	29.94	29.94	29.96	29.96	29.96	29.96	29.96	29.96	29.96	29.95	29.96	29.96	29.95	29.95	29.95	29.95	29.97	29.95	29.95	29.95	29.96	29.96
14	29.96	29.96	29.96	29.96	29.96	29.96	29.95	29.95	29.95	29.94	29.94	29.93	29.92	29.92	29.91	29.91	29.90	29.89	29.89	29.89	29.89	29.88	29.88	29.87
15	29.86	29.85	29.84	29.82	29.82	29.82	29.81	29.80	29.80	29.80	29.81	29.82	29.84	29.85	29.88	29.86	29.89	29.90	29.91	29.94	29.94	29.95	29.95	29.95
16	29.95	29.95	29.94	29.95	29.96	29.96	29.98	29.99	29.98	29.98	29.99	29.99	29.84	29.84	29.85	29.86	29.88	29.90	29.92	29.93	29.93	29.94	29.95	29.95
17	29.95	29.95	29.95	29.95	29.96	29.97	29.97	29.96	29.97	29.98	29.98	29.98	29.99	29.95	29.94	29.93	29.90	29.88	29.87	29.84	29.81	29.79	29.76	29.71
18	29.67	29.65	29.59	29.56	29.54	29.55	29.57	29.59	29.61	29.63	29.64	29.64	29.64	29.65	29.66	29.64	29.65	29.65	29.66	29.67	29.65	29.64	29.65	29.64
19	29.62	29.69	29.66	29.66	29.66	29.66	29.66	29.66	29.66	29.60	29.59	29.58	29.55	29.55	29.55	29.55	29.56	29.56	29.57	29.60	29.60	29.60	29.60	29.60
20	29.59	29.57	29.57	29.56	29.55	29.54	29.53	29.50	29.48	29.45	29.41	29.39	29.34	29.33	29.32	29.32	29.32	29.33	29.34	29.34	29.36	29.37	29.39	29.39
21	29.38	29.38	29.38	29.38	29.39	29.40	29.40	29.41	29.44	29.44	29.45	29.47	29.48	29.50	29.51	29.52	29.54	29.55	29.57	29.59	29.60	29.60	29.60	29.58
22	29.58	29.57	29.57	29.57	29.56	29.56	29.55	29.53	29.53	29.52	29.50	29.48	29.46	29.45	29.44	29.42	29.40	29.37	29.36	29.34	29.32	29.30	29.29	29.27
23	29.27	29.27	29.27	29.29	29.31	29.34	29.39	29.45	29.49	29.54	29.57	29.60	29.61	29.63	29.64	29.66	29.69	29.71	29.72	29.75	29.77	29.78	29.80	29.81
24	29.81	29.81	29.82	29.84	29.85	29.86	29.86	29.86	29.87	29.87	29.87	29.88	29.88	29.88	29.88	29.88	29.89	29.90	29.92	29.94	29.95	29.96	29.96	29.97
25	29.97	29.97	29.97	29.98	29.99	30.00	30.01	30.01	30.01	30.01	30.02	30.02	30.02	30.02	30.02	30.02	30.02	30.03	30.04	30.04	30.03	30.02	30.03	30.03
26	30.03	30.03	30.01	30.00	30.00	29.99	29.97	29.96	29.95	29.94	29.93	29.91	29.89	29.88	29.87	29.86	29.86	29.85	29.84	29.84	29.83	29.83	29.84	29.84
27	29.84	29.84	29.84	29.83	29.83	29.83	29.84	29.83	29.82	29.82	29.82	29.81	29.78	29.78	29.77	29.77	29.78	29.79	29.83	29.82	29.84	29.84	29.85	29.86
28	29.87	29.87	29.86	29.86	29.86	29.86	29.85	29.85	29.84	29.82	29.81	29.79	29.79	29.78	29.75	29.73	29.72	29.70	29.68	29.64	29.62	29.61	29.58	29.56
29	29.54	29.52	29.49	29.48	29.46	29.44	29.41	29.40	29.37	29.35	29.33	29.32	29.29	29.29	29.27	29.26	29.26	29.25	29.26	29.28	29.28	29.30	29.30	29.31
30	29.31	29.31	29.32	29.32	29.32	29.34	29.34	29.35	29.35	29.37	29.37	29.38	29.39	29.39	29.41	29.44	29.46	29.48	29.50	29.53	29.55	29.56	29.58	29.59
31	29.59	29.59	29.61	29.60	29.62	29.64	29.65	29.67	29.67	29.68	29.68	29.68	29.68	29.68	29.68	29.68	29.69	29.69	29.71	29.71	29.71	29.71	29.71	29.70

1843	A. M. 1	2	3	4	5	6	7	8	9	10	11	12	P. M. 1	2	3	4	5	6	7	8	9	10	11	12
Aug. 1	29.71	29.69	29.67	29.65	29.63	29.62	29.61	29.59	29.59	29.58	29.58	29.59	29.59	29.58	29.56	29.56	29.56	29.57	29.56	29.56	29.55	29.54	29.54	29.51
2	29.51	29.49	29.48	29.47	29.46	29.45	29.44	29.41	29.39	29.37	29.36	29.34	29.33	29.34	29.31	29.29	29.27	29.27	29.27	29.27	29.27	29.26	29.25	29.24
3	29.23	29.22	29.20	29.21	29.22	29.22	29.23	29.22	29.22	29.24	29.24	29.23	29.24	29.24	29.24	29.24	29.25	29.25	29.25	29.26	29.26	29.26	29.26	29.26
4	29.26	29.26	29.25	29.25	29.26	29.26	29.27	29.28	29.28	29.28	29.29	29.29	29.31	29.30	29.31	29.32	29.34	29.35	29.37	29.39	29.41	29.43	29.45	29.46
5	29.47	29.48	29.49	29.50	29.51	29.53	29.54	29.54	29.54	29.54	29.54	29.54	29.53	29.53	29.53	29.51	29.50	29.48	29.49	29.50	29.50	29.50	29.50	29.51
6	29.51	29.50	29.51	29.52	29.53	29.54	29.56	29.58	29.60	29.62	29.64	29.66	29.69	29.72	29.74	29.76	29.78	29.81	29.85	29.87	29.90	29.92	29.94	29.95
7	29.95	29.95	29.96	29.96	29.96	29.97	29.96	29.95	29.94	29.93	29.91	29.89	29.87	29.85	29.83	29.80	29.79	29.80	29.83	29.86	29.87	29.88	29.89	29.90
8	29.91	29.92	29.94	29.94	29.95	29.96	29.97	29.96	29.96	29.95	29.95	29.94	29.93	29.91	29.90	29.88	29.89	29.90	29.89	29.90	29.91	29.92	29.94	29.94
9	29.94	29.95	29.95	29.95	29.95	29.96	29.95	29.96	29.97	29.97	29.96	29.96	29.97	29.95	29.95	29.94	29.95	29.95	29.96	29.96	29.97	29.97	29.98	29.98
10	29.98	29.98	29.98	29.98	29.99	29.99	30.00	30.01	30.02	30.03	30.03	30.03	30.03	30.03	30.02	30.03	30.03	30.03	30.04	30.05	30.06	30.06	30.07	30.07
11	30.07	30.07	30.07	30.07	30.07	30.07	30.07	30.07	30.07	30.06	30.08	30.09	30.09	30.07	30.07	30.07	30.07	30.07	30.08	30.08	30.08	30.08	30.09	30.09
12	30.10	30.09	30.10	30.10	30.10	30.10	30.11	30.10	30.10	30.10	30.10	30.09	30.11	30.09	30.08	30.09	30.09	30.10	30.02	30.02	30.03	30.03	30.09	30.04
13	30.04	30.04	30.04	30.04	30.04	30.04	30.03	30.04	30.04	30.02	30.01	30.10	30.08	30.07	30.06	30.05	30.05	30.04	30.04	30.04	30.03	30.03	30.03	30.02
14	30.02	30.00	29.99	29.99	29.99	29.99	29.99	29.99	29.98	29.98	29.97	29.94	29.95	29.95	29.94	29.94	29.94	29.94	29.96	29.95	29.96	29.97	29.97	29.97
15	29.97	29.97	29.97	29.96	29.96	29.96	29.95	29.95	29.95	29.94	29.95	29.95	29.94	29.94	29.92	29.92	29.92	29.92	29.94	29.95	29.95	29.95	29.96	29.96
16	29.96	29.96	29.96	29.96	29.96	29.96	29.97	29.97	29.99	29.99	29.99	29.99	29.98	29.97	29.96	29.96	29.96	29.96	29.97	29.98	29.98	29.98	29.98	29.98
17	29.98	29.98	29.98	29.98	29.98	29.99	29.99	30.00	29.99	29.99	29.99	29.98	29.98	29.98	29.98	29.98	29.97	29.97	29.98	29.99	30.00	30.00	30.00	30.00
18	30.00	30.00	30.00	30.00	30.00	30.00	29.98	29.98	29.98	29.98	29.98	29.98	29.97	29.96	29.96	29.95	29.94	29.93	29.93	29.94	29.94	29.93	29.93	29.94
19	29.93	29.91	29.88	29.87	29.87	29.87	29.85	29.84	29.83	29.81	29.80	29.79	29.77	29.76	29.72	29.68	29.67	29.67	29.65	29.64	29.62	29.62	29.61	29.59
20	29.58	29.57	29.57	29.57	29.58	29.58	29.59	29.59	29.59	29.60	29.60	29.61	29.61	29.61	29.60	29.62	29.63	29.65	29.68	29.70	29.71	29.72	29.73	29.74
21	29.75	29.75	29.75	29.76	29.76	29.76	29.76	29.75	29.74	29.74	29.73	29.71	29.69	29.68	29.66	29.64	29.62	29.59	29.55	29.54	29.52	29.48	29.46	29.41
22	29.36	29.32	29.30	29.29	29.27	29.25	29.24	29.22	29.21	29.19	29.16	29.17	29.16	29.16	29.15	29.15	29.16	29.17	29.18	29.19	29.20	29.21	29.21	29.21
23	29.22	29.22	29.22	29.23	29.24	29.26	29.27	29.30	29.32	29.33	29.33	29.33	29.36	29.37	29.37	29.38	29.38	29.40	29.44	29.46	29.48	29.47	29.48	29.48
24	29.49	29.50	29.52	29.53	29.54	29.56	29.57	29.58	29.59	29.60	29.61	29.61	29.65	29.65	29.64	29.62	29.62	29.62	29.61	29.61	29.61	29.59	29.58	29.57
25	29.60	29.60	29.60	29.60	29.62	29.62	29.62	29.62	29.63	29.63	29.63	29.64	29.57	29.56	29.56	29.56	29.55	29.57	29.58	29.62	29.63	29.64	29.65	29.65
26	29.65	29.66	29.67	29.67	29.68	29.68	29.69	29.69	29.70	29.70	29.70	29.70	29.69	29.69	29.69	29.69	29.71	29.72	29.74	29.76	29.78	29.80	29.81	29.83
27	29.84	29.84	29.86	29.86	29.88	29.89	29.89	29.89	29.89	29.87	29.87	29.87	29.87	29.87	29.86	29.85	29.84	29.84	29.84	29.84	29.84	29.84	29.83	29.83
28	29.83	29.81	29.79	29.78	29.77	29.76	29.75	29.75	29.72	29.70	29.66	29.62	29.60	29.57	29.54	29.54	29.50	29.46	29.46	29.46	29.48	29.50	29.51	29.53
29	29.55	29.57	29.60	29.63	29.64	29.68	29.72	29.73	29.75	29.77	29.79	29.79	29.82	29.83	29.83	29.83	29.88	29.89	29.91	29.93	29.94	29.94	29.94	29.95
30	29.95	29.96	29.97	29.99	30.00	30.01	30.02	30.04	30.02	30.02	30.02	30.02	30.01	30.01	30.02	30.03	30.04	30.03	30.04	30.05	30.05	30.04	30.04	30.06
31	30.06	30.06	30.06	30.04	30.04	30.05	30.05	30.05	30.05	30.06	30.05	30.05	30.05	30.05	30.05	30.05	30.04	30.04	30.04	30.05	30.06	30.05	30.05	30.04
Sept. 1	30.05	30.05	30.06	30.07	30.09	30.11	30.12	30.14	30.16	30.15	30.15	30.17	30.17	30.18	30.18	30.18	30.18	30.18	30.18	30.19	30.18	30.20	30.19	30.18
2	30.18	30.18	30.18	30.17	30.16	30.16	30.15	30.15	30.16	30.16	30.16	30.16	30.14	30.13	30.13	30.13	30.13	30.13	30.11	30.10	30.08	30.06	30.02	30.00
3	30.00	29.97	29.96	29.94	29.94	29.92	29.90	29.91	29.90	29.90	29.90	29.89	29.86	29.85	29.86	29.86	29.89	29.92	29.96	29.99	30.00	30.02	30.04	30.03
4	30.05	30.05	30.05	30.05	30.07	30.10	30.10	30.12	30.14	30.14	30.16	30.17	30.18	30.18	30.19	30.20	30.21	30.23	30.25	30.26	30.26	30.28	30.28	30.28
5	30.29	30.30	30.29	30.28	30.28	30.28	30.27	30.27	30.27	30.26	30.25	30.24	30.22	30.21	30.19	30.18	30.18	30.19	30.19	30.18	30.19	30.18	30.18	30.18
6	30.18	30.16	30.16	30.16	30.16	30.16	30.16	30.16	30.14	30.14	30.14	30.14	30.11	30.10	30.08	30.08	30.08	30.09	30.09	30.10	30.10	30.10	30.10	30.10
7	30.10	30.10	30.10	30.10	30.10	30.11	30.11	30.11	30.11	30.11	30.12	30.11	30.11	30.10	30.10	30.09	30.09	30.09	30.09	30.12	30.12	30.12	30.12	30.12
8	30.11	30.11	30.11	30.11	30.12	30.12	30.13	30.13	30.12	30.12	30.12	30.11	30.10	30.09	30.09	30.09	30.09	30.09	30.09	30.10	30.10	30.10	30.10	30.10
9	30.10	30.10	30.10	30.09	30.08	30.09	30.09	30.09	30.09	30.08	30.07	30.07	30.05	30.03	30.01	30.01	30.01	30.01	30.01	30.00	30.00	30.00	30.00	30.00
10	30.00	29.99	29.98	29.97	29.97	29.97	29.97	29.97	29.96	29.96	29.94	29.91	29.89	29.87	29.87	29.85	29.85	29.85	29.84	29.83	29.82	29.82	29.81	29.82
11	29.82	29.79	29.79	29.79	29.80	29.82	29.83	29.85	29.87	29.89	29.91	29.93	29.95	29.97	29.98	29.99	30.01	30.03	30.06	30.08	30.09	30.11	30.12	30.13
12	30.14	30.15	30.15	30.16	30.19	30.21	30.22	30.22	30.23	30.24	30.23	30.22	30.21	30.22	30.21	30.21	30.20	30.20	30.21	30.21	30.21	30.21	30.21	30.21
13	30.21	30.20	30.19	30.17	30.16	30.16	30.16	30.16	30.14	30.13	30.12	30.11	30.09	30.07	30.04	30.01	30.01	30.00	30.01	30.01	30.00	30.00	30.00	29.99
14	29.97	29.96	29.95	29.94	29.94	29.93	29.93	29.92	29.92	29.91	29.91	29.90	29.87	29.87	29.88	29.86	29.86	29.86	29.86	29.87	29.88	29.88	29.88	29.89
15	29.89	29.89	29.87	29.87	29.87	29.87	29.87	29.88	29.88	29.88	29.87	29.87	29.85	29.85	29.85	29.85	29.86	29.86	29.87	29.87	29.87	29.87	29.86	29.85
16	29.85	29.84	29.84	29.84	29.83	29.83	29.84	29.84	29.85	29.85	29.85	29.85	29.84	29.83	29.83	29.87	29.87	29.89	29.91	29.91	29.92	29.90	29.91	29.91
17	29.91	29.91	29.91	29.91	29.92	29.94	29.94	29.95	29.95	29.96	29.97	29.97	29.97	29.96	29.96	29.96	29.96	29.96	29.96	29.96	29.95	29.94	29.94	29.92
18	29.89	29.87	29.85	29.85	29.83	29.81	29.80	29.80	29.80	29.80	29.86	29.89	29.90	29.90	29.90	29.93	29.96	29.99	30.02	30.04	30.05	30.07	30.09	30.11
19	30.12	30.13	30.13	30.14	30.15	30.16	30.17	30.17	30.17	30.17	30.16	30.16	30.15	30.15	30.12	30.11	30.10	30.10	30.10	30.10	30.08	30.08	30.06	30.03
20	30.03	30.01	30.00	29.98	29.98	29.96	29.95	29.95	29.93	29.91	29.90	29.88	29.87	29.87	29.86	29.86	29.86	29.86	29.86	29.86	29.86	29.86	29.86	29.86
21	29.88	29.88	29.88	29.89	29.90	29.92	29.94	29.96	29.98	29.98	30.00	30.02	30.12	30.02	30.03	30.05	30.06	30.08	30.13	30.15	30.15	30.17	30.18	30.20
22	30.22	30.24	30.25	30.26	30.27	30.29	30.31	30.32	30.33	30.35	30.35	30.32	30.32	30.40	30.40	30.40	30.40	30.42	30.42	30.45	30.45	30.45	30.45	30.43
23	30.34	30.32	30.34	30.34	30.34	30.34	30.36	30.37	30.37	30.38	30.40	30.40	30.42	30.41	30.40	30.39	30.38	30.40	30.40	30.42	30.43	30.43	30.43	30.43
24	30.43	30.41	30.41	30.41	30.40	30.40	30.40	30.41	30.41	30.40	30.40	30.39	30.37	30.35	30.32	30.32	30.32	30.31	30.31	30.29	30.28	30.28	30.26	30.26
25	30.24	30.22	30.20	30.22	30.22	30.22	30.22	30.22	30.22	30.21	30.21	30.21	30.24	30.24	30.23	30.22	30.22	30.22	30.22	30.22	30.22	30.22	30.21	30.20
26	30.19	30.18	30.17	30.16	30.15	30.15	30.14	30.13	30.12	30.12	30.10	30.08	30.08	30.06	30.03	30.00	29.98	29.96	29.96	29.94	29.92	29.92	29.91	29.90
27	29.87	29.86	29.83	29.82	29.82	29.81	29.81	29.79	29.78	29.77	29.77	29.76	29.76	29.76	29.76	29.75	29.76	29.76	29.77	29.77	29.77	29.77	29.77	29.77
28	29.77	29.77	29.77	29.77	29.77	29.79	29.81	29.81	29.81	29.81	29.81	29.81	29.81	29.80	29.80	29.81	29.81	29.82	29.83	29.84	29.85	29.87	29.87	29.87
29	29.87	29.86	29.87	29.88	29.88	29.89	29.89	29.89	29.89	29.89	29.87	29.86	29.86	29.83	29.83	29.81	29.79	29.76	29.745	29.720	29.691	29.659	29.629	29.609
30	29.588	29.568	29.543	29.532	29.530	29.559	29.570	29.593	29.610	29.635	29.649	29.670	29.660	29.666	29.675	29.679	29.699	29.681	29.680	29.672	29.659	29.648	29.649	29.647

1843	A. M. 1	2	3	4	5	6	7	8	9	10	11	12	P. M. 1	2	3	4	5	6	7	8	9	10	11	12
Oct. 1	29.667	29.646	29.659	29.653	29.679	29.695	29.681	29.689	29.701	29.714	29.705	29.705	29.682	29.701	29.678	29.678	29.676	29.670	29.678	29.689	29.715	29.725	29.735	29.732
2	29.735	29.730	29.723	29.730	29.740	29.751	29.759	29.779	29.782	29.800	29.805	29.815	29.820	29.819	29.821	29.835	29.837	29.866	29.881	29.897	29.905	29.910	29.915	29.917
3	29.917	29.908	29.901	29.900	29.910	29.905	29.894	29.889	29.890	29.904	29.901	29.901	29.909	29.906	29.910	29.910	29.910	29.910	29.912	29.928	29.928	29.936	29.939	29.930
4	29.935	29.920	29.919	29.912	29.916	29.905	29.903	29.905	29.910	29.902	29.889	29.890	29.901	29.887	29.882	29.879	29.871	29.879	29.881	29.880	29.889	29.880	29.880	29.869
5	29.860	29.860	29.860	29.865	29.861	29.869	29.871	29.865	29.858	29.851	29.851	29.850	29.821	29.811	29.791	29.771	29.759	29.749	29.735	29.728	29.694	29.669	29.652	29.610
6	29.575	29.530	29.511	29.471	29.461	29.431	29.411	29.391	29.375	29.361	29.345	29.315	29.305	29.291	29.260	29.248	29.226	29.209	29.200	29.199	29.181	29.166	29.179	29.165
7	29.165	29.165	29.165	29.171	29.175	29.185	29.191	29.205	29.215	29.216	29.239	29.240	29.245	29.211	29.205	29.199	29.190	29.182	29.190	29.161	29.130	29.109	29.103	29.091
8	29.085	29.079	29.093	29.094	29.110	29.129	29.141	29.150	29.168	29.182	29.209	29.225	29.245	29.255	29.265	29.292	29.310	29.334	29.355	29.372	29.394	29.400	29.412	29.409
9	29.395	29.378	29.396	29.380	29.365	29.371	29.365	29.375	29.390	29.400	29.412	29.448	29.418	29.431	29.460	29.493	29.525	29.559	29.596	29.620	29.649	29.660	29.691	29.699
10	29.701	29.705	29.705	29.711	29.715	29.715	29.715	29.714	29.712	29.709	29.699	29.681	29.715	29.697	29.679	29.660	29.650	29.630	29.622	29.590	29.570	29.549	29.529	29.481
11	29.461	29.410	29.369	29.332	29.302	29.270	29.230	29.211	29.182	29.159	29.135	29.106	29.071	29.040	29.013	28.990	28.979	28.950	28.959	28.970	28.973	28.990	29.020	29.022
12	29.030	29.030	29.060	29.071	29.100	29.145	29.179	29.222	29.270	29.310	29.320	29.329	29.320	29.309	29.289	29.280	29.294	29.323	29.363	29.368	29.399	29.416	29.419	29.424
13	29.424	29.428	29.439	29.441	29.459	29.460	29.462	29.460	29.450	29.431	29.412	29.412	29.416	29.419	29.410	29.412	29.415	29.430	29.455	29.469	29.519	29.560	29.574	29.595
14	29.610	29.620	29.620	29.642	29.669	29.675	29.705	29.720	29.730	29.738	29.732	29.735	29.721	29.705	29.704	29.700	29.700	29.700	29.700	29.690	29.690	29.689	29.680	29.680
15	29.678	29.678	29.678	29.660	29.671	29.671	29.672	29.675	29.675	29.679	29.679	29.675	29.675	29.656	29.660	29.669	29.671	29.682	29.680	29.690	29.682	29.680	29.674	29.674
16	29.663	29.648	29.626	29.612	29.601	29.589	29.582	29.570	29.554	29.549	29.523	29.510	29.500	29.460	29.445	29.424	29.411	29.402	29.400	29.379	29.355	29.348	29.340	29.340
17	29.335	29.344	29.349	29.355	29.372	29.398	29.410	29.440	29.465	29.499	29.515	29.532	29.560	29.582	29.600	29.630	29.670	29.705	29.720	29.742	29.759	29.770	29.790	29.802
18	29.810	29.810	29.805	29.815	29.831	29.840	29.856	29.875	29.900	29.924	29.945	29.955	29.976	30.062	30.010	30.030	30.059	30.079	30.100	30.123	30.142	30.152	30.159	30.172
19	30.200	30.200	30.202	30.205	30.232	30.254	30.256	30.260	30.250	30.270	30.282	30.265	30.229	30.220	30.210	30.192	30.192	30.170	30.146	30.135	30.132	30.128	30.115	30.099
20	30.080	30.039	30.040	30.023	30.019	30.000	30.000	30.000	29.989	29.969	29.960	29.940	29.925	29.904	29.880	29.846	29.812	29.792	29.765	29.712	29.702	29.658	29.610	29.575
21	29.562	29.562	29.554	29.560	29.595	29.610	29.650	29.682	29.712	29.740	29.760	29.774	29.765	29.771	29.769	29.765	29.780	29.795	29.801	29.789	29.778	29.766	29.758	29.739
22	29.708	29.670	29.654	29.630	29.612	29.580	29.572	29.572	29.564	29.557	29.554	29.557	29.542	29.539	29.524	29.510	29.510	29.510	29.509	29.502	29.503	29.508	29.514	29.525
23	29.542	29.557	29.570	29.594	29.612	29.626	29.670	29.689	29.695	29.700	29.671	29.689	29.668	29.665	29.610	29.558	29.519	29.500	29.499	29.492	29.465	29.475	29.465	29.465
24	29.455	29.418	29.402	29.395	29.398	29.394	29.389	29.383	29.370	29.359	29.381	29.340	29.322	29.305	29.282	29.295	29.275	29.285	29.285	29.286	29.278	29.278	29.270	29.273
25	29.281	29.271	29.260	29.259	29.259	29.252	29.259	29.268	29.260	29.270	29.262	29.252	29.255	29.250	29.245	29.241	29.241	29.245	29.245	29.252	29.255	29.260	29.265	29.265
26	29.279	29.281	29.289	29.301	29.320	29.351	29.369	29.391	29.400	29.409	29.405	29.409	29.406	29.406	29.410	29.415	29.425	29.432	29.460	29.465	29.484	29.495	29.500	29.505
27	29.505	29.500	29.502	29.502	29.499	29.499	29.495	29.492	29.465	29.459	29.459	29.435	29.410	29.396	29.365	29.350	29.340	29.320	29.305	29.291	29.290	29.263	29.235	29.200
28	29.160	29.118	29.064	29.026	28.982	28.920	28.873	28.829	28.790	28.748	28.695	28.630	28.618	28.600	28.600	28.605	28.625	28.640	28.652	28.680	28.700	28.715	28.736	28.751
29	28.760	28.791	28.808	28.837	28.881	28.940	28.992	29.030	29.075	29.110	29.153	29.181	29.195	29.210	29.250	29.275	29.314	29.346	29.365	29.380	29.406	29.425	29.435	29.450
30	29.449	29.450	29.456	29.462	29.470	29.462	29.471	29.466	29.462	29.461	29.450	29.420	29.400	29.385	29.360	29.345	29.325	29.320	29.300	29.245	29.232	29.200	29.195	29.200
31	29.185	29.185	29.190	29.200	29.220	29.255	29.275	29.300	29.325	29.355	29.375	29.400	29.400	29.455	29.470	29.485	29.524	29.542	29.548	29.545	29.550	29.548	29.555	29.552
Nov. 1	29.550	29.549	29.552	29.562	29.572	29.572	29.590	29.610	29.622	29.647	29.654	29.665	29.675	29.675	29.680	29.702	29.710	29.715	29.725	29.735	29.750	29.752	29.752	29.761
2	29.768	29.760	29.755	29.755	29.772	29.790	29.800	29.801	29.810	29.815	29.818	29.800	29.785	29.795	29.764	29.755	29.750	29.728	29.741	29.726	29.720	29.721	29.700	29.680
3	29.650	29.620	29.605	29.598	29.588	29.563	29.550	29.550	29.516	29.510	29.495	29.466	29.461	29.435	29.405	29.395	29.385	29.376	29.367	29.355	29.351	29.351	29.344	29.320
4	29.295	29.288	29.265	29.250	29.248	29.244	29.245	29.295	29.330	29.371	29.400	29.436	29.450	29.452	29.467	29.491	29.509	29.542	29.565	29.601	29.620	29.660	29.701	29.725
5	29.669	29.770	29.795	29.818	29.850	29.863	29.881	29.910	29.912	29.901	29.899	29.889	29.871	29.855	29.842	29.840	29.805	29.790	29.765	29.740	29.710	29.650	29.650	29.635
6	29.560	29.540	29.512	29.485	29.472	29.455	29.462	29.472	29.480	29.491	29.520	29.525	29.532	29.555	29.564	29.590	29.600	29.601	29.599	29.582	29.560	29.540	29.500	29.420
7	29.382	29.328	29.265	29.232	29.222	29.211	29.220	29.250	29.265	29.300	29.315	29.325	29.360	29.380	29.390	29.406	29.403	29.403	29.400	29.379	29.375	29.358	29.359	29.350
8	29.331	29.312	29.295	29.287	29.274	29.270	29.274	29.302	29.315	29.346	29.368	29.415	29.459	29.495	29.516	29.558	29.589	29.620	29.666	29.708	29.715	29.748	29.765	29.786
9	29.793	29.800	29.815	29.824	29.834	29.840	29.863	29.860	29.860	29.845	29.836	29.805	29.760	29.715	29.680	29.648	29.630	29.600	29.550	29.501	29.465	29.428	29.408	29.363
10	29.330	29.300	29.282	29.270	29.270	29.270	29.289	29.308	29.320	29.350	29.360	29.399	29.430	29.441	29.472	29.514	29.545	29.590	29.615	29.635	29.680	29.709	29.727	29.745
11	29.765	29.795	29.810	29.837	29.870	29.905	29.945	29.978	30.005	30.020	30.050	30.044	30.040	30.035	30.041	30.036	30.049	30.052	30.060	30.055	30.070	30.075	30.069	30.032
12	30.055	30.055	30.055	30.055	30.055	30.049	30.049	30.055	30.035	30.005	29.995	29.990	29.970	29.959	29.954	29.948	29.944	29.935	29.940	29.952	29.952	29.952	29.952	29.956
13	29.962	29.962	29.962	29.985	30.006	30.015	30.021	30.030	30.045	30.055	30.062	30.062	30.062	30.060	30.060	30.064	30.070	30.090	30.100	30.100	30.100	30.100	30.099	30.094
14	30.094	30.106	30.110	30.113	30.125	30.146	30.165	30.188	30.200	30.210	30.215	30.219	30.225	30.235	30.240	30.251	30.265	30.277	30.285	30.285	30.295	30.280	30.265	30.249
15	30.215	30.215	30.195	30.168	30.130	30.100	30.065	30.039	30.002	29.980	29.950	29.888	29.855	29.700	29.746	29.730	29.758	29.785	29.804	29.832	29.860	29.875	29.905	29.918
16	29.921	29.930	29.940	29.946	29.960	29.971	30.002	30.009	30.011	30.018	30.005	29.999	29.950	29.915	29.910	29.890	29.871	29.855	29.820	29.800	29.761	29.745	29.710	29.670
17	29.619	29.600	29.565	29.517	29.490	29.451	29.431	29.410	29.370	29.348	29.335	29.310	29.320	29.320	29.320	29.328	29.317	29.311	29.300	29.295	29.279	29.268	29.259	29.240
18	29.235	29.240	29.230	29.217	29.200	29.193	29.185	29.185	29.164	29.164	29.165	29.158	29.125	29.113	29.120	29.120	29.136	29.148	29.140	29.159	29.163	29.162	29.175	29.167
19	29.199	29.210	29.222	29.250	29.290	29.305	29.303	29.340	29.351	29.369	29.376	29.376	29.400	29.400	29.400	29.415	29.415	29.413	29.419	29.419	29.410	29.400	29.390	29.352
20	29.309	29.274	29.240	29.209	29.190	29.160	29.152	29.155	29.160	29.192	29.220	29.249	29.263	29.292	29.301	29.345	29.350	29.352	29.340	29.310	29.291	29.260	29.250	29.236
21	29.200	29.199	29.199	29.190	29.196	29.196	29.203	29.220	29.249	29.260	29.270	29.265	29.235	29.285	29.254	29.245	29.205	29.165	29.096	29.041	28.939	28.899	28.886	28.868
22	28.835	28.830	28.821	28.804	28.718	28.778	28.782	28.800	28.842	28.902	29.012	29.113	29.200	29.245	29.275	29.310	29.355	29.385	29.403	29.408	29.423	29.415	29.410	29.409
23	29.392	29.385	29.360	29.348	29.340	29.340	29.348	29.348	29.336	29.326	29.335	29.310	29.289	29.275	29.261	29.265	29.272	29.290	29.293	29.293	29.305	29.310	29.324	29.340
24	29.340	29.345	29.346	29.355	29.375	29.392	29.405	29.439	29.451	29.455	29.475	29.475	29.500	29.503	29.506	29.520	29.526	29.534	29.550	29.555	29.570	29.570	29.580	29.585
25	29.565	29.561	29.556	29.556	29.554	29.550	29.550	29.545	29.541	29.551	29.520	29.505	29.500	29.506	29.491	29.470	29.465	29.470	29.465	29.463	29.456	29.444	29.417	29.382
26	29.380	29.345	29.300	29.271	29.260	29.250	29.235	29.246	29.252	29.250	29.250	29.246	29.241	29.210	29.195	29.180	29.170	29.175	29.199	29.203	29.200	29.192	29.180	29.167
27	29.151	29.151	29.148	29.121	29.103	29.100	29.100	29.100	29.094	29.090	29.082	29.090	29.093	29.110	29.123	29.170	29.200	29.240	29.270	29.345	29.400	29.440	29.474	29.520
28	29.560	29.603	29.640	29.678	29.715	29.755	29.790	29.802	29.828	29.845	29.878	29.870	29.864	29.851	29.819	29.785	29.757	29.708	29.671	29.686	29.765	29.792	29.805	29.840
29	29.871	29.900	29.916	29.935	29.966	29.980	29.992	30.017	30.035	30.065	30.080	30.111	30.139	30.135	30.160	30.175	30.220	30.247	30.272	30.280	30.284	30.284	30.274	30.265
30	30.250	30.226	30.215	30.212	30.214	30.199	30.156	30.140	30.115	30.071	30.041	30.010	29.925	29.895	29.845	29.810	29.805	29.805	29.825	29.828	29.850	29.885	29.900	29.950

	A. M.												P. M.											
1843	1	2	3	4	5	6	7	8	9	10	11	12	1	2	3	4	5	6	7	8	9	10	11	12
Dec. 1	29.971	29.988	29.999	29.999	30.015	30.020	30.020	30.010	30.062	30.051	30.058	30.058	30.055	30.050	30.035	30.035	30.061	30.046	30.040	30.045	30.032	30.050	30.000	30.010
2	30.000	29.988	29.970	29.945	29.941	29.928	29.935	29.945	29.958	29.958	29.965	29.950	29.937	29.940	29.950	29.978	30.010	30.120	30.040	30.063	30.068	30.068	30.080	30.100
3	30.110	30.118	30.120	30.120	30.130	30.131	30.135	30.148	30.130	30.129	30.116	30.113	30.110	30.094	30.075	30.059	30.077	30.090	30.091	30.095	30.095	30.000	30.095	30.095
4	30.090	30.079	30.079	30.079	30.090	30.080	30.096	30.105	30.115	30.120	30.100	30.080	30.020	29.990	29.956	29.961	29.943	29.919	29.893	29.872	29.855	29.833	29.795	29.789
5	29.730	29.728	29.715	29.642	29.611	29.585	29.517	29.538	29.525	29.532	29.546	29.530	29.520	29.545	29.521	29.553	29.564	29.584	29.610	29.646	29.656	29.671	29.703	29.716
6	29.716	29.720	29.730	29.723	29.730	29.745	29.760	29.790	29.822	29.840	29.870	29.895	29.925	29.935	29.951	29.961	29.961	29.961	29.970	29.965	30.000	29.980	29.970	29.976
7	29.945	29.930	29.910	29.887	29.858	29.835	29.811	29.776	29.747	29.726	29.700	22.700	29.658	29.618	29.585	29.595	29.535	29.592	29.616	29.650	29.684	29.705	29.760	29.772
8	29.805	29.807	29.832	29.855	29.875	29.898	29.907	29.915	29.930	29.948	29.929	29.943	29.930	29.915	29.960	29.952	29.991	30.000	30.005	30.010	30.020	30.025	30.040	30.052
9	30.062	30.068	30.071	30.069	30.075	30.089	30.088	30.100	30.096	30.100	30.101	30.080	30.061	30.055	30.035	30.035	30.022	30.018	30.025	30.010	30.008	30.010	30.011	30.003
10	30.000	30.005	30.016	30.016	30.032	30.032	30.032	30.048	30.062	30.070	30.072	30.069	30.050	30.060	30.066	30.062	30.059	30.050	30.055	30.055	30.040	30.020	30.016	30.007
11	30.005	30.000	29.976	29.949	29.931	29.921	29.926	29.919	29.939	29.930	29.959	29.959	29.972	29.966	29.981	30.010	30.022	30.042	30.055	30.065	30.080	30.092	30.123	30.125
12	30.125	30.132	30.151	30.156	30.146	30.146	30.154	30.163	30.178	30.192	30.196	30.185	30.205	30.175	30.175	30.175	30.180	30.190	30.180	30.170	30.159	30.149	30.141	30.130
13	30.120	30.105	30.083	30.069	30.059	30.059	30.065	30.048	30.059	30.080	30.080	30.045	30.069	30.069	30.066	30.080	30.069	30.095	30.090	30.107	30.000	30.091	30.096	30.096
14	30.090	30.090	30.055	30.055	30.055	30.055	30.055	30.063	30.054	30.054	30.040	30.030	30.007	29.990	29.950	29.902	29.900	29.905	29.905	29.896	29.850	29.828	29.812	29.789
15	29.751	29.751	29.740	29.751	29.779	29.807	29.820	29.830	29.860	29.880	29.900	29.919	29.950	29.955	29.950	29.959	29.960	29.970	29.980	29.995	30.008	30.020	30.042	30.054
16	30.060	30.062	30.064	30.055	30.066	30.082	30.092	30.100	30.115	30.128	30.156	30.165	30.170	30.152	30.152	30.152	30.145	30.141	30.139	30.115	30.102	30.083	30.069	30.050
17	30.037	30.049	30.059	30.070	30.099	30.100	30.105	30.125	30.146	30.161	30.170	30.191	30.191	30.202	30.196	30.200	30.210	30.215	30.223	30.220	30.230	30.240	30.240	30.245
18	30.230	30.240	30.230	30.217	30.213	30.218	30.223	30.230	30.243	30.236	30.254	30.254	30.243	30.238	30.230	30.230	30.250	30.244	30.246	30.246	30.246	30.238	30.233	30.228
19	30.223	30.215	30.203	30.200	30.185	30.185	30.175	30.169	30.165	30.175	30.180	30.170	30.180	30.169	30.169	30.166	30.160	30.160	30.161	30.155	30.150	30.160	30.151	30.151
20	30.151	30.166	30.150	30.134	30.120	30.102	30.085	30.080	30.038	30.068	30.060	30.034	30.014	30.005	29.983	29.992	29.980	29.979	29.965	29.942	29.919	29.919	29.907	29.906
21	29.909	29.906	29.940	29.974	29.986	30.020	30.051	30.080	30.115	30.147	30.150	30.156	30.159	30.154	30.154	30.158	30.155	30.132	30.138	30.125	30.102	30.107	30.088	30.064
22	30.055	30.019	30.000	29.974	29.973	29.946	29.900	29.868	29.820	29.764	29.758	29.749	29.703	29.688	29.700	29.700	29.716	29.708	29.711	29.704	29.735	29.750	29.749	29.746
23	29.815	29.803	29.862	29.925	29.939	29.963	30.005	30.020	30.028	30.051	30.064	30.064	30.050	30.015	30.008	30.011	30.011	30.006	30.010	30.005	29.990	29.990	29.984	29.984
24	29.998	29.985	29.997	29.990	30.002	30.013	30.025	30.035	30.060	30.081	30.100	30.118	30.114	30.132	30.159	30.180	30.134	30.209	30.221	30.240	30.261	30.261	30.283	30.280
25	30.270	30.260	30.255	30.236	30.229	30.229	30.200	30.210	30.200	30.208	30.199	30.177	30.174	30.150	30.127	30.114	30.120	30.110	30.110	30.095	30.106	30.100	30.092	30.090
26	30.078	30.078	30.085	30.100	30.101	30.110	30.120	30.125	30.146	30.155	30.150	30.154	30.158	30.160	30.161	30.170	30.170	30.170	30.165	30.169	30.173	30.178	30.170	30.173
27	30.162	30.155	30.150	30.154	30.147	30.147	30.147	30.156	30.170	30.196	30.200	30.193	30.191	30.174	30.172	30.195	30.205	30.200	30.210	30.216	30.217	30.222	30.229	30.220
28	30.228	30.220	30.215	30.221	30.215	30.221	30.230	30.240	30.260	30.260	30.280	30.262	30.240	30.225	30.215	30.218	30.200	30.194	30.200	30.195	30.195	30.196	30.183	30.184
29	30.169	30.158	30.162	30.158	30.152	30.137	30.137	30.130	30.135	30.125	30.119	30.108	30.104	30.090	30.077	30.085	30.075	30.074	30.050	30.050	30.048	30.031	30.017	30.004
30	29.972	29.942	29.930	29.924	29.900	29.892	29.885	29.899	29.874	29.868	29.850	29.858	29.794	29.774	29.764	29.759	29.752	29.709	29.690	29.665	29.649	29.617	29.599	29.570
31	29.552	29.517	29.498	29.465	29.483	29.506	29.510	29.520	29.530	29.542	29.532	29.515	29.510	29.509	29.509	29.511	29.510	29.502	29.500	29.496	29.491	29.485	29.465	29.432
1844																								
Jan. 1	29.420	29.415	29.410	29.396	29.375	29.362	29.399	29.381	29.384	29.399	29.361	29.316	29.315	29.306	29.306	29.310	29.310	29.306	29.302	29.300	29.296	29.308	29.303	29.310
2	29.315	29.321	29.341	29.362	29.380	29.412	29.450	29.476	29.505	29.526	29.556	29.563	29.599	29.620	29.645	29.654	29.666	29.679	29.689	29.665	29.646	29.620	29.618	29.585
3	29.560	29.560	29.550	29.530	29.510	29.468	29.488	29.480	29.470	29.480	29.500	29.517	29.515	29.506	29.500	29.508	29.502	29.499	29.481	29.468	29.460	29.440	29.420	29.410
4	29.395	29.395	29.400	29.402	29.410	29.419	29.452	29.479	29.510	29.540	29.550	29.553	29.549	29.560	29.560	29.552	29.548	29.530	29.520	29.509	29.494	29.470	29.435	29.395
5	29.359	29.313	29.274	29.231	29.202	29.187	29.176	29.170	29.160	29.136	29.108	29.090	29.093	29.071	29.055	29.049	29.020	28.998	28.990	28.974	28.962	28.995	28.950	28.935
6	28.911	28.895	28.890	28.890	28.883	28.870	28.871	28.886	28.902	28.916	28.925	28.925	28.915	28.940	28.950	28.966	28.971	28.995	29.020	29.050	29.067	29.085	29.103	29.117
7	29.151	29.170	29.198	29.210	29.210	29.260	29.272	29.307	29.329	29.350	29.368	29.363	29.370	29.390	29.406	29.421	29.445	29.460	29.485	29.510	29.532	29.550	29.579	29.601
8	29.622	29.650	29.660	29.690	29.700	29.716	29.765	29.801	29.820	29.846	29.856	29.875	29.880	29.915	29.945	29.965	29.975	29.985	29.996	30.020	30.040	30.056	30.056	30.070
9	30.078	30.068	30.078	30.068	30.079	30.070	30.092	30.062	30.065	30.042	30.030	30.020	30.020	29.999	29.979	29.961	29.932	29.925	29.908	29.908	29.908	29.910	29.916	29.906
10	29.910	29.920	29.930	29.938	29.934	29.934	29.942	29.931	29.946	29.946	29.968	29.976	30.002	30.019	30.039	30.055	30.058	30.071	30.090	30.091	30.099	30.099	30.104	30.100
11	30.110	30.115	30.126	30.143	30.153	30.165	30.171	30.186	30.207	30.200	30.195	30.195	30.180	30.162	30.155	30.158	30.145	30.124	30.124	30.112	30.100	30.089	30.066	30.055
12	30.034	30.012	30.004	29.978	29.954	29.935	29.920	29.900	29.880	29.870	29.884	29.875	29.865	29.870	29.874	29.890	29.894	29.900	29.910	29.900	29.910	29.920	29.923	29.930
13	29.940	29.945	29.950	29.960	29.966	29.980	29.993	30.014	30.038	30.051	30.051	30.035	30.045	30.049	30.049	30.051	30.055	30.072	30.095	30.120	30.133	30.159	30.180	30.200
14	30.215	30.235	30.245	30.261	30.281	30.295	30.318	30.339	30.350	30.351	30.364	30.368	30.316	30.313	30.317	30.315	30.326	30.326	30.343	30.338	30.332	30.320	30.312	30.307
15	30.295	30.269	30.266	30.253	30.258	30.258	30.258	30.226	30.220	30.231	30.215	30.201	30.210	30.192	30.204	30.206	30.213	30.212	30.210	30.200	30.205	30.205	30.195	30.180
16	30.170	30.195	30.148	30.145	30.144	30.148	30.140	30.145	30.142	30.160	30.150	30.132	30.110	30.089	30.089	30.099	30.100	30.100	30.110	30.120	30.116	30.124	30.121	30.118
17	30.096	30.089	30.085	30.080	30.079	30.075	30.060	30.071	30.089	30.088	30.100	30.095	30.080	30.057	30.056	30.048	30.048	30.038	30.031	30.035	30.035	30.030	30.030	30.030
18	30.030	30.010	30.008	30.003	29.999	29.990	30.000	30.000	30.000	29.980	29.960	29.938	29.899	29.860	29.831	29.810	29.776	29.771	29.733	29.730	29.715	29.684	29.678	29.665
19	29.660	29.698	29.714	29.710	29.708	29.700	29.690	29.670	29.660	29.651	29.649	29.656	29.656	29.670	29.695	29.715	29.725	29.745	29.750	29.760	29.774	29.795	29.809	29.818
20	29.825	29.817	29.817	29.840	29.830	29.838	29.858	29.870	29.889	29.890	29.894	29.880	29.900	29.888	29.879	29.878	29.855	29.864	29.834	29.825	29.806	29.791	29.770	29.750
21	29.728	29.716	29.712	29.710	29.720	29.726	29.722	29.730	29.735	29.746	29.750	29.760	29.749	29.756	29.766	29.770	29.774	29.780	29.786	29.782	29.805	29.793	29.790	29.780
22	29.773	29.769	29.769	29.764	29.761	29.765	29.761	29.765	29.785	29.780	29.789	29.789	29.779	29.770	29.786	29.803	29.806	29.820	29.820	29.825	29.843	29.857	29.868	29.862
23	29.867	29.870	29.869	29.883	29.887	29.887	29.896	29.908	29.916	29.914	29.912	29.931	29.922	29.918	29.925	29.931	29.950	29.950	29.953	29.962	29.968	29.970	29.974	29.979
24	29.975	29.971	29.975	29.968	29.970	29.962	29.967	29.976	29.971	29.961	29.961	29.965	29.972	29.963	29.959	29.959	29.959	29.951	29.949	29.945	29.954	29.957	29.952	29.958
25	29.950	29.941	29.955	29.944	29.931	29.907	29.899	29.900	29.863	29.851	29.803	29.767	29.711	29.686	29.711	29.780	29.850	29.883	29.910	29.932	29.983	29.992	30.000	29.992
26	30.004	30.000	30.008	30.008	30.010	30.022	30.032	30.040	30.052	30.045	30.040	30.045	30.026	30.038	30.049	30.063	30.069	30.080	30.080	30.070	30.062	30.050	30.063	30.040
27	29.991	29.970	29.979	29.948	29.935	29.920	29.926	29.935	29.926	29.935	29.925	29.923	29.937	29.928	29.911	29.906	29.894	29.874	29.860	29.821	29.810	29.804	29.780	29.735
28	29.699	29.633	29.600	29.554	29.504	29.479	29.483	29.520	29.522	29.520	29.546	29.533	29.527	29.530	29.539	29.538	29.548	29.560	29.572	29.600	29.617	29.625	29.640	29.660
29	29.699	22.668	29.683	29.672	29.685	29.685	29.670	29.670	29.620	29.585	29.538	29.448	29.400	29.322	29.256	29.235	29.250	29.258	29.274	29.270	29.270	29.256	29.255	29.255
30	29.240	29.240	29.269	29.303	29.355	29.370	29.370	29.382	29.410	29.420	29.439	29.442	29.432	29.400	29.395	29.383	29.355	29.360	29.390	29.360	29.382	29.400	29.400	29.415
31	29.415	29.412	29.412	29.435	29.429	29.438	29.438	29.475	29.510	29.533	29.550	29.561	29.585	29.598	29.606	29.624	29.642	29.660	29.675	29.702	29.712	29.725	29.725	29.746

1844	A. M. 1	2	3	4	5	6	7	8	9	10	11	12	P. M. 1	2	3	4	5	6	7	8	9	10	11	12
Feb. 1	29.758	29.770	29.793	29.825	29.850	29.864	29.884	29.905	29.915	29.923	29.931	29.916	29.907	29.880	29.855	29.826	29.800	29.750	29.742	29.700	29.671	29.636	29.605	29.580
2	29.540	29.495	29.450	29.421	29.402	29.399	29.398	29.405	29.412	29.428	29.448	29.455	29.463	29.470	29.500	29.545	29.580	29.621	29.670	29.711	29.759	29.790	29.811	29.852
3	29.864	29.880	29.885	29.905	29.916	29.925	29.940	29.956	29.958	29.969	29.974	29.971	29.973	29.960	29.948	29.928	29.910	29.898	29.872	29.836	29.820	29.797	29.765	29.726
4	29.683	29.632	29.581	29.552	29.522	29.480	29.449	29.446	29.448	29.450	29.455	29.446	29.422	29.418	29.418	29.418	29.422	29.416	29.415	29.415	29.405	29.405	29.420	29.395
5	29.385	29.367	29.350	29.345	29.342	29.338	29.318	29.315	29.308	29.309	29.312	29.309	29.312	29.312	29.318	29.330	29.330	29.332	29.328	29.335	29.338	29.345	29.354	29.358
6	29.355	29.358	29.358	29.355	29.362	29.368	29.362	29.368	29.368	29.361	29.359	29.355	29.334	29.330	29.310	29.300	29.298	29.295	29.295	29.290	29.262	29.252	29.232	29.200
7	29.165	29.104	29.055	29.004	28.955	28.880	28.842	28.851	28.865	28.855	28.868	28.850	28.865	28.860	28.845	28.842	28.830	28.825	28.825	28.820	28.804	28.820	28.820	28.815
8	28.805	28.800	28.800	28.810	28.810	28.810	28.815	28.815	28.810	28.809	28.799	28.860	28.800	28.790	28.790	28.785	28.782	28.790	28.800	28.800	28.800	28.800	28.802	28.790
9	28.800	28.800	28.815	28.830	28.850	28.870	28.892	28.914	28.938	28.962	28.979	29.025	29.052	29.104	29.141	29.176	29.222	29.255	29.310	29.345	29.370	29.396	29.406	29.410
10	29.416	29.417	29.413	29.415	29.434	29.440	29.454	29.476	29.500	29.510	29.546	29.560	29.587	29.580	29.596	29.600	29.614	29.650	29.660	29.680	29.709	29.715	29.731	29.747
11	29.758	29.768	29.774	29.788	29.806	29.820	29.836	29.858	29.863	29.870	29.870	29.878	29.878	29.870	29.872	29.885	29.890	29.900	29.905	29.910	29.905	29.905	29.898	29.882
12	29.866	29.842	29.826	29.810	29.800	29.782	29.772	29.772	29.755	29.746	29.764	29.770	29.763	29.763	29.769	29.785	29.804	29.810	29.816	29.820	29.830	29.840	29.845	29.850
13	29.838	29.846	29.836	29.825	29.831	29.820	29.820	29.820	29.835	29.830	29.829	29.829	29.814	29.810	29.808	29.799	29.789	29.798	29.803	29.795	29.781	29.786	29.782	29.770
14	29.746	29.741	29.728	29.706	29.703	29.695	29.693	29.688	29.695	29.687	29.680	29.690	29.670	29.654	29.628	29.632	29.626	29.615	29.615	29.614	29.603	29.592	29.556	29.536
15	29.504	29.498	29.471	29.470	29.477	29.466	29.478	29.488	29.500	29.519	29.550	29.576	29.595	29.600	29.610	29.645	29.659	29.690	29.708	29.733	29.747	29.760	29.765	29.776
16	29.785	29.780	29.780	29.785	29.790	29.785	29.795	29.805	29.805	29.810	29.823	29.828	29.816	29.805	29.798	29.792	29.791	29.781	29.770	29.770	29.770	29.768	29.751	29.760
17	29.766	29.760	29.754	29.747	29.751	29.748	29.758	29.766	29.764	29.746	29.759	29.751	29.765	29.746	29.730	29.713	29.705	29.700	29.697	29.684	29.677	29.665	29.653	29.641
18	29.620	29.600	29.569	29.550	29.545	29.537	29.520	29.507	29.498	29.480	29.462	29.410	29.395	29.355	29.343	29.310	29.289	29.258	29.235	29.204	29.166	29.139	29.110	29.100
19	29.084	29.057	29.042	29.128	29.138	29.034	29.055	29.071	29.100	29.121	29.164	29.201	29.235	29.259	29.290	29.310	29.320	29.359	29.416	29.445	29.460	29.480	29.496	29.500
20	29.499	29.500	29.520	29.550	29.571	29.598	29.604	29.620	29.635	29.635	29.649	29.650	29.630	29.611	29.603	29.604	29.600	29.602	29.598	29.579	29.559	29.555	29.513	29.469
21	29.449	29.416	29.400	29.406	29.408	29.392	29.372	29.364	29.353	29.345	29.344	29.330	29.335	29.306	29.296	29.290	29.290	29.295	29.295	29.290	29.293	29.290	29.290	29.298
22	29.300	29.306	29.311	29.315	29.346	29.355	29.381	29.406	29.440	29.455	29.463	29.474	29.444	29.435	29.422	29.410	29.410	29.415	29.430	29.430	29.442	29.448	29.455	29.462
23	29.480	29.475	29.470	29.465	29.478	29.485	29.485	29.485	29.460	29.432	29.395	29.374	29.336	29.260	29.180	29.145	29.067	28.997	28.932	28.897	28.851	28.813	28.780	28.755
24	28.755	28.750	28.760	28.794	28.830	28.884	28.914	28.968	29.005	29.045	29.085	29.124	29.175	29.198	29.211	29.238	29.256	29.295	29.323	29.355	29.370	29.390	29.414	29.438
25	29.444	29.440	29.430	29.420	29.400	29.367	29.340	29.270	29.205	29.150	29.080	29.049	28.995	28.918	28.892	28.868	28.862	28.845	28.839	28.826	28.814	28.794	28.770	29.753
26	28.740	28.725	28.716	28.704	28.703	28.703	28.715	28.735	28.750	28.776	28.804	28.835	28.825	28.890	28.923	28.960	28.900	29.050	29.095	29.197	29.154	29.180	29.200	29.218
27	29.218	29.231	29.225	29.225	29.220	29.220	29.200	29.181	29.152	29.135	29.120	29.110	29.120	29.112	29.135	29.132	29.142	29.145	29.152	29.162	29.175	29.195	29.210	29.223
28	29.230	29.225	29.230	29.232	29.245	29.250	29.252	29.259	29.259	29.251	29.230	29.223	29.199	29.181	29.152	29.132	29.150	29.162	29.192	29.205	29.205	29.212	29.212	29.217
29	29.250	29.260	29.285	29.295	29.310	29.314	29.340	29.352	29.362	29.355	29.351	29.355	29.350	29.312	29.295	29.245	29.216	29.195	29.160	29.100	29.042	28.975	28.905	28.850
Mar. 1	28.800	28.871	28.755	28.788	28.800	28.820	28.830	28.860	28.865	28.870	28.895	28.902	28.920	28.915	28.920	28.905	28.905	28.820	28.820	28.874	28.872	28.860	28.855	28.851
2	28.860	28.846	28.839	28.832	28.826	28.822	28.854	28.848	28.832	28.846	28.842	28.850	28.856	28.851	28.846	28.845	28.821	28.820	28.808	28.797	28.784	28.777	28.760	28.750
3	28.715	28.709	28.700	28.682	28.684	28.690	28.680	28.680	28.680	28.670	28.676	28.665	28.655	28.660	28.676	28.746	28.806	28.871	28.932	28.971	29.015	29.060	29.095	29.143
4	29.171	29.185	29.195	29.205	29.203	29.240	29.266	29.300	29.315	29.342	29.365	29.400	29.409	29.429	29.451	29.462	29.462	29.492	29.505	29.523	29.550	29.565	29.594	29.603
5	29.607	29.612	29.612	29.615	22.625	29.635	29.635	29.645	29.640	29.635	29.631	29.625	29.608	29.604	29.593	29.596	29.596	29.600	29.605	29.610	29.605	29.610	29.605	29.605
6	29.607	29.607	29.607	29.616	29.631	29.650	29.670	29.700	29.704	29.702	29.749	29.766	29.798	29.803	29.810	29.820	29.851	29.862	29.896	29.906	29.925	29.940	29.952	29.958
7	29.963	29.972	29.985	30.005	30.015	30.020	30.045	30.055	30.060	30.076	30.086	30.086	30.080	30.060	30.050	30.035	30.035	30.050	30.059	30.062	30.060	30.053	30.033	30.020
8	30.010	30.005	29.994	29.994	29.966	29.966	29.922	29.920	29.880	29.855	29.825	29.805	29.768	29.715	29.668	29.655	29.635	29.621	29.612	29.592	29.600	29.580	29.552	29.525
9	29.498	29.455	29.440	29.409	29.405	29.390	29.375	29.335	29.276	29.244	29.190	29.138	29.168	29.230	29.360	29.450	29.509	29.530	29.571	29.599	29.599	29.588	29.595	29.581
10	29.579	29.566	29.552	29.547	29.552	29.559	29.550	29.556	29.564	29.579	29.582	29.591	29.585	29.571	29.564	29.560	29.545	29.535	29.519	29.475	29.428	29.408	29.340	29.314
11	29.265	29.265	29.241	29.216	29.190	29.140	29.100	29.060	29.000	28.990	28.973	28.965	28.951	28.940	28.956	28.941	28.966	28.975	28.985	28.977	28.983	28.996	29.003	28.998
12	29.016	29.020	29.038	29.030	29.029	29.016	29.012	29.040	29.070	29.091	29.108	29.162	29.162	29.220	29.322	29.362	29.423	29.498	29.555	29.598	29.608	29.644	29.650	29.666
13	29.660	29.662	29.660	29.670	29.692	29.698	29.707	29.731	29.730	29.742	29.751	29.774	29.771	29.770	29.760	29.763	29.760	29.760	29.760	29.755	29.745	29.740	29.736	29.728
14	29.728	29.710	29.707	29.703	29.700	29.698	29.698	29.692	29.676	29.661	29.648	29.632	29.605	29.589	29.559	29.543	29.512	29.503	29.499	22.492	29.468	29.460	29.452	29.442
15	29.426	29.408	29.392	29.372	29.359	29.352	29.356	29.356	29.350	29.350	29.358	29.368	29.368	29.380	29.385	29.398	29.408	29.420	29.442	29.452	29.478	29.500	29.508	29.522
16	29.550	29.562	29.595	29.610	29.632	29.650	29.658	29.698	29.704	29.722	29.748	29.768	29.796	29.806	29.822	29.822	29.852	29.892	29.908	29.922	29.960	29.972	29.995	30.009
17	30.018	30.035	30.049	30.060	30.082	30.099	30.102	30.110	30.118	30.130	30.142	30.152	30.148	30.145	30.142	30.142	30.145	30.145	30.156	30.160	30.152	30.160	30.151	30.149
18	30.142	30.123	30.105	30.099	30.075	30.068	30.072	30.055	30.052	30.052	30.045	30.035	30.010	30.000	29.975	29.985	29.980	29.994	29.990	29.986	29.992	29.979	29.978	29.970
19	29.964	29.955	29.942	29.924	29.918	29.912	29.900	29.892	29.879	29.862	29.839	29.816	29.779	29.755	29.730	29.713	29.685	29.679	29.642	29.612	29.568	29.530	29.485	29.445
20	29.412	29.375	29.348	29.332	29.322	29.312	29.324	29.376	29.440	29.485	29.528	29.572	29.616	29.635	29.646	29.660	29.674	29.702	29.730	29.753	29.770	29.786	29.804	29.800
21	29.800	29.800	29.800	29.805	29.799	29.806	29.812	29.796	29.792	29.780	29.760	29.744	29.715	29.692	29.660	29.636	29.622	29.606	29.603	29.509	29.564	29.553	29.526	29.500
22	29.482	29.455	29.430	29.397	29.382	29.352	29.340	29.316	29.295	29.281	29.268	29.256	29.250	29.246	29.245	29.240	29.252	29.261	29.292	29.299	29.306	29.321	29.330	29.345
23	29.352	29.353	29.366	29.383	29.398	29.414	29.425	29.446	29.455	29.448	29.447	29.455	29.452	29.439	29.425	29.402	29.386	29.376	29.361	29.340	29.303	29.292	29.286	29.244
24	29.211	29.159	29.092	29.050	29.000	28.990	28.965	28.981	28.996	28.995	29.016	29.028	29.047	29.071	29.092	29.105	29.130	29.162	29.194	29.202	29.232	29.250	29.257	29.267
25	29.285	29.282	29.279	29.255	29.267	29.258	29.251	29.243	29.222	29.199	29.172	29.118	29.061	28.998	28.945	28.940	28.939	28.940	28.952	28.944	28.985	29.050	29.110	29.170
26	29.218	29.261	29.285	29.305	29.351	29.394	29.400	29.432	29.440	29.445	29.453	29.455	29.455	29.459	29.455	29.457	29.451	29.465	29.469	29.464	29.455	29.459	29.443	29.436
27	29.426	29.421	29.405	29.404	29.411	29.425	29.440	29.445	29.465	29.495	29.505	29.530	29.559	29.575	29.600	29.622	29.652	29.705	29.763	29.805	29.846	29.886	29.910	29.925
28	29.954	29.995	30.008	30.012	30.038	30.052	30.060	30.090	30.100	30.125	30.134	30.142	30.126	30.100	30.096	30.092	30.115	30.098	30.124	30.136	30.151	30.156	30.161	30.187
29	30.190	30.194	30.200	30.205	30.216	30.239	30.232	30.255	30.250	30.270	30.283	30.272	30.270	30.264	30.265	30.265	30.268	30.273	30.285	30.282	30.296	30.286	30.280	30.288
30	30.275	30.273	30.270	30.268	30.268	30.276	30.283	30.280	30.280	30.280	30.275	30.267	30.260	30.245	30.230	30.212	30.207	30.214	30.220	30.220	30.217	30.198	30.210	30.196
31	30.192	30.180	30.169	30.159	30.163	30.163	30.163	30.165	30.149	30.126	30.119	30.098	30.077	30.055	30.027	30.000	29.990	29.985	29.995	30.004	30.011	30.008	30.010	30.002

XXXIV.—*On the Vibrations of an Interrupted Medium. By the* Rev. PHILIP KELLAND, M.A., F.R.S.S.L. & E., *Professor of Mathematics in the University of Edinburgh.*

(Read January 15. 1844.)

IN certain investigations which I have presented to the Society, relative to the modifications which light undergoes when it meets with a medium more dense than that in which it is travelling, the law of force has been supposed to be that of the inverse square of the distance. On re-examination of this subject, I find that there is no necessity for restricting the computations by the hypothesis of any particular law. The conclusions are perfectly independent of the law, provided one of the equations of reduction, the value of which it is not possible to compute by any known methods of analysis, be admitted as an experimental result. The object of the present Memoir is twofold: 1*st*, To present the analysis of the general theorem of vibrations at the surface of an interrupted medium in its most simple form; and, 2*d*, To apply the results to the case of reflection unaccompanied by refraction. To accomplish the former object, I have, after deducing the equations of motion, sought to determine the values of the different constants which enter them, by means of the condition of symmetry. This investigation has led me to the generalization of some of the remarkable results which M. Cauchy has given relative to this subject, besides presenting me with some other conclusions, one of which is remarkable for its simplicity and completeness. It is this: A particle in a medium substantially symmetrical, will, when displaced from its position of equilibrium, be acted on by no accelerating paces; it will consequently not be urged farther from its position of rest, at least until the other particles shall have been displaced. This is an important theorem, for it removes one objection to the possibility of attractive particles forming a system of stable equilibrium. In reference to the latter part of my investigation, I have only to remark that the equations cannot be solved in all their generality, inasmuch as they contain two more unknown quantities than the number of equations. But by approximation (omitting all terms of high orders of small quantities), I find that, in general, when there is no refraction, the reflection is equal to the incidence, and the rays suffer no retardation. But when the medium is crystallized, that is, when the forces within it have a different value from those in air, the reflected light is less than the incident, and the vibrations in the plane of incidence suffer retardation.

In order to avoid repetition, and to be as brief as possible, I will adopt the figure at p. 40 of the Memoir "On the polarization of light reflected at the surface of a crystal."—Vol. xv. The notation may also remain similar to that of the article referred to, with the exception that R_x, R_y, R_z; T_x, T_y, T_z stand for the normal (or lost) vibrations reflected and transmitted.

$$\therefore \quad \alpha = (\mathrm{I}-\mathrm{R}) \sin \phi + \mathrm{R}_x$$
$$\&\text{c.} = \quad \&\text{c.}$$

and $$\delta\, \alpha = -2\, \mathrm{I} \sin^2 \frac{k\, i}{2} \sin \phi + 2\, \mathrm{R} \sin^2 \frac{k\, r}{2} \sin \phi + \frac{1}{e} \frac{d\, \mathrm{I}}{d\, x} \sin k\, i \sin \phi$$

$$-\frac{1}{e}\frac{d\, \mathrm{R}}{d\, x} \sin k\, r \sin \phi - \mathrm{R}_x (1 - e^{-m\, \delta\, x} \cos f\, \delta\, y) + \frac{d\, \mathrm{R}_x}{d\, y} \frac{1}{f} e^{-m\, \delta\, x} \sin f\, \delta\, y,$$

$\delta\, \beta$ differs from the same quantity in the article referred to, by

$$\delta\, \mathrm{R}_y \text{ or } - \mathrm{R}_y (1 - e^{-m\, \delta\, x} \cos f\, \delta\, y) + \frac{d\, \mathrm{R}_y}{d\, y} \frac{1}{f} e^{-m\, \delta\, x} \sin f\, \delta\, y$$

$\delta\, \gamma$ differs from the like quantity, by

$$\delta\, \mathrm{R}_z \text{ or } - \mathrm{R}_z (1 - e^{-m\, \delta\, x} \cos f\, \delta\, y) + \frac{d\, \mathrm{R}_z}{d\, y} \frac{1}{f} e^{-m\, \delta\, x} \sin f\, \delta\, y$$

$\delta\, \beta_{,}$ and $\delta\, \gamma_{,}$ differ in the same way from the same quantities in the previous Memoir.

Also (p. 44) the equation for the vibration parallel to the axis of x is

$$\frac{d^2 \alpha}{d\, t^2} = \Sigma \left(\phi\, r + \frac{\phi'\, r}{r} \delta\, x^2\right) \Big\{ -2\, \mathrm{I} \sin \phi \sin^2 \frac{k\, i}{2} + 2\, \mathrm{R} \sin \phi \sin^2 \frac{k\, r}{2}$$

$$+ \frac{1}{e}\frac{d\, \mathrm{I}}{d\, x} \sin \phi \sin k\, i - \frac{1}{e}\frac{d\, \mathrm{R}}{d\, x} \sin \phi \sin k\, r - \mathrm{R}_x (1 - e^{-m\, \delta\, x} \cos f\, \delta\, y) + \frac{d\, \mathrm{R}_x}{d\, y} \frac{1}{f} e^{-m\, \delta\, x} \sin f\, \delta\, y \Big\}$$

$+$ &c.

Now, for the motion in an ordinary medium, we have

$$2\, \Sigma \left(\phi\, r + \frac{\phi'\, r}{r} p^2\right) \sin^2 \frac{k\, i}{2} = \frac{c^2}{2};$$

where i is in the direction of incidence, and p perpendicular to it.

Hence, also, $\quad \delta\, x = i \cos \phi + p \sin \phi, \;\; \delta\, y = -i \sin \phi + p \cos \phi.$

By substititution, we obtain

$$\Sigma \left(\phi\, r + \frac{\phi'\, r}{r} \delta\, x^2\right) \delta\, \alpha = \Sigma \left(\phi\, r + \frac{\phi'\, r}{r} \overline{i^2 \cos^2 \phi + p^2 \sin^2 \phi}\right) \times$$

$$\Big\{ -2\, \mathrm{I} \sin \phi \sin^2 \frac{k\, i}{2} + 2\, \mathrm{R} \sin \phi \sin^2 \frac{k\, r}{2} + \frac{1}{e}\frac{d\, \mathrm{I}}{d\, x} \sin \phi \sin k\, i - \frac{1}{e}\frac{d\, \mathrm{R}}{d\, x} \sin \phi \sin k\, r$$

$$- \mathrm{R}_x (1 - e^{-m\, \delta\, x} \cos f\, \delta\, y) + \frac{d\, \mathrm{R}_x}{d\, y} \frac{1}{f} e^{-m\, \delta\, x} \sin f\, \delta\, y \Big\},$$

$$\Sigma \frac{\phi' r}{r} \delta x \delta y \delta \beta = \Sigma \frac{\phi' r}{r} (-i^2+p^2) \sin \phi \cos^2 \phi \left\{ -2\,\mathrm{I} \sin^2 \frac{k i}{2} \right.$$

$$\left. + 2\,\mathrm{R} \sin^2 \frac{k r}{2} + \frac{1}{e} \frac{d\mathrm{I}}{dx} \sin k i - \frac{1}{e} \frac{d\mathrm{R}}{dx} \sin k r \right\}$$

$$- \Sigma \frac{\phi' r}{r} \delta x \delta y\, \mathrm{R}_y (1 - e^{-m \delta x} \cos f \delta y) + \Sigma \frac{\phi' r}{r} \delta x \delta y \frac{1}{f} \frac{d \mathrm{R}_y}{dy} e^{-m \delta x} \sin f \delta y$$

the sum of which is

$$\Sigma (\phi r + \frac{\phi' r}{r} p^2) \sin \phi \left\{ -2\,\mathrm{I} \sin^2 \frac{k i}{2} + 2\,\mathrm{R} \sin^2 \frac{kr}{2} + \frac{1}{e} \frac{d\mathrm{I}}{dx} \sin k i \right.$$

$$\left. - \frac{1}{e} \frac{d\mathrm{R}}{dx} \sin k r \right\} - \Sigma (\phi r + \frac{\phi' r}{r} \delta x^2) \left\{ \mathrm{R}_x \overline{1 - e^{-m \delta x} \cos f \delta y} - \frac{d\mathrm{R}_x}{dy} \frac{1}{f} e^{-m \delta x} \sin f \delta y \right\}$$

$$- \Sigma \frac{\phi' r}{r} \delta x \delta y \left\{ \mathrm{R}_y (1 - e^{-m \delta x} \sin f \delta y) - \frac{d\mathrm{R}_y}{dy} \frac{1}{f} e^{-m \delta x} \sin f \delta y \right\}$$

$$= \frac{c^2}{2} \sin \phi\, (-\mathrm{I} + \mathrm{R}) + \frac{\mathrm{M}}{e} \sin \phi \left(\frac{d\mathrm{I}}{dx} - \frac{d\mathrm{R}}{dx} \right)$$

$$- \mathrm{D}\,\mathrm{R}_x + \frac{\mathrm{F}}{f} \frac{d \mathrm{R}_y}{dy}$$

where $\mathrm{M} = \Sigma \left\{ \phi r + \frac{\phi' r}{r} \delta x^2 \pm \frac{\phi' r}{r} \delta x \delta y \frac{\cos \phi}{\sin \phi} \right\} \sin (e \delta x \pm f \delta y)$

$$= \Sigma (\phi r + \frac{\phi' r}{r} \delta x^2) \sin e \delta x \cos f \delta y + \frac{\cos \phi}{\sin \phi} \Sigma \frac{\phi' r}{r} \delta x \delta y \cos e \delta x \sin f dy$$

for both the incident and the reflected wave.

$$\mathrm{D} = \Sigma (\phi r + \frac{\phi' r}{r} \delta x^2) (1 - e^{-m \delta x} \cos f \delta y)$$

$$\mathrm{F} = \Sigma \frac{\phi' r}{r} \delta x \delta y\, e^{-m \delta x} \sin f \delta y.$$

Again, $\Sigma (\phi r' + \frac{\phi' r'}{r'} \delta x'^2) \delta a_{,} = \Sigma (\phi r' + \frac{\phi' r'}{r'} \overline{t^2 \cos^2 \overline{\phi} + p^2 \sin^2 \overline{\phi}})$

$$\left\{ -2\,\mathrm{T} \sin \phi_{,} \cos \theta \sin^2 \frac{k e}{2} + 2\,\mathrm{T}' \sin \phi' \sin \theta' \sin^2 \frac{k o}{2} \right.$$

$$\left. + \frac{1}{e_{,}} \frac{d\mathrm{T}}{dx} \sin \phi_{,} \cos \theta \sin k_{,} e - \frac{1}{e'} \frac{d\mathrm{T}'}{dx} \sin \phi' \sin \theta' \sin k' o \right\}$$

$$+ \Sigma\ (\phi r' + \frac{\phi' r'}{r'} \delta x'^2) \left\{ -\mathrm{T}_x (1 - e^{-m_{,} \delta x} \cos f \delta y) + \frac{1}{f} \frac{d \mathrm{T}_x}{dy} e^{-m_{,} \delta x} \sin f \delta y \right\}$$

where t stands for either e or o, as the case may be; and $\overline{\phi}$ for $\phi_{,}$ or ϕ':

and $\Sigma \frac{\phi' r'}{r'} \delta x' \delta y' \delta \beta_{,} = \Sigma \frac{\phi' r'}{r'} \sin \phi_{,} \cos \phi_{,} (-e^2 + p^2) (-2\,\mathrm{T} \cos \phi_{,} \cos \theta \sin^2 \frac{k_{,} e}{2}$

$$+\frac{1}{e_{,}}\frac{d\,\mathrm{T}}{d\,x}\cos\phi_{,}\cos\theta\sin k_{,}e) + \Sigma\frac{\phi' r'}{r'}\sin\phi'\cos\phi'\,(-o^2+p^2)\,(2\,\mathrm{T}'\cos\phi'\sin\theta'\sin^2\frac{k'o}{2}$$

$$-\frac{1}{e'}\frac{d\,\mathrm{T}'}{d\,x}\cos\phi'\sin\theta'\sin k'o) + \Sigma\frac{\phi' r'}{r'}\,\delta\,x'\,\delta\,y'\frac{1}{f}\frac{d\,\mathrm{T}_y}{d\,y}\,e^{-m\beta x}\sin f\,d\,y\,;$$

the sum of which is

$$\Sigma\,(\phi\,r'+\frac{\phi'\,r'}{r'}p^2)\,(-2\,\mathrm{T}\sin\phi_{,}\cos\theta\sin^2\frac{k\,e}{2}+2\,\mathrm{T}'\sin\phi'\sin\theta'\sin^2\frac{k\,o}{2}$$

$$+\frac{1}{e_{,}}\frac{d\,\mathrm{T}}{d\,x}\sin\phi_{,}\cos\theta\sin k_{,}e-\frac{1}{e'}\frac{d\,\mathrm{T}'}{d\,x}\sin\phi'\sin\theta'\sin k'\,o)$$

$$-\mathrm{D}_{,}\,\mathrm{T}_x + \mathrm{F}_{,}\frac{1}{f}\frac{d\,\mathrm{T}_y}{d\,y}$$

$$=-\frac{c^2}{2}\mathrm{T}\sin\phi_{,}\cos\theta+\frac{c^2}{2}\mathrm{T}'\sin\phi'\sin\theta'+\frac{\mathrm{M}_{,}}{e_{,}}\frac{d\,\mathrm{T}}{d\,x}\sin\phi_{,}\cos\theta$$

$$-\frac{\mathrm{M}'}{e'}\frac{d\,\mathrm{T}'}{d\,x}\sin\phi'\sin\theta'-\mathrm{D}_{,}\mathrm{T}_x+\frac{\mathrm{F}_{,}}{f}\frac{d\,\mathrm{T}_y}{d\,y}$$

Lastly, $\quad \Sigma\,(\phi\,r'+\frac{\phi'\,r'}{r'}\,\delta\,x'^2)\,(\alpha_{,}-\alpha)=\mathrm{Q}_{x_{,}}\,(\alpha_{,}-\alpha).$

Hence, adding these terms together, we get

$$\frac{d^2\,\alpha}{d\,t^2}=\frac{c^2}{2}\sin\phi\,(-\mathrm{I}+\mathrm{R})+\frac{\mathrm{M}}{e}\sin\phi\left(\frac{d\,\mathrm{I}}{d\,x}-\frac{d\,\mathrm{R}}{d\,x}\right)$$

$$-\frac{c^2}{2}\mathrm{T}\sin\phi_{,}\cos\theta+\frac{c^2}{2}\mathrm{T}'\sin\phi'\sin\theta'+\frac{\mathrm{M}_{,}}{e_{,}}\frac{d\,\mathrm{T}}{d\,x}\sin\phi_{,}\cos\theta$$

$$-\frac{\mathrm{M}'}{e'}\frac{d\,\mathrm{T}'}{d\,x}\sin\phi'\sin\theta'-\mathrm{D}_{,}\mathrm{T}_x+\frac{\mathrm{F}_{,}}{f}\frac{d\,\mathrm{T}_y}{d\,y}-\mathrm{D}\,\mathrm{R}_x+\frac{\mathrm{F}}{f}\frac{d\,\mathrm{R}_y}{d\,y}$$

$$+\mathrm{Q}_{x_{,}}\,(\alpha_{,}-\alpha).$$

By a precisely similar process we find

$$\frac{d^2\,\beta}{d\,t^2}=-\frac{c^2}{2}\cos\phi\,(\mathrm{I}+\mathrm{R})+\frac{\mathrm{M}}{e}\left(\frac{d\,\mathrm{I}}{d\,x}+\frac{d\,\mathrm{R}}{d\,x}\right)\cos\phi$$

$$-\frac{c^2}{2}\mathrm{T}\cos\phi_{,}\cos\theta+\frac{c^2}{2}\mathrm{T}'\cos\phi'\sin\theta'+\frac{\mathrm{M}_{,}}{e_{,}}\frac{d\,\mathrm{T}}{d\,x}\cos\phi_{,}\cos\theta$$

$$-\frac{\mathrm{M}'}{e'}\frac{d\,\mathrm{T}'}{d\,x}\cos\phi'\sin\theta'+\frac{\mathrm{F}}{f}\frac{d\,\mathrm{R}_{,}}{d\,y}+\frac{\mathrm{F}_{,}}{f}\frac{d\,\mathrm{T}_{,}}{d\,y}-\mathrm{D}\,\mathrm{R}_y-\mathrm{D}_{,}\mathrm{T}_y$$

$$+\frac{\mathrm{F}}{f}\frac{d\,\mathrm{R}_x}{d\,y}+\frac{\mathrm{F}_{,}}{f}\frac{d\,\mathrm{T}_x}{d\,y}+\mathrm{Q}_{y_{,}}(\beta_{,}-\beta)$$

$$\frac{d^2\,\gamma}{d\,t^2}=-\frac{c^2}{2}(\mathrm{I}'-\mathrm{R}')-\frac{c^2}{2}(\mathrm{T}\sin\theta+\mathrm{T}'\cos\theta')$$

$$\frac{\mathrm{M}}{e}\left(\frac{d\,\mathrm{I}'}{d\,x}-\frac{d\,\mathrm{R}'}{d\,x}\right)+\frac{\mathrm{M}_{,}}{e_{,}}\frac{d\,\mathrm{T}}{d\,x}\sin\theta+\frac{\mathrm{M}'}{e'}\frac{d\,\mathrm{T}'}{d\,x}\cos\theta'-\mathrm{D}\,\mathrm{R}_z-\mathrm{D}_{,}\mathrm{T}_z$$

$$+\mathrm{Q}_{z_{,}}(\gamma_{,}-\gamma).$$

Moreover, $\frac{d^2 \alpha_,}{d t^2}$, $\frac{d^2 \beta_,}{d t^2}$, $\frac{d^2 \gamma_,}{d t^2}$ respectively differ from $\frac{d^2 \alpha}{d t^2}$, $\frac{d^2 \beta}{d t^2}$, $\frac{d^2 \gamma}{d t^2}$ only in having $Q_x (\alpha - \alpha_,)$ in place of $Q_{x,} (\alpha_, - \alpha)$, $Q_y (\beta - \beta_,)$ in place of $Q_{y,} (\beta_, - \beta)$, and $Q_z (\gamma - \gamma_,)$ in place of $Q_{z,} (\gamma_, - \gamma)$.

But $\frac{d^2 \alpha}{d t^2} = -c^2 \alpha$, &c. = &c.

Hence the following equations result:

$$-\frac{c^2}{2} \sin \phi (\mathrm{I} - \mathrm{R}) + \frac{\mathrm{M}}{e} \sin \phi \left(\frac{d \mathrm{I}}{d x} - \frac{d \mathrm{R}}{d x}\right) - \frac{c^2}{2} \mathrm{T} \sin \phi_, \cos \theta$$

$$+ \frac{c^2}{2} \mathrm{T}' \sin \phi' \sin \theta' + \frac{\mathrm{M}_,}{e_,} \frac{d \mathrm{T}}{d x} \sin \phi_, \cos \theta - \frac{\mathrm{M}'}{e'} \frac{d \mathrm{T}'}{d x} \sin \phi' \sin \theta'$$

$$- \mathrm{D}_, \mathrm{T}_x - \mathrm{D} \mathrm{R}_x + \frac{\mathrm{F}}{f} \frac{d \mathrm{R}_y}{d x} + \frac{\mathrm{F}_,}{f} \frac{d \mathrm{T}_y}{d y} + Q_{x,} (\alpha_, - \alpha)$$

$$= -c^2 (\mathrm{I} - \mathrm{R}) \sin \phi - c^2 \mathrm{R}_x \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1.)$$

$$= -c^2 \mathrm{T} \sin \phi_, \cos \theta + c^2 \mathrm{T}' \sin \phi' \sin \theta' - c^2 \mathrm{T}_x + \overline{Q_{x,} + Q_x} (\alpha_, - \alpha) \qquad (2.)$$

By subtracting these equations, we get

$$-c^2 \alpha + c^2 \alpha_, - (Q_{x,} + Q_x)(\alpha_, - \alpha) = 0$$

or

$$(c^2 - Q_{x,} - Q_x)(\alpha_, - \alpha) = 0$$

wherefore $\alpha_, - \alpha = 0$ . . . . (1.)

Similarly $\beta_, - \beta = 0$ . . . . (2.)

$\gamma_, - \gamma = 0$ . . . . (3.)

And by adding the above equations, and striking out the parts which are common to both sides, we obtain

$$\frac{\mathrm{M}}{e}\left(\frac{d \mathrm{I}}{d x} - \frac{d \mathrm{R}}{d x}\right) \sin \phi + \frac{\mathrm{M}_,}{e_,} \frac{d \mathrm{T}}{d x} \sin \phi_, \cos \theta - \frac{\mathrm{M}'}{e'} \frac{d \mathrm{T}}{d x} \sin \phi' \sin \theta'$$

$$+ \left(\frac{c^2}{2} - \mathrm{D}\right) \mathrm{R}_x + \left(\frac{c^2}{2} - \mathrm{D}_,\right) \mathrm{T}_x + \frac{\mathrm{F}}{f} \frac{d \mathrm{R}_y}{d y} + \frac{\mathrm{F}_,}{f} \frac{d \mathrm{T}_y}{d y} = 0 \quad . \quad . \quad . \quad . \quad (4.)$$

$$\frac{\mathrm{M}}{e}\left(\frac{d \mathrm{I}}{d x} + \frac{d \mathrm{R}}{d x}\right) \cos \phi + \frac{\mathrm{M}_,}{e_,} \frac{d \mathrm{T}}{d x} \cos \phi_, \cos \theta - \frac{\mathrm{M}'}{e'} \frac{d \mathrm{T}'}{d x} \cos \phi' \sin \theta'$$

$$+ \frac{\mathrm{F}}{f} \frac{d \mathrm{R}_x}{d y} + \frac{\mathrm{F}_,}{f} \frac{d \mathrm{T}_x}{d y} + \left(\frac{c^2}{2} - \mathrm{D}\right) \mathrm{R}_y + \left(\frac{c^2}{2} - \mathrm{D}_,\right) \mathrm{T}_y = 0 \quad . \quad . \quad . \quad (5.)$$

$$\frac{\mathrm{M}}{e}\left(\frac{d \mathrm{I}'}{d x} - \frac{d \mathrm{R}'}{d x}\right) + \frac{\mathrm{M}_,}{e_,} \frac{d \mathrm{T}}{d x} \sin \theta + \frac{\mathrm{M}'}{e'} \frac{d \mathrm{T}'}{d x} \cos \theta'$$

$$+ \left(\frac{c^2}{2} - \mathrm{D}\right) \mathrm{R}_z + \left(\frac{c^2}{2} - \mathrm{D}_,\right) \mathrm{T}_z = 0 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6.)$$

These equations differ from those which I gave in my Memoir on light reflected at the surface of a crystal, in having the terms R_y, R_z, T_y, T_z additional. Thus it

appears that the equations do not depend on the *law of force*, but are equally true in all cases.

Before I proceed with any further discussion of these equations, I desire to prove some important theorems relative to the values of the constants in symmetrical media.

THEOREM 1.—*Relation between the sums of powers of one co-ordinate, and products of powers of different ones.*

Let f, g, h be the co-ordinates of a particle; $x-f$, $y-g$, $z-h$ those of another measured from it. Then

$$\Sigma\, m\, \phi\, r = \Sigma\, m\, \phi \sqrt{(x-f)^2+(y-g)^2+(z-h)^2}$$

Suppose the particle whose co-ordinates are f, g, h, to be moved to a point $f+\alpha,\ g+\beta,\ h+\gamma$, then $\Sigma\, m\, \phi\, r$, becomes $\Sigma\, m\, \phi\sqrt{(x-f-\alpha)^2+(y-g-\beta)^2+(z-h-\gamma)^2}$.

Also
$$(x-f-\alpha)^2+(y-g-\beta)^2-(z-h-\gamma)^2=r^2$$
$$-2\{(x-f)\alpha+(y-g)\beta+(z-h)\gamma\}+\alpha^2+\beta^2+\gamma^2.$$

Let δ be the distance through which the particle is moved, and let $(x-f)\alpha+(y-g)\beta+(z-h)\gamma$ be denoted by ϵ; then

$$\Sigma\, m\, \phi\, r_{,} = \Sigma\, m\, \phi \sqrt{r^2-2\epsilon+\delta^2}$$
$$=\Sigma\, m\, \phi\, r + \ .\ .\ .\qquad + \Sigma\, m\, f\, r \left(\frac{2\,\epsilon-\delta^2}{r^2}\right)^{2\,n} + \ .\ .\ .$$

Now this must be a function of δ: consequently

$$\Sigma\, m f_{,} r\, \epsilon^{2n} \text{ must equal } \mathrm{P}\, \delta^{2n}$$

or
$$\Sigma\, m f_{,} r \{\alpha\,(x-f)+\beta\,(y-g)+\gamma\,(z-h)\}^{2n} = \mathrm{P}\,(\alpha^2+\beta^2+\gamma^2)^n$$

P being some function of r.

By expanding each side we get

$$\Sigma\, m f_{,} r \Big\{ \alpha^{2n}(x-f)^{2n} + \frac{2n\,(2n-1)}{1\,.\,2}\,\overline{\beta\,(y-g)+\gamma\,(z-h)}^{\,2}\ \alpha^{2n-2}\,(x-f)^{2n-2} + \text{\&c.}$$
$$+\ \frac{2n\,(2n-1)\ldots(2n-2r+1)}{1\,.\,2\,.\,.\,.\,.\,.\,.\,.\,.\,2r}\,\overline{\beta\,(y-g)+\gamma\,(z-h)}^{\,2r}\ \alpha^{2n-2r}\,(x-f)^{2n-2r} + \text{\&c.}\Big\}$$
$$=\mathrm{P}\Big\{\alpha^{2n}+n\,(\beta^2+\gamma^2)\,\alpha^{2n-2}+\ .\ .\ .\ +\frac{n\,(n-1)\ldots(n-r+1)}{1\,.\,2\,.\,.\,.\,.\,.\,.\,.\,r}\,(\beta^2+\gamma^2)^r\,\alpha^{2n-2r}+\text{\&c.}\Big\}$$

Hence we obtain

$$\Sigma\, m f_{,} r\,(x-f)^{2n} = \mathrm{P}$$

$$\Sigma\, m f_{,} r\,(y-g)^2\,(x-f)^{2n-} = \frac{n\,\mathrm{P}}{\dfrac{2n\,(2n-1)}{1\,.\,2}}\quad .\ .\ .\ .\ (\mathrm{A}\ (1))$$

$$\Sigma m f,r\,(x-f)^{2n-2r}(y-g)^{2r-2s}(z-h)^{2s}=\frac{\dfrac{n\,(n-1)\ldots(n-r+1)}{1\,.\,2\ldots\ldots r}\cdot\dfrac{r\,(r-1)\ldots(r-s+1)}{1\,.\,2\ldots\ldots s}}{\dfrac{2n\,(2n-1)\,.\,(2n-2r+1)}{1\,.\,2\ldots\ldots\ldots 2r}\cdot\dfrac{2r\,(2r-1)\,.\,(2r-2s+1)}{1\,.\,2\ldots 2s}}\,\mathrm{P}$$

$$=\frac{(n-r+1)\ldots(2n-2r)\times(r-s+1)\ldots(2r-2s)\,(s+1)\ldots 2s}{(n+1)\ldots 2n}\,\mathrm{P}. \quad (2)$$

THEOREM 2.—*To find the relation between* $\Sigma m f,r\,(x-f)^{2n}$ and $\Sigma\,m\,f,r\,r^{2n}$

This amounts to the determination of the value of P in the above expression.

Let θ be the angle between r and δ

then $\quad \epsilon = r\,\delta\cos\theta$

and $\quad \epsilon^{2n} = r^{2n}\,\delta^{2n}\cos^{2n}\theta$

$\therefore \quad \mathrm{P} = \Sigma m r^{2n}\cos^{2n}\theta f,r$

Now, the area of a spherical surface in the mass is $\iint r^2\sin\theta\, d\theta\, d\phi$
Hence for such a surface

$$\mathrm{P} = \iint r^{2n+2}\cos^{2n}\theta\sin\theta\, d\theta\, d\phi\, f,r$$

$$= -\int \frac{r^{2n+2} f,r\cos^{2n+1}\theta}{2n+1}\, d\phi$$

$$= \int \frac{r^{2n+2} f,r\, d\phi}{2n+1}$$

$$= \frac{1}{2n+1}\,\Sigma\, m r^{2n} f,r$$

or $$\Sigma\, m\,(x-f)^{2n} f,r = \frac{1}{2n+1}\Sigma m r^{2n} f,r \quad \ldots\ldots\ldots \quad (\mathrm{B})$$

This proposition might have been proved with little difficulty without having recourse to integrals, but the result is so obvious, that I do not think it necessary to add such a proof.

THEOREM 3.—*A system of particles act on one another by forces which vary inversely as the square of the distance between them. One of the particles is removed from its position of equilibrium, to find the force put in play on it.*

Let the co-ordinates of this particle be measured in such directions that the axis of x may be the line of motion.

Now, it is evident that, since the medium is one of symmetry, the force put in play is in the direction of the motion; and is represented by

$$\Sigma\, m \frac{x-f-a}{\{(x-f-a)^2+(y-g)^2+(z-h)^2\}^{\frac{3}{2}}}$$

$$= \Sigma\, m \frac{x-f-a}{\{r^2-2(x-f)\,a+a^2\}^{\frac{3}{2}}}$$

$$= \Sigma\, m \frac{(x-f-a)}{r^3}\left\{1+\frac{3}{2}\left(\frac{2\,\overline{x-f}\,a-a^2}{r^2}\right)+\frac{3\,.\,5}{2\,.\,4}\left(\frac{2\,(x-f)\,a-a^2}{r^2}\right)^2\right.$$

$$\left. + \&c. \quad + \frac{3\,.\,5\ldots(2n+1)}{2\,.\,4\ldots\;2n}\left(\frac{2\,(x-f)\,a-a^2}{r^2}\right)^n + \quad \&c.\right\}$$

The co-efficient of a^{2n} in this expression is as follows:

$$\pm\,\Sigma\, m\left\{\frac{3\,.\,5\ldots(2n+1)}{2\,.\,4\ldots.\;2n}\,\frac{n}{r^{2n+3}}\,2\,(x-f)-\frac{3\,.\,5\ldots(2n+3)}{2\,.\,4\ldots(2n+2)}\,\frac{\frac{(n+1)\,n\,(n-1)}{1\,.\,2\,.\,3}}{r^{2n+5}}\,2^3\,(x-f)^3\right.$$

$$\left. + \&c.\right\}$$

every term of which involves $(x-f)$ as a factor. Hence this quantity is zero. Again, the co-efficient of a^{2n+1} is

$$\pm\,\Sigma\, m\left\{\frac{3\,.\,5\ldots(2n+1)}{2\,.\,4\ldots.\;2n}\,\frac{1}{r^{2n+3}}-\frac{3\,.\,5\ldots(2n+3)}{2\,.\,4\ldots(2n+2)}\,\frac{(n+1)\,n}{1\,.\,2}\,\frac{2^2\,(x+f)^2}{r^{2n+5}}\right.$$

$$+\frac{3\,.\,5\ldots(2n+5)}{2\,.\,4\ldots(2n+4)}\,\frac{(n+2)\,(n+1)\,n\,(n-1)}{1\,.\,2\,.\,3\,.\,4}\,\frac{2^4\,(x-f)^4}{r^{2n+7}}-\quad\&c.$$

$$-\frac{3\,.\,5\ldots(2n+3)}{2\,.\,4\ldots(2n+2)}\,(n+1)\,\frac{2\,(x-f)^2}{r^{2n+5}}+\frac{3\,.\,5\ldots(2n+5)}{2\,.\,5\ldots(2n+4)}\,\frac{(n+2)\,(n+1)\,n}{1\,.\,2\,.\,3}\,\frac{2^3\,(x-f)^4}{r^{2n+7}}$$

$$\left.-\frac{3\,.\,5\ldots(2n+7)}{2\,.\,4\ldots(2n+6)}\,\frac{(n+3)\,(n+2)\,(n+1)\,n\,(n-1)}{1\,.\,2\,.\,3\,.\,4\,.\,5}\,\frac{2^5\,(x-f)^6}{r^{2n+9}}+\quad\&c.\right\}$$

It remains that we find the sum of this series. To effect this, we remark that the co-efficients can all be reduced by the result of Theorem 2. Applying this theorem, the co-efficient is reduced to

$$\pm\,\Sigma\,\frac{m}{r^{2n+3}}\left\{\frac{3\,.\,5\ldots(2n+1)}{2\,.\,4\ldots..\;2n}-\frac{3\,.\,5\ldots(2n+3)}{2\,.\,4\ldots(2n+2)}\,\frac{(n+1)\,n}{1\,.\,2}\,\frac{2^2}{3}\right.$$

$$+\frac{3\,.\,5\ldots(2n+5)}{2\,.\,4\ldots(2n+4)}\,\frac{(n+2)\,(n+1)\,n\,(n-1)}{1\,.\,2\,.\,3\,.\,4}\,\frac{2^4}{5}-\quad\&c.$$

$$\left.-\frac{3\,.\,5\ldots(2n+3)}{2\,.\,4\ldots(2n+2)}\,(n+1)\,\frac{2}{3}+\frac{3\,.\,5\ldots(2n+5)}{2\,.\,4\ldots(2n+4)}\,\frac{(n+2)\,(n+1)\,n}{1\,.\,2\,.\,3}\,\frac{2^3}{5}-\quad\&c.\right\}$$

Now, we observe that the co-efficient of a^{2n+1} in

$$\frac{p-a}{(1-2\,p\,a+a^2)^{\frac{3}{2}}}\ \text{is}\ \pm\left\{\frac{3\,.\,5\ldots(2n+1)}{2\,.\,4\ldots..\;2n}-\frac{3\,.\,5\ldots(2n+3)}{2\,.\,4\ldots(2n+2)}\,\frac{(n+1)\,n}{1\,.\,2}\,.\,2^2\,p^2\right.$$

$$+ \quad \&c. \quad - \frac{3 \,.\, 5 \ldots (2n+3)}{2 \,.\, 4 \ldots (2n+2)} (n+1)\, 2 p^2 + \frac{3 \,.\, 5 \ldots (2n+5)}{2 \,.\, 4 \ldots (2n+4)} \frac{(n+2)(n+1)\, n}{1 \,.\, 2 \,.\, 3} 2^3 p^4$$

$$- \quad \&c. \Big\}$$

Hence the part of the required co-efficient which lies within brackets is evidently the same as that of a^{2n+1} in $\int_0^1 \frac{p-a}{(1-2\,p\,a+a^2)^{\frac{3}{2}}} d\,p.$

But $$\int \frac{p-a}{(1-2\,p\,a+a^2)^{\frac{3}{2}}} d\,p = \frac{\sqrt{1-2\,a\,p+a^2}}{2\,a^2} + \frac{1-a^2}{2\,a^2} \frac{1}{\sqrt{1-2\,p\,a+a^2}}$$

$\therefore$ The above co-efficient is the same thing as the co-efficient of a^{2n+1} in the expansion of

$$\pm \Sigma \frac{m}{r^{2n+3}} \left\{ \frac{1-a}{2\,a^2} + \frac{1-a^2}{2\,a^2 (1-a)} - \frac{\sqrt{1+a^2}}{2\,a^2} - \frac{1-a^2}{2\,a^2} \frac{1}{\sqrt{(1+a^2)}} \right\}$$

or of $$\pm \Sigma \frac{m}{r^{2n+3}} \left\{ \frac{1}{a^2} - \frac{1}{a^2 \sqrt{1+a^2}} \right\}.$$

But this co-efficient is evidently zero. Hence, every term in the expression of

$$\Sigma\, m \frac{z-f-a}{\{(z-f-a)^2+(y-g)^2+(z-h)^2\}^{\frac{3}{2}}} \text{ is } zero.$$

We have consequently proved, that a particle of a system exerting forces which vary inversely as the square of the distance, will not tend to move *at all* by the action of the other particles of the system on it when out of its position of equilibrium.

Let us next proceed to find the values of c^2, M, &c. For the first of them, we will take the sum *throughout* an unform medium, whereby its value will become

$$c^2 = \Sigma\, m\, (\phi\, r + \frac{\phi'\, r}{r}\, \delta\, y^2)\, 2 \sin^2 \frac{k\, \delta\, z}{2} \quad (1)$$

where $\delta\, x$, $\delta\, y$, $\delta\, z$ are the co-ordinates of any particle m, measured from that under consideration, r the distance between the two, and $r\, \phi\, r$ the law of force.

Now, $$2 \sin^2 \frac{k\, \delta\, z}{2} = 1 - \cos k\, \delta\, z = 1 - (1 - \frac{(k\, \delta\, z)^2}{1 \,.\, 2} + \&c.)$$

$$\therefore \quad c^2 = \Sigma\, m\, (\phi\, r + \frac{\phi'\, r}{r}\, \delta\, y^2) - \Sigma\, m\, (\phi\, r + \frac{\phi'\, r}{r}\, \delta\, y^2)\, (1 - \frac{(k\, \delta\, x)^2}{1 \,.\, 2} + \&c.)$$

This expression can be reduced by means of the Theorems just obtained.

Equation A (1) gives $\Sigma\, m\, \delta\, x^2\, \delta\, y^{2n-2} \frac{\phi'\, r}{r} = \frac{1}{2n-1} \Sigma\, m\, \delta\, x^{2n} \frac{\phi'\, r}{r}$

which, by applying equation (B), is reduced to

$$\Sigma\, m\, \delta\, x^2\, \delta\, y^{2n-2} \frac{\phi'\, r}{r} = \frac{1}{(2n-1)(2n+1)} \Sigma\, m\, r^{2n} \frac{\phi'\, r}{r}$$

The same equations give likewise,

$$\Sigma\, m \frac{\phi' r}{r}\, \delta y^2 = \frac{1}{3}\, \Sigma\, m \frac{\phi' r}{r}\, r^2$$

$$\Sigma\, m\, \phi\, r\, x^{2n} = \frac{1}{2_{n+1}}\ \Sigma\, m\, \phi\, r\, r^{2n} \text{ \&c. \&c.}$$

By substituting these results in the value of c^2 it becomes

$$c^2 = \Sigma\, m \left(\phi\, r + \frac{\phi' r}{r} \frac{r^2}{3} \right)$$

$$- \Sigma\, m\, \phi\, r \left(1 - \frac{k^2\, r^2}{3\, 1.2} + \frac{k^4\, r^4}{5\, 1.2.3.4} - \text{\&c.} \right)$$

$$- \Sigma\, m \frac{\phi' r}{r} \left(\frac{r^2}{3} - \frac{k^2\, r^4}{3.5\, 1.2} + \frac{k^4\, r^6}{5.7\, 1.2.3.4} - \text{\&c.} \right)$$

Of this expression the first line is not dependent on expansion, the second and third are. It is desirable to keep them distinct.

To sum the series which multiply $\phi\, r$ and $\frac{\phi' r}{r}$ respectively under the symbol $\Sigma\, m$.

1. $$1 - \frac{k^2\, r^2}{3\, 1.2} + \frac{k^4\, r^4}{5\, 1.2.3} - \text{\&c.} = \frac{1}{k\, r} \left\{ k\, r - \frac{k^3\, r^3}{1.2.3} + \text{\&c.} \right\}$$

$$= \frac{\sin k\, r}{k\, r}$$

which is the multiplier of $\phi\, r$.

2. Let $\frac{r^2}{3} - \frac{k^2\, r^4}{3.5.1.2} + \frac{k^4\, r^6}{5.7\, 1.2.3.4} - \text{\&c.} = u$;

then $$\int_0 \frac{u}{r}\, d\, r = \frac{r^2}{1.2.3} - \frac{k^2\, r^4}{1..5} + \frac{k^4\, r^6}{1...7} - \text{\&c.}$$

$$= \frac{1}{k^3\, r} \left(\frac{k^3\, r^3}{1.2.3} - \frac{k^5\, r^5}{1...5} + \text{\&c.} \right)$$

$$= \frac{k\, r - \sin k\, r}{k^3\, r}$$

$$\therefore\quad u = -\frac{r}{k^3} \frac{d}{d\, r} \cdot \frac{\sin k\, r}{r}$$

$$= \frac{1}{k^3} \left(\frac{\sin k\, r}{r} - k \cos k\, r \right)$$

which is the co-efficient of $\frac{\phi' r}{r}$.

By substituting these values in the expression for c^2 it is reduced to

$$c^2 = \Sigma\, m \left(\phi\, r + \frac{\phi' r}{r} \frac{r^2}{3} \right)$$

$$- \Sigma\, m\, \phi\, r \frac{\sin k\, r}{k\, r} - \Sigma\, m \frac{\phi' r}{r} \left(\frac{\sin k\, r}{k^3\, r} - \frac{\cos k\, r}{k^2} \right) \quad (a).$$

This is a remarkably simple expression for c^2.

By combining the two portions, it may be written thus:

$$c^2=\Sigma\frac{m}{r^2}\left\{\phi r\left(r^2-\frac{r\sin kr}{k}\right)+\phi' r\left(\frac{r^3}{3}-\frac{\sin kr}{k^3}+\frac{r\cos kr}{k^2}\right)\right\}$$

$$=\Sigma\frac{m}{r^2}\frac{d}{dr}\left\{\phi r\left(\frac{r^3}{3}-\frac{\sin kr}{k^3}+\frac{r\cos kr}{k^2}\right)\right\}.$$

If we write $f(r)$ for $r\,\phi\,(r)$, the force at distance r, we get

$$c^2=\Sigma\frac{m}{k^2 r^2}\frac{d}{dr}\left\{\left(\frac{k^2 r^2}{3}-\frac{\sin kr}{kr}+\cos kr\right)fr\right\} \qquad (b),$$

which is identical with M. Cauchy's equation (15), *Nouveaux Exercises* (Prague), p. 187, and leads immediately to his equation (28), *Exercises d'Analyse*, &c., p. 299. The facility with which I have deduced this equation, is a proof of the utility of the method of proceeding which I adopted in the Memoirs from which the original value of c^2 is extracted.

It is worthy of remark, that, by converting sums into integrals *at once*, we obtain the same result as by the process we have followed. We shall prove this as follows:

Let r be the distance of a particle from the origin, θ the angle between r and the axis of x, ϕ the angle between the planes of x and r and x and z: then the mass of an element is $\varrho\, r^2 \sin\theta\, d\theta\, d\phi\, dr$

$$\therefore\ \Sigma m\,\phi r\cos k\,\delta x=\varrho\iiint dr\,d\theta\,d\phi\,r^2\,\phi r\sin\theta\cos k\,\delta x$$

But $\qquad \delta x=r\cos\theta,\ \delta y=r\sin\theta\sin\phi$

$$\therefore\ \Sigma m\,\phi r\cos kx=2\pi\varrho\iint_0^\pi dr\,d\theta\,r^2\,\phi r\sin\theta\cos(kr\cos\theta)$$

$$=2\pi\varrho\int dr\,r\,\phi r\left(-\frac{\sin kr\cos\theta}{k}+C\right)$$

$$=4\pi\varrho\int dr\,r\,\phi r\,\frac{\sin kr}{k}$$

and

$$\Sigma m\frac{\phi' r}{r}\,\delta y^2=\iint_0^\pi\int_0^{2\pi} dr\,d\theta\,d\phi\frac{\phi' r}{r}\,r^4\sin^3\theta\sin^2\phi$$

$$=\pi\varrho\iint_0^\pi dr\,d\theta\,\phi' r\,.\,r^3\sin^3\theta$$

$$=\pi\varrho\int dr\left(-\cos\theta+\frac{\cos^3\theta}{3}+C\right)\phi' r\,r^3$$

$$=\frac{4\pi\varrho}{3}\int.\,dr\,\phi' r\,r^3$$

$$\Sigma m\frac{\phi' r}{r}\,\delta y^2\cos(k\,\delta x)=\pi\varrho\iint_0^\pi dr\,d\theta\,\phi' r\,r^3\sin^3\theta\cos(kr\cos\theta)$$

$$=\pi\varrho\iint_{-kr}^{kr}\frac{r^3\,\phi' r\,dr\,dv}{kr}\left(1-\frac{v^2}{k^2r^2}\right)\cos v$$

Now $$\int v^2 \cos v\, d\,v = v^2 \sin v + 2\, v \cos v - 2 \sin v$$

$$\therefore \quad \Sigma\, m\, \frac{\phi' r}{r} \delta\, y^2 \cos (k\, \delta\, x) = 4\, \pi\, \rho \int \frac{r^2\, \phi' r\, dr}{k} \left(- \frac{\cos k\, r}{k\, r} + \frac{\sin k\, r}{k^2\, r^2} \right)$$

Substituting all these results in equation (1), it gives

$$c^2 = 4\, \pi\, \rho \int_o^\infty d\, r \left\{ r^2 \phi\, r + \frac{\phi' r\, r^3}{3} - r\, \phi\, r \frac{\sin k\, r}{k} + r\, \phi' r \frac{\cos k\, r}{k^2} - \phi' r \frac{\sin k\, r}{k^3} \right\} \quad (c).$$

This is evidently the same as equation (a).

Finally let us sum this expression for r.

If we integrate, the result is

$$c^2 = \frac{4\, \pi\, \rho}{k^2} \left\{ \frac{k^2\, r^2}{3} - \frac{\sin k\, r}{k\, r} + \cos k\, r \right\} f\, r$$

to be taken between the limits $r = o$, $r = \infty$. For the latter limit the expression *generally* vanishes. Hence

$$c^2 = - \frac{4\, \pi\, \rho}{k^2} \left\{ \frac{k^4\, r^4}{1.2.3.4} - \frac{k^4\, r^4}{1.2.3.4.5} \right\} f\, r_{r=o}$$

$$= - \frac{2\, \pi\, \rho}{15} k^2 (r^4 f\, r)_{r=o}$$

For one value of $f\, r$ this process fails, viz. when $f\, r = r^2$. In this case it is evident that the above expression does not vanish when $r = \infty$. The reason why this is so, is that $r^2 f r$ is a finite quantity. Now, if we return to the original expression for c^2, we shall find that it is the representation of the *difference* between two terms, which in the operation have assumed different forms. To obtain the correct result, I would suggest that the terms retain both the same form, which they will do, if we write $\cos a\, \delta\, x$ instead of 1, and, finally, put $a = 0$. We shall then have,

$$c^2 = \Sigma\, m \left(\phi\, r + \frac{\phi' r}{r} \delta\, y^2 \right) (\cos a\, \delta\, x - \cos k\, \delta\, x).$$

which, when the law of force is that of the inverse square of the distance becomes,

$$c^2 = \Sigma \frac{m}{r^3} \left\{ \frac{\sin a\, r}{a\, r} - \frac{3 \sin a\, r}{a^3\, r^3} + \frac{3 \cos a\, r}{a^2\, r^2} \right.$$
$$\left. - \left(\frac{\sin k\, r}{k\, r} - \frac{3 \sin k\, r}{k^3\, r^3} + \frac{3 \cos k\, r}{k^2\, r^2} \right) \right\} \quad (d).$$

If we now substitute integrals in place of sums, we obtain

$$c^2 = 4\, \pi\, \rho \int_o^\infty d\, r \left(\frac{\sin a\, r}{a\, r^2} - \frac{3 \sin a\, r}{a^3\, r^4} + \frac{3 \cos a\, r}{a^2\, r^3} \right.$$
$$\left. - \frac{\sin k\, r}{k\, r^2} + \frac{3 \sin k\, r}{k^3\, r^4} - \frac{3 \cos k\, r}{k^2\, r^3} \right)$$

$$=4\pi\rho\left\{\left(-\frac{\cos a r}{a^2 r^2}+\frac{\sin a r}{a^3 r^3}+\frac{\cos k r}{k^2 r^2}-\frac{\sin k r}{k^3 r^3}\right)_{r=\infty}^{a=o}\right.$$

$$\left.\left(-\frac{\cos a r}{a^2 r^2}+\frac{\sin a r}{a^3 r^3}+\frac{\cos k r}{k^2 r^2}-\frac{\sin k r}{k^2 r^3}\right)_{r=o}^{a=o}\right\}$$

$$=0$$

Thus, that portion of the value of c^2, which we obtain by substituting integrals in place of sums, is zero. This conclusion I arrived at previously, and it appears to be, in every way, conformable with the nature of the function.

Let us now recur to equation (d), and endeavour to obtain the approximate form of c^2 by *summation*. We will suppose (as an approximation merely) that the particles may be regarded as aggregated in spherical surfaces about the molecule under consideration. Let ϵ be the distance between two consecutive particles, $r=n\,\epsilon$, where n is a number; then $4\pi n^2$ is the number of particles in a spherical surface, of which the radius is r. Putting, therefore, instead of $\Sigma\, m$, $S\, 4\pi m\, n^2$, where S refers to the number n: we get

$$c^2=4\pi m\, S\,\frac{1}{n\,\epsilon^3}\left\{\frac{\sin a n \epsilon}{a n \epsilon}-\frac{3\sin a n\epsilon}{a^3 n^3 \epsilon^3}+\frac{3\cos a n \epsilon}{a^2 n^2 \epsilon^2}\right.$$

$$\left.-\left(\frac{\sin k n \epsilon}{k n e}-\frac{3\sin k n\epsilon}{k^3 n^3 \epsilon^3}+\frac{3\cos k n\epsilon}{k^2 n^2 \epsilon^2}\right)\right\}$$

$$=\frac{4\pi m}{\epsilon^3}\, S\left\{\frac{1}{\beta}\left(\frac{\sin\beta n}{n^2}+\frac{3\sin\beta n}{\beta^2 n^4}+\frac{3\cos\beta n}{\beta n^3}\right)\right.$$

$$\left.-\frac{1}{a}\left(\frac{\sin a n}{n^2}-\frac{3\sin a n}{a^2 n^4}+\frac{3\cos a n}{a n^3}\right)\right\}$$

where β is written for $a\,\epsilon$, and a for $k\,\epsilon$.

$$\therefore\quad c^2=\frac{4\pi m}{\epsilon^3}\, S\left\{\beta\frac{d}{d\beta}\left(\frac{\sin\beta n}{\beta^3 n^4}-\frac{\cos\beta n}{\beta^2 n^3}\right)-a\frac{d}{d a}\left(\frac{\sin a n}{a^3 n^4}-\frac{\cos a n}{a^2 n^3}\right)\right\}$$

$$=\frac{4\pi m}{\epsilon^3}\, S\left\{\beta\frac{d}{d\beta}\,\frac{1}{\beta}\,\frac{d}{d\beta}\,\frac{-\sin\beta n}{\beta n^4}-a\frac{d}{d a}\,\frac{1}{a}\,\frac{d}{d a}\,\frac{-\sin a n}{a n^4}\right\}$$

$$=\frac{4\pi m}{\epsilon^3}\left\{a\frac{d}{d a}\left(\frac{1}{a}\frac{d}{d a}\left(\frac{1}{a}\, S\frac{\sin a n}{n^4}\right)\right)-\beta\frac{d}{d\beta}\left(\frac{1}{\beta}\frac{d}{d\beta}\right)\frac{1}{\beta}\, S\frac{\sin\beta n}{n^4}\right)\right)\right\}$$

The value of this expression depends on the summation of the series $S\dfrac{\sin a n}{n^4}$ from $n=1$, to $n=\infty$. There is no known process, so far as I am aware, by which this summation can be effected. We can, however, obtain that part of the series which is required for our present purpose by the following process.

$$\sin a+\sin 2a+\sin 3a+\&c.=\frac{1}{2}\cot\frac{a}{2}$$

$$=\frac{1}{a}-\frac{a}{12}+\&c.$$

$$\therefore\quad -\frac{\cos a}{1}-\frac{\cos 2a}{2}-\frac{\cos 3a}{3}-\&c.=\log a-\frac{a^2}{24}+\&c.+C.$$

by integration.

and $$-\frac{\sin a}{1^2}-\frac{\sin 2a}{2^2}-\frac{\sin 3a}{3^2}-\text{\&c.}=a\log a-a+\mathrm{C}\,a-\frac{a^3}{72}+\text{\&c.}$$

which vanishes when $a=0$.

$$\frac{\cos a}{1^3}+\frac{\cos 2a}{2^3}+\frac{\cos 3a}{3^3}+\text{\&c.}=\frac{a^2}{2}\log a-\frac{a^2}{4}-\frac{a^2}{2}+\mathrm{C}\frac{a^2}{2}+\mathrm{D}-\frac{a^4}{288}+\text{\&c.}$$

Finally, $$\frac{\sin a}{1^4}+\frac{\sin 2a}{2^4}+\frac{\sin 3a}{3^4}+\text{\&c.}=\frac{a^3}{6}\log a+\mathrm{A}\,a^3+\mathrm{D}\,a-\frac{a^5}{1440}+\text{\&c.}$$

where A is a constant.

$$\therefore\quad \frac{1}{a}\,S\,\frac{\sin an}{n^4}=\frac{a^2}{6}\log a+\mathrm{A}\,a^2+\mathrm{D}-\frac{a^4}{1440}+\text{\&c.}$$

$$\frac{d}{da}\left(\frac{1}{a}\,S\,\frac{\sin an}{n^4}\right)=\frac{a}{6}+\frac{a}{3}\log a+2\,\mathrm{A}\,a-\frac{a^3}{360}+\text{\&c.}$$

$$\frac{d}{da}\left(\frac{1}{a}\frac{d}{da}\left(\frac{1}{a}\,S\,\frac{\sin an}{n^4}\right)\right)=\frac{1}{3a}-\frac{a}{160}+\text{\&c.}$$

By substitution, we obtain,

$$c^2=\frac{4\pi m}{\epsilon^3}\left\{\frac{1}{3}-\frac{a^2}{180}-\text{\&c.}-\left(\frac{1}{3}-\frac{\beta^2}{180}+\text{\&c.}\right)\right\}$$

$$=\frac{4\pi m}{\epsilon^3}\left\{\frac{a^2}{180}-\text{\&c.}\right\},\ \text{for } \beta=0.$$

Hence in value, $$c^2=-\frac{4\pi m}{\epsilon}\,\frac{k^2}{180}.$$

and $$v^2=-\frac{4\pi m}{180\,\epsilon}=-\frac{\pi m}{45\,\epsilon}.$$

This is precisely of the *form* which experiment requires. We learn from the result, too, that m is negative, that is, that the force is *repulsive*.

It will be remarked that whilst approximation, on the hypothesis that the principal effect is due to the particles in the immediate vicinity of that under consideration, gives the same *order* to the first term in c^2 as the process of summation does, it gives it a different *sign*.

This will appear by expanding either equation (1) or equation (a) in terms of k.

From the former $$c^2=2\,\Sigma\,m\left(\frac{1}{r^3}-\frac{3\,\delta y^3}{r^5}\right)\sin^2\frac{k\,\delta x}{2}$$

$$=2\,\Sigma\,m\left(\frac{1}{r^3}-\frac{3\,\delta y^2}{r^5}\right)\frac{k^2\,\delta x^2}{4}$$

$$=\frac{1}{2}\,\Sigma\,m\left(\frac{1}{3r}-\frac{3}{3\,.\,5\,r}\right)k^2$$

$$=\frac{k^2}{15}\,\Sigma\,\frac{m}{r}$$

From the latter $c^2 = \Sigma \left(-\frac{\sin k r}{k r^4}+\frac{3}{r^5}\left(\frac{\sin k r}{k^3 r}-\frac{\cos k r}{k^2}\right)\right)$

$$= \Sigma\, m \left\{\frac{k^2}{6 r}+\frac{3 k^2}{r}\left(\frac{1}{120}-\frac{1}{24}\right)\right\}$$

$$= \Sigma\, \frac{m}{r} k^2 \left(\frac{1}{6}-\frac{1}{10}\right)$$

$$= \frac{k^2}{15}\, \Sigma\, \frac{m}{r}.$$

It is clear that an approximation of this kind is illusory, inasmuch as $\frac{\Sigma\, m}{r}$ is infinite. We are not justified, therefore, in arguing any thing relative to the sign of m from this process.

To find the value of M.

$$M = \Sigma\, m \left(\phi\, r+\frac{\phi' r}{r}\,\delta\, x^2\right) \sin e\,\delta\, x \cos f\,\delta\, y+\frac{\cos \phi}{\sin \phi}\, \Sigma\, m\, \frac{\phi' r}{r}\,\delta\, x\,\delta\, y \cos e\,\delta\, x \sin f\,\delta\, y$$

taken for *half* the medium.

Now $\quad m=\varrho\, r^2 \sin \theta\, d\,\theta\, d\,\phi\, d\,r,\ \delta\, x=r \cos \theta,\ \delta\, y=r \sin \theta \sin \phi.$

Instead of $\cos f\,\delta\, y$ write 1, and instead of $\cos e\,\delta\, x$ write 1, for the other parts of these expressions respectively are very small compared with these, as we have just proved.

Now $\quad \Sigma\, m\, \phi\, r \sin e\,\delta\, x=\varrho \iiint r^2\, \phi\, r \sin \theta \sin (\varrho\, r \cos \theta)\, d\,r\, d\,\phi\, d\,\theta$

$$= \varrho \iint r^2\, \phi\, r\, \frac{1-\cos e\,r}{e\,r}\, d\,r\, d\,\phi$$

$$= 2\,\pi\,\varrho \int r^2\, \phi\, r\, \frac{1-\cos e\,r}{e\,r}\, d\,r$$

$$= \pi\,\varrho \int e\, r^3\, \phi\, r\, d\,r$$

$$\Sigma\, m\, \frac{\phi' r}{r}\,\delta\, x^2 \sin e\,\delta\, x= \iiint \varrho\, r^2\, \frac{\phi' r}{r}\, r^2 \cos^2 \theta \sin \theta \sin (e\, r \cos \theta)\, d\,r\, d\,\phi\, d\,\theta$$

$$= 2\,\pi\,\varrho \iint r^3\, \phi' r\, \frac{p^2}{e^3\, r^3} \sin p\, d\,p\, d\,r$$

from $p=0$ to $p=e\,r$

$$= 2\,\pi\,\varrho \int \frac{r^3\, \phi' r\, d\,r}{e^3\, r^3} \left\{-2+2 \cos e\,r+2\,e\,r \sin e\,r-e^2\, r^2 \cos e\,r\right\}$$

$$= \frac{\pi\,\varrho\, e}{2} \int r^4\, \phi' r\, d\,r$$

$$\Sigma\, m\, \frac{\phi' r}{r}\,\delta\, x\,\delta\, y \sin f\,\delta\, y=\varrho \iiint \frac{\phi' r}{r}\, r^4 \sin^2 \theta \cos \theta \sin \phi \sin (f\, r \sin \theta \sin \phi)\, d\,r\, d\,\theta\, d\,\phi$$

$$=\varrho\iiint\frac{\phi' r}{r}r^4\sin\phi\frac{p^2}{f^3 r^3\sin^3\phi}\,dp\,dr\,d\phi\sin p$$

from $p=0$ to $p=fr\sin\phi$

$$=\varrho\iint\frac{\phi' r}{f^3 r\sin^2\phi}\Big\{-2+2\cos(fr\sin\phi)+2fr\sin\phi\sin(fr\sin\phi)$$

$$-f^2r^2\sin^2\phi\cos(fr\sin\phi)\Big\}\,dr\,d\phi$$

This cannot be integrated with respect to ϕ by any known process; but the *form* of the result (supposed to vanish at the upper limit) is ϱf P.

Hence $$M=Ae+Bf\frac{\cos\phi}{\sin\phi}$$

and $$\frac{M}{e}=A+B\frac{f}{e}\frac{\cos\phi}{\sin\phi}=A+B$$

$$\frac{M_{,}}{e_{,}}=-\left(A+B\frac{f}{e_{,}}\frac{\cos\phi_{,}}{\sin\phi_{,}}\right)=-(A+B)$$

the integrals for x, being all negative

$$\therefore\quad\frac{M}{e}+\frac{M_{,}}{e_{,}}=0$$

Similarly $$\frac{F}{f}+\frac{F_{,}}{f}=0$$

Another equation $\frac{M}{e}+\frac{F}{f}=0$ must be employed in reducing the equations in a symmetrical medium. But, although I am satisfied of the truth of the equation, I am not prepared to establish it by direct reasoning.

The application which I propose to make of the equations now established, is, to determine the phase and intensity of the reflected vibration in the case where there is no proper transmitted one.

For the sake of brevity let us write a for $\dfrac{\frac{c^2}{2}-D}{M}$, and b for $\dfrac{\frac{c^2}{2}-D_{,}}{M_{,}}$, s for $-\dfrac{F_{,}}{F}$; the medium not being necessarily symmetrical.

Our equations of motion thus become ($x=0$).

$$(I-R)\sin\phi+R_x=T_x \quad\ldots\ldots\quad (1)$$

$$(I+R)\cos\phi+R_y=T_y \quad\ldots\ldots\quad (2)$$

$$I'-R'+R_z=T_z \quad\ldots\ldots\quad (3)$$

$$aR_x+bT_x+\left(\frac{dI}{dx}-\frac{dR}{dx}\right)\frac{\sin\phi}{e}-\frac{dR_y}{dy}\frac{1}{e}+s\frac{dT_y}{dy}\frac{1}{e}=0 \quad\ldots\quad (4)$$

$$aR_y+bT_y+\left(\frac{dI}{dx}+\frac{dR}{dx}\right)\frac{\cos\phi}{e}-\frac{dR_x}{dy}\frac{1}{e}+s\frac{dT_x}{dy}\frac{1}{e}=0 \quad\ldots\quad (5)$$

$$aR_z+bT_z+\left(\frac{dI'}{dx}-\frac{dR'}{dx}\right)\frac{1}{e}=0 \quad\ldots\ldots\quad (6)$$

Denote $fy+ct$ by θ, and when $x=0$

$$\mathrm{I}=i\cos\theta,\ \mathrm{R}=r\cos(\theta+\alpha),\ \mathrm{R}_x=\rho\cos(\theta+\beta)$$

$$\mathrm{T}_x=t\cos(\theta+\beta'),\ \mathrm{R}_y=\sigma\cos(\theta+\gamma),\ \mathrm{T}_y=\tau\cos(\theta+\gamma')$$

The equations (1), (2), (4) and (5) become

$$(i\cos\theta-r\cos\overline{\theta+\alpha})\sin\phi=t\cos(\theta+\beta')-\rho\cos(\theta+\beta) \quad (1)$$

$$(i\cos\theta+r\cos\overline{\theta+\alpha})\cos\phi=\tau\cos\overline{\theta+\gamma'}-\sigma\cos\overline{\theta+\gamma} \quad (2)$$

$$a\rho\cos\overline{\theta+\beta}+bt\cos\overline{\theta+\beta'}-(i\sin\theta+r\sin\overline{\theta+\alpha})\sin\phi+\sigma\sin\overline{\theta+\gamma}\tan\phi - \sigma\tau\sin\overline{\theta+\gamma'}\tan\phi=0 \quad (3)$$

$$a\sigma\cos\overline{\theta+\gamma}+b\tau\cos\overline{\theta+\gamma'}-(i\sin\theta-r\sin\overline{\theta+\alpha})\cos\phi+\rho\sin\overline{\theta+\beta}\tan\phi - st\sin(\theta+\beta')\tan\phi=0 \quad (4)$$

Now θ is indeterminate; hence, equating to zero the co-efficients respectively of $\cos\theta$ and of $\sin\theta$ we obtain:

$$(i-r\cos\alpha)\sin\phi=t\cos\beta'-\rho\cos\beta \quad (1)$$

$$r\sin\alpha\sin\phi=-t\sin\beta'+\rho\sin\beta \quad (2)$$

$$(i+r\cos\alpha)\cos\phi=\tau\cos\gamma'-\sigma\cos\gamma \quad (3)$$

$$-r\sin\alpha\cos\phi=-\tau\sin\gamma'+\sigma\sin\gamma \quad (4)$$

$$a\rho\cos\beta+bt\cos\beta'-r\sin\alpha\sin\phi+\sigma\sin\gamma\tan\phi-s\tau\sin\gamma'\tan\phi=0 \quad (5)$$

$$-a\rho\sin\beta-bt\sin\beta'-(i+r\cos\alpha)\sin\phi+\sigma\cos\gamma\tan\phi-s\tau\cos\gamma'\tan\phi=0 \quad (6)$$

$$a\sigma\cos\gamma+b\tau\cos\gamma'+r\sin\alpha\cos\phi+\rho\sin\beta\tan\phi-st\sin\beta'\tan\phi=0 \quad (7)$$

$$-a\sigma\sin\gamma-b\tau\sin\gamma'-(i-r\cos\alpha)\cos\phi+\rho\cos\beta\tan\phi-st\cos\beta'\tan\phi=0 \quad (8)$$

Also of i' r' represent the co-efficients of vibration of the incident and reflected wave parallel to the axis of z; and if the actual vibrations make respectively the angles ω, δ with the plane of xy, $i'=i\tan\omega$, $r'=r\tan\delta$. If, however, the reflected vibration parallel to z suffer a different retardation from that which is in the place of xy, we must simply denote it by $r'\cos(\theta+\alpha')$. Let also $\mathrm{R}_z=\pi\cos(\theta+\epsilon)$, $\mathrm{T}_z=\psi\cos(\theta+\epsilon')$. Then we have the following equations:

$$i'-r'\cos\alpha'=\psi\cos\epsilon'-\pi\cos\epsilon \quad (9)$$

$$r'\sin\alpha'=-\psi\sin\epsilon'+\pi\sin\epsilon \quad (10)$$

$$a\pi\cos\epsilon+b\psi\cos\epsilon'-r'\sin\alpha'=0 \quad (11)$$

$$-a\pi\sin\epsilon-b\psi\sin\epsilon'-(i'+r'\cos\alpha')=0 \quad (12)$$

These are the twelve equations of motion: and the first eight of them contain ten unknown quantities; the last four, six; which have no apparent dependence on the others. We will first solve them separately by means of particular hypotheses.

We commence with waves in the plane of incidence. 1. Let us suppose that the lost vibrations in the same direction have the same phase, *i. e.*, that $\beta=\beta'$, $\gamma=\gamma'$.

Our equations become

$$(i-r\cos a)\sin\phi=(t-\varrho)\cos\beta;\ r\sin a\sin\phi=-(t-\varrho)\sin\beta$$
$$(i+r\cos a)\cos\phi=(\tau-\sigma)\cos\gamma;\ r\sin a\cos\phi=(\tau-\sigma)\sin\gamma$$
$$r\sin a\sin\phi=(a\varrho+b\,t)\cos\beta+(\sigma-s\,t)\tan\phi\sin\gamma$$
$$(i+r\cos a)\sin\phi=-(a\varrho+b\,t)\sin\beta+(\sigma-s\,\tau)\tan\phi\cos\gamma$$
$$r\sin a\cos\phi=-(a\,\sigma+b\,\tau)\cos\gamma-(\varrho-s\,t)\tan\phi\sin\beta$$
$$(i-r\cos a)\cos\phi=-(a\,\sigma+b\,\tau)\sin\gamma+(\varrho-s\,t)\tan\phi\cos\beta$$

By substituting in the last four equations the values of $\cos\beta$, $\cos\gamma$, &c, from the first four, we obtain

$$r\sin a=\frac{a\varrho+b\,t}{t-\varrho}(i-r\cos a)+\frac{\sigma-s\,\tau}{\tau-\sigma}r\sin a$$

$$(i+r\cos a)=\frac{a\varrho+b\,t}{t-\varrho}r\sin a+\frac{\sigma-s\,\tau}{\tau-\sigma}(i+r\cos a)$$

$$r\sin a=-\frac{a\,\sigma+b\,\tau}{\tau-\sigma}(i+r\cos a)+\frac{\varrho-s\,t}{\tau-\varrho}\tan^2\phi\,r\sin a$$

$$i-r\cos a=-\frac{a\,\sigma+b\,\tau}{\tau-\sigma}r\sin a+\frac{\varrho-s\,t}{t-\varrho}\tan^2\phi\,(i-r\cos a)$$

Hence
$$\left(1-\frac{\sigma-s\,\tau}{\tau-\sigma}\right)r\sin a=\frac{a\varrho+b\,t}{t-\varrho}(i-r\cos a)$$

$$\left(1-\frac{\sigma-s\,\tau}{\tau-\sigma}\right)(i+r\cos a)=\frac{a\varrho+b\,t}{t-\varrho}r\sin a$$

$$\therefore\quad i^2-r^2\cos^2 a=r^2\sin^2 a$$

or
$$i=r$$

Moreover, by means of the first four equations we get

$$(i^2-r^2)\sin\phi\cos\phi=(t-\varrho)(\tau-\sigma)\cos(\beta-\gamma)$$

$$\therefore\quad t=\varrho\ \text{or}\ \tau=\sigma\ \text{or}\ \beta-\gamma=\frac{\pi}{2}$$

a. If $t=\varrho,\ a=0$

b. If $\tau=\sigma,\ a=\pi$

c. If $\beta-\gamma=\frac{\pi}{2}$, $\sin\beta=\cos\gamma$, $\cos\beta=-\sin\gamma$

and
$$\frac{\sin a}{1-\cos a}=-\tan\beta=\cot\gamma=\frac{1+\cos a}{\sin a}$$

$$\therefore\quad \gamma=\frac{a}{2}\ \text{and}\ \beta=\frac{\pi}{2}+\frac{a}{2}$$

For (*a*.) we get $r=i,\ a=0,\ \beta=\frac{\pi}{2},\ \gamma=0$

$$\rho=-\frac{4i}{a+b}\sin\phi,\ \sigma=-\frac{2bi}{a+b}\cos\phi$$

$$t=-\frac{4i}{a+b}\sin\phi,\ \tau=\ \frac{2ai}{a+b}\cos\phi$$

For (*b*.) we get $r=i,\ a=\pi,\ \beta=0,\ \gamma=\frac{\pi}{2}$

$$\sigma=-\frac{2i}{(a+b)\cos\phi},\ \rho=-\frac{2bi}{a+b}\sin\phi$$

$$\tau=-\frac{2i}{(a+b)\cos\phi},\ t=\ \frac{2ai}{a+b}\sin\phi$$

For (*c*.) we get $i=r,\ \gamma=\frac{a}{2},\ \beta=\frac{\pi}{2}+\frac{a}{2}$

which includes both the other cases.

The problem which we have now solved may be considered as the simplest form in which reflection can be produced without refraction. Perhaps, in the application of this theory to light, no instance might be found exactly to correspond with this, but it has the advantage of serving as a first approximation, by means of which the more complete solution of the problem can be arrived at by successive steps. It will appear in the sequel that the solution of the problem is too laborious to invite us to any thing beyond a second approximation. The results of that approximation will, however, we think, be found sufficiently important in themselves to warrant us in proceeding thus far with the solution of our problem.

We now proceed to the discussion of our equations. Let

$$\beta=\beta'+\delta,\ \gamma=\gamma'+\delta'$$
$$\cos\beta=\cos\beta'+h,\ \sin\beta=\sin\beta'+e$$
$$\cos\gamma=\cos\gamma'+k,\ \sin\gamma=\sin\gamma'+l$$

$$\therefore\quad (i-r\cos a)\sin\phi=t\cos\beta'-\rho\cos\beta'-\rho h\quad . \ . \ \text{by (1)}$$

and $$\cos\beta'=\frac{(i-r\cos a)\sin\phi}{t-\rho}+\frac{\rho h}{t-\rho}$$

Similarly $$\sin\beta'=-\frac{r\sin a\sin\phi}{t-\rho}+\frac{\rho e}{t-\rho}\quad . \ . \ . \ . \ . \ \text{by (2)}$$

$$\therefore\quad \cos\beta=\frac{(i-r\cos a)\sin\phi}{t-\rho}+\frac{th}{t-\rho}$$

$$\sin\beta=-\frac{r\sin a\sin\phi}{t-\rho}+\frac{te}{t-\rho}$$

Also $$\cos\gamma' = \frac{(i+r\cos\alpha)\cos\phi}{\tau-\sigma} + \frac{\sigma k}{\tau-\sigma} \quad \ldots\ldots \text{ by (3)}$$

$$\sin\gamma' = \frac{r\sin\alpha\cos\phi}{\tau-\sigma} + \frac{\sigma l}{\tau-\sigma} \quad \ldots\ldots \text{ by (4)}$$

$$\therefore \quad \cos\gamma = \frac{(i+r\cos\alpha)\cos\rho}{\tau-\sigma} + \frac{\tau k}{\tau-\sigma}$$

$$\sin\gamma = \frac{r\sin\alpha\cos\phi}{\tau-\sigma} + \frac{\tau l}{\tau-\sigma}$$

From these equations we obtain

$$\rho^2(e^2+h^2)+2\rho\sin\phi\,(h\,\overline{i-r\cos\alpha}-e\,r\sin\alpha)=$$
$$t^2(e^2+h^2)+2\,t\sin\phi\,(h\,\overline{i-r\cos\alpha}-e\,r\sin\alpha)$$

or $$(t^2-\rho^2)(e^2+h^2)+2(t-\rho)\sin\phi\,(h\,\overline{i-r\cos\alpha}-e\,r\sin\alpha)=0$$

and $$(\tau^2-\sigma^2)(l^2+k^2)+2(\tau-\sigma)\cos\phi\,(k\,\overline{i+r\cos\alpha}+l\,r\sin\alpha)=0$$

or $$\begin{cases}(t+\rho)(e^2+h^2)+2\sin\phi\,(h\,\overline{i-r\cos\alpha}-e\,r\sin\alpha)=0 & \ldots \quad (1')\\ (\tau+\sigma)\ (l^2+k^2)+2\cos\phi\,(h\,\overline{i+r\cos\alpha}+l\,r\sin\alpha)=0 & \ldots \quad (2')\end{cases}$$

By substituting the values of $\cos\beta$, $\cos\beta'$, &c. given above, in the remaining four equations, they become

$$(a\rho+b\,t)\frac{(i-r\cos\alpha)\sin\phi}{t-\rho} - r\sin\alpha\sin\phi + \frac{\sigma-s\,\tau}{\tau-\sigma}\,r\sin\alpha\sin\phi$$
$$+\frac{a\rho\,t\,h+b\,t\,\rho\,h}{t-\rho}+\tan\phi\,\frac{\sigma\tau\,l-s\,\sigma\tau\,l}{\tau-\sigma}=0 \quad \ldots\ldots\ldots\ldots \quad (5)$$

$$(a\rho+b\,t)\frac{r\sin\alpha\sin\phi}{t-\rho}-(i+r\cos\alpha)\sin\phi+\frac{(\sigma-s\,\tau)\sin\phi\,(i+r\cos\alpha)}{\tau-\sigma}$$
$$-\frac{a\rho\,t\,e+b\rho\,t\,e}{t-\rho}+\tan\phi\,\frac{\sigma\tau k-s\,\tau\sigma k}{\tau-\sigma}=0 \quad \ldots\ldots\ldots\ldots \quad (6)$$

$$(a\,\sigma+b\,\tau)\frac{(i+r\cos\alpha)\cos\phi}{\tau-\sigma}+r\sin\alpha\cos\phi-\frac{(\rho-s\,t)\,r\sin\alpha}{t-\rho}\cdot\frac{\sin^2\phi}{\cos\phi}$$
$$+\frac{a\,\sigma\tau k+b\,\tau\sigma k}{\tau-\sigma}+\tan\phi\,\frac{\rho\,t\,e-s\,t\,\rho\,e}{t-\rho}=0 \quad \ldots\ldots\ldots\ldots \quad (7)$$

$$-(a\,\sigma+b\,\tau)\frac{r\sin\alpha\cos\phi}{\tau-\sigma}-(i-r\cos\alpha)\cos\phi+(\rho-s\,t)\frac{(i-r\cos\alpha)}{t-\rho}\cdot\frac{\sin^2\phi}{\cos\phi}$$
$$-\frac{a\,\sigma\tau\,l+b\,\tau\sigma\,l}{\tau-\sigma}+\tan\phi\,\frac{\rho\,t\,h-s\,t\,\rho\,h}{t-\rho}=0 \quad \ldots\ldots\ldots\ldots \quad (8)$$

From (5) and (6) by eliminating $a\rho+b\,t$ we get

$$-r^2\sin^2\alpha\sin\phi\,\frac{(\overline{1+s}\,\tau-2\,\sigma)}{\tau-\sigma}+(i^2-r^2\cos^2\alpha)\sin\phi\,\frac{(\overline{1+s}\,\tau-2\,s)}{\tau-\sigma}$$
$$+\frac{(a+b)\,\rho\,\tau}{\tau-\rho}(h\,r\sin\alpha+e\,\overline{i-r\cos\alpha})$$
$$+\tan\phi\,\frac{(1-s)\,\sigma\tau}{\tau-\sigma}(l\,r\sin\alpha-k\,\overline{i-r\cos\alpha})=0$$

or $$(i^2-r^2)\sin\phi\,\frac{\overline{1+s}\,\tau-2\sigma}{\tau-\sigma}+\frac{(a+b)\,\rho\,t\,(hr\sin\alpha+e\,\overline{i-r\cos\alpha})}{t-\rho}$$

$$+\tan\phi\,\frac{(1-s)\,\sigma\tau}{\tau-\sigma}\,(lr\sin\alpha-k\,\overline{i-r\cos\alpha})=0 \quad . \; . \; . \; . \; . \; . \; . \; . \; . \quad \text{(I)}$$

And by eliminating the second term, we get

$$\frac{a\rho+bt}{t-\rho}\,(i^2-r^2)\sin\phi+\frac{(a+b)\,\rho\,t}{t-\rho}\,(h\,\overline{i+r\cos\alpha}+er\sin\alpha)$$

$$+\tan\phi\,\frac{\sigma\tau}{\tau-\sigma}\,(1-s)\,(l\,\overline{i+r\cos\alpha}-kr\sin\alpha)=0 \quad . \; . \; . \; . \; . \quad \text{(II)}$$

By treating (7) and (8) in a similar manner, we obtain

$$-(i^2-r^2)\frac{(\overline{t-\rho}\cos^2\phi+s\,\overline{t-\rho}\sin^2\phi)}{(t-\rho)\cos\phi}+\frac{(a+b)\,\sigma\tau}{\tau-\sigma}(kr\sin\alpha-l\,\overline{i+r\cos\alpha})$$

$$+\tan\phi\,(1-s)\,\frac{\rho\,t}{t-\rho}\,(er\sin\alpha+h\,\overline{i+r\cos\alpha})=0 \quad . \; . \; . \; . \; . \; . \; . \; . \; . \; . \quad \text{(III)}$$

$$\frac{a\sigma+b\tau}{\tau-\sigma}\,(i^2-r^2)\cos\phi+\frac{(a+b)\,\sigma\tau}{\tau-\sigma}\,(k\,\overline{i-r\cos\alpha}-lr\sin\alpha)$$

$$+\tan\phi\,(1-s)\,\frac{\rho\,t}{t-\rho}\,(e\,\overline{i-r\cos\alpha}+hr\sin\alpha)=0 \quad . \; . \; . \; . \; . \; . \; . \; . \quad \text{(IV)}$$

From I. $$(i^2-r^2)\sin\phi\,\frac{(1+s)\,\tau-2\sigma}{\tau-\sigma}+\frac{(a+b)\,\rho\,t\,(hr\sin\alpha+e\,\overline{i-r\cos\alpha})}{t-\rho}=0$$

II. $$(a\rho+bt)\,(i^2-r^2)\sin\phi+(a+b)\,\rho\,t\,(h\,\overline{i+r\cos\alpha}+er\sin\alpha)=0$$

Now, our first approximation gave $\beta=\frac{\pi}{2}$.

Let β then be equal to $\frac{\pi}{2}+\theta$ where θ is a small quantity: $\beta'=\frac{\pi}{2}+\psi$

Hence $$\cos\beta=-\sin\theta\,,\ \cos\beta'=-\sin\psi$$

$$\sin\beta=\cos\theta\,,\ \sin\beta'=\cos\psi$$

$\therefore$ $$h=-(\sin\theta-\sin\psi)=-(\theta-\psi)+\frac{1}{6}(\theta^3-\psi^3)\ \text{ approximately}$$

$$e=\cos\theta-\cos\psi=-\frac{\theta^2-\psi^2}{2}+$$

$$k=-\frac{\gamma^2-\gamma'^2}{2}+\ .\ .$$

$$l=(\gamma-\gamma')-\frac{1}{6}(\gamma^3-\gamma'^3)$$

We return now to our original equations, and determine the values of t, ρ, &c. from them.

By (1) and (2) we obtain

$$t\sin(\beta-\beta')=(i-r\cos\alpha)\sin\phi\sin\beta+r\sin\alpha\sin\phi\cos\beta$$

$$t = \frac{(i-r\cos a)\cos\theta - r\sin a\sin\theta}{\sin(\theta-\psi)}\sin\phi$$

$$= \frac{i\cos\theta - r\cos(a-\theta)}{\sin(\theta-\psi)}\sin\phi$$

$$= \frac{i - \dfrac{i\theta^2}{2} - r + \dfrac{r(a-\theta)^2}{2}}{\theta-\psi}\left(1+\frac{\overline{\theta-\psi}^2}{6}\right)\sin\phi$$

$$= \sin\phi\,\frac{i-r-\frac{1}{2}(i\theta^2 - r\,\overline{a-\theta}^2)}{\theta-\psi}$$

omitting the product $(i-r)\ (\theta-\psi)^2$ of three dimensions.

Similarly we obtain

$$\varrho = \frac{i\cos\psi - r\cos(a-\psi)}{\sin(\theta-\psi)}\sin\phi$$

$$= \sin\phi\,\frac{i-r-\frac{1}{2}(i\psi^2 - r\,\overline{a-\psi}^2)}{\theta-\psi}$$

$$t-\varrho = -\frac{1}{2}\sin\phi\,(i\,\overline{\theta+\psi} + r\,\overline{2a-\theta-\psi})$$

$$= -\frac{1}{2}\sin\phi\,(2ar + \overline{i-r}\,.\,\overline{\theta+\psi})$$

$$\frac{\varrho t}{t-\varrho} = -\frac{2\sin\phi}{(\theta-\psi)^2}\,\frac{(i-r)^2 + (i-r)\,r\,.\,\overline{a^2 - a(\theta+\psi)}}{i(\theta+\psi) + r(2a-\theta-\psi)}$$

$$= -\frac{\sin\phi}{(\theta-\psi)^2}\,\frac{(i-r)}{ar}\left\{i-r+i(a^2 - a\,\overline{\theta+\psi}) - (i-r)^2\,\frac{\theta+\psi}{2ai}\right\}$$

to no dimensions,

$$a\varrho + bt = \frac{\sin\phi}{\theta-\psi}\left\{(a+b)(i-r) + \frac{1}{2}\,r\,(\overline{(a+b)\,a^2 - 2a(a\psi + b\theta)})\right\}$$

$$\tau = \cos\phi\,\frac{i\gamma - r\,\overline{a-\gamma} - \dfrac{1}{6}\,(i\gamma^3 - r\,\overline{a-\gamma}^3) + (i\gamma - r\,\overline{a-\gamma})\,\dfrac{\overline{\gamma-\gamma'}^2}{6}}{\gamma-\gamma'}$$

$$\sigma = \cos\phi\,\frac{i\gamma' - r\,\overline{a-\gamma'} - \dfrac{1}{6}\,(i\gamma'^3 - r\,\overline{a-\gamma'}^3) + (i\gamma' - r\,\overline{a-\gamma'})\,\dfrac{\overline{\gamma-\gamma'}^2}{6}}{\gamma-\gamma'}$$

$$\tau-\sigma = \cos\phi\,\left\{i+r-\frac{1}{6}\,(i\,\overline{\gamma^2+\gamma\gamma'+\gamma'^2} + r\,\overline{3a^2 - 3(\gamma+\gamma')\,a + \gamma^2 + \gamma\gamma' + \gamma'^2}) \right.$$
$$\left. + \frac{1}{6}\,(i+r)\,(\gamma-\gamma')^2\right\}$$

$$\frac{\sigma\tau}{\tau-\sigma} \text{ to no dimensions } = \cos\phi\,\frac{(i\gamma - r\,\overline{a-\gamma})\,(i\gamma' - r\,\overline{a-\gamma'})}{(i+r)\,(\gamma-\gamma')^2}$$

$$\frac{(1+s)\,\tau - 2\sigma}{\tau-\sigma} = 2-(1-s)\,\frac{\tau}{\tau-\sigma}$$

$$= 2-(1-s)\,\frac{i\gamma - r(a-\gamma)}{(i+r)\,(\gamma-\gamma')} \text{ to one dimension.}$$

If we substitute these approximations in 1′ we obtain, to two dimensions

$$2\sin\phi\,\frac{i-r}{\theta-\psi}(\theta-\psi)^2+2\sin\phi\{-(\theta-\psi)(i-r)\}=0$$ an identical equation:

and in (2′)

$$\cos\phi\left\{\frac{\overline{i+r}\,\overline{\gamma+\gamma'}-2ra}{\gamma-\gamma'}\right\}(\gamma-\gamma')^2+2\cos\phi\,(-\frac{\gamma^2-\gamma'^2}{2}\overline{i+r}+\overline{\gamma-\gamma'}\,ra)=0$$

also an identical equation.

If we proceed one dimension higher we get

$$-\sin\phi\,\frac{i\,\overline{\theta^2+\psi^2}-r\,(\overline{a-\theta}^2+\overline{a-\psi}^2)}{2(\theta-\psi)}(\theta-\psi)^2+2\sin\phi\left\{-\frac{ra^2}{2}\overline{\theta-\psi}+\frac{\theta^2-\psi^2}{2}ra\right\}=0$$

or $$-\frac{(i-r)(\theta^2+\psi^2)-2ra^2+2ra\,\overline{\theta+\psi}}{2}-ra^2+ra(\theta+\psi)=0$$

which (omitting higher dimensions) is again an identical equation. Also (2′) has no term of the next dimension.

We proceed to make the substitutions in I. II. III. IV.

I. $$hr\sin a+e(i-r\cos a)=-(\theta-\psi)\,ra-\frac{\theta^2-\psi^2}{2}(i-r)$$

Hence the equation to two dimensions is

$$(i^2-r^2)\sin\phi\left(2-\overline{1-s}\,\frac{i\gamma-r\,\overline{a-\gamma}}{(i+r)(\gamma-\gamma')}\right)$$

$$+(a+b)\frac{2\sin\phi}{\theta-\psi}(ra+\frac{i-r}{2}\,\overline{\theta+\psi})\,\frac{(i-r)^2-r(i-r)(a^2-a\,\overline{\theta+\psi})}{i(\theta+\psi)+r(2a-\theta-\psi)}=0$$

Dividing, we get

$$2(i+r)-(1-s)\frac{i\gamma-r(a-\gamma)}{\gamma-\gamma'}+\frac{a+b}{\theta-\psi}\times$$

$$\frac{i-r+ra(a-\overline{\theta+\psi})+\dfrac{\overline{i-r}^2(\theta+\psi)}{2ra}}{1+\dfrac{i-r}{2ra}(\theta+\psi)}=0$$

or $$2(i+r)-(1-s)\frac{i\gamma-r(a-\gamma)}{\gamma-\gamma'}+\frac{a+b}{\theta-\psi}\Big(i-r+ra\,(a-\overline{\theta+\psi})\Big)=0$$

But $$\iota=-\frac{4i}{a+b}\sin\phi\;\therefore\;\frac{4i}{a+b}=-\frac{i-r}{\theta-\psi}\text{ approx.}$$

and $$\tau=\frac{2ai}{a+b}\cos\phi\;\therefore\;\frac{2a}{a+b}=\frac{2\gamma-a}{\gamma-\gamma'}$$

$$\sigma=-\frac{2bi}{a+b}\cos\phi\;\therefore-\frac{2b}{a+b}=\frac{2\gamma'-a}{\gamma-\gamma'}$$

If we suppose $b=a$, as is most probably the case, we have

$$0=2\gamma+2\gamma'-2a$$

$$\therefore\ a=\gamma+\gamma' \quad . \quad . \quad . \quad . \quad . \quad (a)$$

and
$$i-r=-\frac{2i}{a}(\theta-\psi) \quad . \quad . \quad . \quad (b)$$

II. $h(i+r\cos a)+e\,r\sin a=-\overline{\theta-\psi}\,(i+r)$.

Hence, to no dimensions we have

$$\frac{i^2-r^2}{\theta-\psi}\{\overline{a+b}\,\overline{i-r}+\tfrac{1}{2}r(\overline{a+b}\,a^2-2a\,\overline{a\psi+b\theta})\}$$

$$-\frac{\overline{a+b}}{(\theta-\psi)}\{\overline{i-r}^2+\overline{i-r}\,r\,(a^2-a\,\overline{\theta+\psi})\}\,\overline{i+r}=0$$

or
$$\overline{a+b}\,\overline{i-r}+\tfrac{1}{2}r(\overline{a+b}\,a^2-2a\,\overline{a\psi+b\theta})$$

$$-\overline{a+b}\{\overline{i-r}+r\,\overline{a^2-a(\theta+\psi)}\}=0$$

$$\therefore\ \overline{a+b}\,a^2-2a\,\overline{a\psi+b\theta}=2(a^2-a\,\overline{\theta+\psi})(a+b)$$

$$(a+b)\,a-2\,\overline{a\psi+b\theta}=(2a-2\,\overline{\theta+\psi})(a+b)$$

$$(a+b)\,a=2(-a\psi-b\theta+\overline{a+b}\,\overline{\psi+\theta})$$

$$\therefore\ (a+b)\,a=2(a\theta+b\psi)$$

If
$$a=b;\ a=(\psi+\theta) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (c)$$

Approximately, then, $a=\gamma+\gamma'$, and $\beta+\beta'-\pi$

III. $$\frac{(t-\varrho)\cos^2\phi+(s\,t-\varrho)\sin^2\phi}{t-\varrho}=1-\frac{(1-s)\,t\sin^2\phi}{t-\varrho}$$

$$-1=(1-s)\sin^2\phi\frac{i-r+\dfrac{1}{2}\,i\,(a^2-2a\theta)}{a\,r\,(\theta-\psi)\left(1+\dfrac{(i-r)(\theta+\psi)}{2ar}\right)}$$

$$=1+(1-s)\sin^2\phi\frac{i-r+\dfrac{i}{2}(a^2-2a\theta)-\dfrac{(i-r)^2(\theta-\psi)}{2ar}}{a\,r\,(\theta-\psi)}$$

Hence, to one dimension, we have

$$-\frac{(i^2-r^2)}{\cos\phi}\left\{1+(1-s)\sin^2\phi\,\frac{i-r+\dfrac{i}{2}(a^2-2a\theta)-\dfrac{(i-r)^2(\theta+\psi)}{2ar}}{a\,r\,(\theta-\psi)}\right\}$$

$$-(a+b)\cos\theta\frac{i^2(2\gamma-a)(2\gamma'-a)}{\gamma-\gamma'}+\frac{\sin^2\phi}{\cos\phi}(1-s)\frac{(i^2-r^2)}{a\,r\,(\theta-\psi)}\times$$

$$\left\{i-r+i\,a^2-a\,\overline{\theta+\psi}-(i-r)^2\frac{\theta+\psi}{2ai}\right\}=0$$

or $$-\frac{i^2-r^2}{\cos\phi}-(1-s)(i^2-r^2)\frac{\sin^2\phi}{\cos\phi}\left\{\frac{\frac{i}{2}(a^2-2a\theta)-\frac{(i-r)^2(\theta+\psi)}{2ai}}{ai(\theta-\psi)}\right\}$$

$$-(a+b)\cos\phi\,\frac{i^2(2\gamma-a)(2\gamma'-a)}{\gamma-\gamma'}+(1-s)\frac{\sin^2\phi}{\cos\phi}\,\frac{i^2-r^2}{ai(\theta-\psi)}$$

$$\left\{i(a^2-a\,\overline{\theta+\psi})-(i-r)^2\frac{\theta+\psi}{2ai}\right\}=0$$

or $$-\frac{i^2-r^2}{\cos\phi}+(1-s)\frac{\sin^2\phi}{\cos\phi}\,\frac{i^2-r^2}{ai(\theta-\psi)}\left\{\frac{i}{2}\,a^2-ia\psi\right\}+\frac{4ab}{a+b}\cos\phi\,(\gamma-\gamma')\,i^2=0$$

or $$\frac{i^2-r^2}{\cos\phi}+(1-s)\frac{\sin^2\phi}{\cos\phi}\,\frac{4i^2}{a+b}(a-2\psi)-\frac{4ab}{a+b}\cos\phi\,(\gamma-\gamma')\,i^2=0$$

IV. To two dimensions:

$$\frac{a(2\gamma'-r\,\overline{a-\gamma'})+b(i\gamma-r\,\overline{a-\gamma})}{(\gamma-\gamma')(i+r)}\cos\phi\,(i^2-r^2)$$

$$-(a+b)\cos\phi\,\frac{(i\gamma-r\,\overline{a-\gamma})(2\gamma'-r\,\overline{a-\gamma'})}{(i+r)(\gamma-\gamma')}\,r\,a$$

$$+(1-s)\tan\phi\,\frac{\sin\phi\,(i-r)}{a\,r(\theta-\psi)^2}\left\{i-r+i(a^2-a\,\overline{\theta+\psi})-(i-r)^2\frac{\theta+\psi}{2ai}\right\}$$

$$\left\{\frac{(\theta^2-\psi^2)}{2}(i-r)+(\theta-\psi)\,r\,a\right\}=0$$

If $a=b;\quad a=\gamma+\gamma',\ a=\psi+\theta$ (by II.)

$$\theta-\psi=-\frac{a}{2i}(i-r)$$

$$\therefore 2\psi=a+\frac{a}{2i}(i-r)$$

$$a-2\psi=-\frac{a}{2i}(i-r)$$

III. gives $$\frac{i^2-r^2}{\cos\phi}+(1-s)\frac{\sin^2\phi}{\cos\phi}\,\frac{4i^2}{a-b}\,\frac{a}{2i}(i-r)-\frac{4ab}{a+b}\cos\phi\,(\gamma-\gamma')\,i^2=0$$

or $$\left(i-(1-s)\sin^2\phi\,\frac{i}{2}\right)(i-r)=a\cos^2\phi\,(\gamma-\gamma')$$

$$\therefore \gamma-\gamma'=\frac{(i-r)}{i\,a\cos^2\phi}\left(1-\frac{1-s}{2}\sin^2\phi\right)$$

or $$\gamma-\gamma'=\frac{(i-r)}{i\,a\cos^2\phi}\ \text{nearly};$$

$$\gamma+\gamma'=a$$

$$\therefore \gamma=\frac{1}{2}\left(a+\frac{(i-r)}{a\,i\cos^2\phi}\right)$$

$$\gamma'=\frac{1}{2}\left(a-\frac{(i-r)}{a\,i\cos^2\phi}\right)$$

IV. gives $\frac{a(i\gamma-r\gamma)+i\gamma-r\gamma'}{(\gamma-\gamma')(i+r)}\cos\phi\,(i^2-r^2)=a(i-r)^2\cos\phi\,\frac{a}{\gamma-\gamma'}$

$$\frac{(a+b)\cos\phi}{(i+r)(\gamma-\gamma')}(i\gamma-r)\,\overline{a-\gamma})\,(i\gamma'-r\overline{a-\gamma'})\,r\,a=-2\,a\cos\phi\,(i+r)\,r\,a\,\frac{(\gamma-\gamma')}{4}$$

$$\therefore\ a(i-r)^2\cos^2\phi\frac{a}{\gamma-\gamma'}+a\,i^2\,a\,(\gamma-\gamma')\cos^2\phi-(1-s)\sin^2\phi\,(i-r)\frac{2i}{a}=0$$

or $\quad a^2\,i\,(i-r)\cos^4\phi\,.\,a+i\,(i-r)\,a-(1-s)\frac{2i}{a}(i-r)\sin^2\phi=0$

$$\therefore\ a=\frac{\frac{2(1-s)}{a}\sin^2\phi}{a^2\cos^4\phi+1}$$

Finally, I. gives to two dimensions

$$(i^2-r^2)\left\{2-(1-s)\frac{i\gamma-r\gamma'}{(i+r)(\gamma-\gamma')}\right\}$$

$$+(a+b)\frac{(i-r)}{(\theta-\psi)^2\,a\,r}\left\{i-r-\frac{(i-r)^2}{2i}\right\}\left\{(\theta-\psi)\,r\,a+\frac{\theta^2-\psi^2}{2}(i-r)\right\}$$

$$+(1-s)\frac{(i\gamma-r\gamma)(i\gamma'-r\gamma)}{(i+r)(\gamma-\gamma')}\,r\,a=0$$

$$2(i^2-r^2)-(1-s)(i^2-r^2)\left(\frac{1}{2}+\frac{a\,a\cos^2\phi}{4}\right)$$

$$-\frac{(a+b)(i-r)}{a+b}4i\left\{1-\frac{i-r}{2i}\right\}\left\{1+\frac{i-r}{2r}\right\}$$

$$-\frac{(1-s)}{2}i^2\,a\,\frac{(i-r)}{a\,i\cos^2\phi}=0$$

or $\quad 2(i+r)-\frac{(i+r)^2}{r}-(1-s)\frac{i+r}{2}-(1-s)\,a\left\{\frac{a\,i\cos^2\phi}{2}+\frac{i}{2\,a\cos^2\phi}\right\}=0$

$$-2(i-r)-(1-s)\,i=(1-s)\,a\left\{\frac{a\,i\cos^2\theta}{2}+\frac{i}{2\,a\cos^2}\right\}$$

$$i-r=-\frac{1}{2}(1-s)\,i-\frac{1-s}{2}\,i\,a\,\frac{a^2\cos^4\phi+1}{2\,a\cos^2\phi}$$

$$=-\frac{1}{2}(1-s)\,i-\frac{(1-s)^2}{2\,a^2}\,i\tan^2\phi$$

$$=\frac{1}{2}\,m\,i+\frac{m^2}{2\,a^2}\,i\tan^2\phi\quad(m=s-1)$$

$$r=i\left\{1-\frac{1}{2}\,m+\frac{m^2}{2\,a^2}\tan^2\phi\right\}$$

But if a be not equal to b, we have the following equations:

$$\theta-\psi=-\frac{a+b}{4i}(i-r)\quad .\quad .\quad .\quad .\quad (1.)$$

$$\frac{2\gamma-a}{\gamma-\gamma'}=\frac{2a}{a+b}\quad .\quad .\quad .\quad .\quad .\quad (2.)$$

$$\frac{2\gamma - a}{\gamma - \gamma'} = -\frac{2b}{a+b} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3.)$$

$$2(i+r) - (1-s)\frac{i\gamma - r(a-\gamma)}{\gamma - \gamma'} + \frac{a+b}{\theta + \psi}\left(i - r + ra(a - \overline{\theta+\psi})\right) = 0 \quad (4.)$$

$$(a+b)\,a = 2(a\theta + b\psi) \quad . \quad . \quad . \quad . \quad . \quad . \quad (5.)$$

$$i^2 - r^2 + (1-s)\sin^2\phi\,\frac{4i^2}{a+b}(a - 2\psi) - \frac{4ab}{a+b}\cos^2\phi\, i^2(\gamma - \gamma') = 0 \quad . \quad (6.)$$

and IV. . . . . (7.)

From (1.) and (5.),

$$2(a+b)\,\theta = (a+b)\,a - \frac{(a+b)}{2i}b(i-r)$$

$$\theta = \frac{a}{2} - \frac{b}{4i}(i-r)$$

$$\psi = \frac{a}{2} + \frac{a}{4i}(i-r)$$

From (2.) and (3.), $b\gamma + a\gamma' = \frac{a+b}{2}a$

From (6.),

$$\gamma - \gamma' = \frac{i^2 - r^2 - (1-s)\frac{4i^2}{a+b}\frac{a}{2i}(i-r)\sin^2\phi}{\frac{4ab}{a+b}\cos^2\phi\, i^2}$$

$$= \frac{a+b}{4ab}\;\frac{i^2 - r^2}{i^2\cos^2\phi}\text{ nearly}$$

$$= \frac{(a+b)(i-r)}{2abi\cos^2\phi}\text{ nearly}$$

$$\therefore\ \gamma = \frac{a}{2} + \frac{a}{2abi\cos^2\phi}(i-r) = \frac{a}{2} + \frac{i-r}{2bi\cos^2\phi}$$

$$\gamma' = \frac{a}{2} - \frac{b}{2abi\cos^2\phi}(i-r) - \frac{a}{2} - \frac{i-r}{2ai\cos^2\phi}$$

IV. gives

$$\frac{\frac{a+b}{2}a(i+r) - (a+b)ra}{\frac{(a+b)(i^2-r^2)}{2abi\cos^2\phi}}\cos\phi\,(i^2 - r^2)$$

$$+\frac{(a+b)\cos\phi\,\frac{i^2 r}{i+r}a\frac{(i-r)^2}{abi^2\cos^4\phi}}{\frac{(a+b)(i-r)}{2abi\cos^2\phi}} - (1-s)\frac{\sin^2\phi}{\cos\phi}\;\frac{4i}{a+b} = 0$$

or

$$abi\cos^2\phi\,a(i-r) + \frac{a(i-r)i}{\cos^2\phi} - (1-s)\frac{\sin^2\phi}{\cos^2\phi}\,\frac{4i}{a+b}(i-r) = 0$$

$$a = \frac{(1-s)\frac{4}{a+b}\sin^2\phi}{ab\cos^4\phi + 1}$$

Equation I. gives

$$(i^2-r^2)\left\{2-(1-s)\frac{i\gamma-r(a-\gamma)}{(i+r)(\gamma-\gamma')}\right\}$$

$$+(a+b)\frac{i-r}{(\theta-\psi)^2 ar}\left\{i-r+ia(a-\theta-\psi)-\frac{(i-r)^2(\theta+\psi)}{2ai}\right\}\left\{(\theta+\psi)ra+\frac{\theta^2-\psi^2}{2}(i-r)\right\}$$

$$+(1-s)\frac{\{i\gamma-r(a-\gamma)\}\{i\gamma'-r(a-\gamma')\}}{(i+r)(\gamma-\gamma')^2}(\gamma-\gamma')ra=0$$

Now $$\frac{i\gamma-r(a-\gamma)}{(i+r)(\gamma-\gamma')}=\frac{(i-r)\frac{a}{2}+(i+r)\frac{i-r}{2bi\cos^2\phi}}{(i+r)(a+b)\frac{i-r}{2abi\cos^2\phi}}$$

$$=\frac{aabi\cos^2\phi}{(a+b)(i+r)}+\frac{a}{a+b}$$

$$\frac{i\gamma'-r(a-\gamma')}{(i+r)(\gamma-\gamma')}=\frac{aabi\cos^2\phi}{(a+b)(i+r)}-\frac{b}{a+b}$$

$$\therefore\ \frac{\{i\gamma-r(a-\gamma)\}\{i\gamma'-r(a-\gamma')\}}{(i+r)^2(\gamma-\gamma')^2}=-\frac{ab}{a+b}+\frac{a-b}{(a+b)^2}\ \frac{aabi\cos^2\phi}{i+r}$$

Substituting, we obtain

$$2(i^2-r^2)-(1-s)\frac{a}{a+b}(i^2-r^2)-(1-s)\frac{ab}{a+b}i(i-r)a\cos^2\phi$$

$$-(a+b)\frac{i-r}{\frac{a+b\ (i-r)}{4i}}\left\{i-r-ia\frac{a-b}{4i}(i-r)-\frac{(i-r)^2}{2i}-\frac{(a-b)(i-r)^3}{8i^2a}\right\}$$

$$\times\left\{\frac{i+r}{2r}+\frac{(a-b)}{8ir}(i-r)^2\right\}$$

$$+(1-s)\frac{(a+b)(i^2-r^2)}{2abi\cos^2\phi}\ ra\ \left\{-\frac{ab}{(a+b)^2}+\frac{a-b}{(a+b)^2}\ \frac{aabi\cos^2\phi}{i+r}\right\}=0$$

Taking this to two dimensions, to which alone it is correct, we have

$$2(i^2-r^2)-(1-s)\frac{a}{a+b}(i^2-r^2)-(1-s)\frac{a}{a+b}i(i-r)a\cos^2\phi$$

$$-\frac{2i}{r}(i^2-r^2)+(i-r)^2\frac{i+r}{r}$$

$$-\frac{(1-s)}{2}\ \frac{(i^2-r^2)ra}{(a+b)i\cos^2\phi}=0$$

or $$-2(i-r)-(1-s)\frac{a}{a+b}2i-(i-s)\frac{ab}{a+b}ia\cos^2\phi$$

$$-\frac{1-s}{(a+b)}\ \frac{ia}{\cos^2\phi}=0$$

$$i-r=-(1-s)\frac{a\,i}{a+b}-\frac{(1-s)\,a\,i}{2}\left\{\frac{a\,b}{a+b}\cos^2\phi+\frac{1}{(a+b)\cos^2\phi}\right\}$$

$$r=i\left\{1-(s-1)\frac{a}{a+b}+\frac{(s-1)^2}{a\,(a+b)}\tan^2\phi\right\}$$

$$=i\left\{1-\frac{m\,a}{a+b}+\frac{m^2}{a\,(a+b)}\tan^2\phi\right\}$$

If $a-b$ be not supposed small, there is another term, which is introduced thus:

$$2\,(i^2-r^2)+\ \&c.\ +\left\{i\,a\,(i-r)+\frac{(i-r)^3}{2\,i\,a}\right\}(a-b)$$

or

$$-2\,(i-r)+\ \&c.\ +(a-b)\left\{\frac{i\,\dfrac{4\sin^2\phi}{a+b}\,(1-s)}{a\,b\cos^4\phi+1}+\frac{\dfrac{(i-r)^2\,(a\,b\cos^4\phi+1)}{8(1-s)\sin^2\phi}}{a+b}\right\}$$

$$r=i\left\{1-\frac{m\,a}{a+b}+\frac{m^2}{a\,(a+b)}\tan^2\phi+(a-b)\left(\frac{m\,\dfrac{2\sin^2\phi}{a+b}}{a\,b\cos^4\phi+1}+\frac{\dfrac{m\,a^2\,(a\,b\cos^4\phi+1)}{(a+b)}}{16\sin^2\phi}\right)\right\}$$

Thus the reflection increases with the angle of incidence, so far as this approximation proceeds.

It is not difficult to work out the solution for the vibrations parallel to the surface.

Should the conclusions to be derived from the comparison of the two results appear to give the law of elliptic polarization, I shall recur to the subject at some future time.

XXXV.—*Chemical Examination of the Tagua Nut or Vegetable Ivory.* By ARTHUR CONNELL, Esq., Professor of Chemistry in the University of St Andrews.

(Read 15th January 1844.)

THIS remarkable seed or nut is now well known in London, as a substance extensively carved into a variety of ornaments, and capable of receiving as high a polish as the finest ivory; which it also greatly resembles in colour. I lately obtained various specimens both of the nut in its natural state and of the fine turnings produced in the process of working it, being desirous of submitting them to a chemical examination.

The nuts in my possession vary in size from a pigeon's to a hen's egg, and have a somewhat angular shape. They are covered with a brown epidermis, and have an outer shell $\frac{1}{30}$ of an inch thick, and consisting of an outer white and an inner brown layer. The inner mass of the nut is remarkably close grained and homogeneous to the naked eye; and when cut and polished exactly resembles animal ivory. The hardness is considerable; the white mass yielding with some difficulty to a knife. Thin portions are translucent. The density of the white mass is 1.376, at 53° F.

Dr BALFOUR, Professor of Botany in the University of Glasgow, has been so kind as to inform me, that this vegetable ivory " is the albumen (botanically speaking), of a palm called *Phytelephas macrocarpa*, which is found on the banks of the river Magdalena in the Republic of Columbia. The natives call it Tagua or Cabeza de Negre (Negro's head)."

Mr COOPER has described, in the Microscopic Journal,* the structure exhibited by a thin slice of this substance under the microscope. He found it to consist of a homogeneous substance, without any appearance of cellular or other elementary structure, but traversed in one direction by parallel canals or tubes, somewhat irregular in their shape, and evidently filled with an oily fluid; and he attributes to the presence of these reservoirs of oil, joined to the compact texture of the substance, the pure and lasting polish of which it is susceptible.

I had made considerable progress in my chemical examination of this curious substance, before I was aware that it had been submitted to analysis by my friend

* Vol. ii. p. 97.

Dr Douglas Maclagan. I was then directed by Dr Balfour to Cormack's Journal of Medical Science,* for the results of Dr Maclagan's analysis. It appears that his specimen contained some soft matter as well as hard, and the portion examined by him consisted partly of both these kinds. The constituents he found were—

Hard woody fibre,	76.5
Vegetable albumen,	1.5
Bitter matter, soluble in water and alcohol,	2.5
Gum, with phosphate of lime,	5.5
Ashes,	0.5
Moisture,	13.5
	100.

The bitter matter, he states, evolved a urinous odour by heat; and the ashes were phosphates and a little silica. A portion of the harder part he found to contain only 9 per cent. of moisture, and 2 per cent. of ashes, consisting of earthy phosphates, silica, and various alcaline salts.

An allusion is made by Dr Maclagan to an examination of this substance by Dr Percy of Birmingham, but I have never seen his results.

My own results do not differ very materially from those of Dr Maclagan; and those differences which do occur, arise, I believe, chiefly from the circumstance that the azotised principles of vegetables have been much studied by chemists, in the short interval which has elapsed since his analysis was made. The leading difference is, that I have found the vegetable ivory to contain from 3 to 4 per cent. of an azotised substance, which appears to be either identical with or closely allied to legumin or vegetable casein. I have also found nearly 1 per cent. of fixed oil.

The matter examined by me, was, as already stated, the fine turnings obtained in the process of working and carving the nut into ornaments. I got them from one of the London workmen; and, of course, they must be viewed as the hardest portion of the nut. They constituted a white powdery substance, mixed with some larger shavings, and were capable of being reduced to still finer particles by rubbing in a mortar. When moistened with water, the transparency of the thin shavings was increased.

When the turnings were heated they took fire and burned with flame, the combustion sustaining itself like that of wood. A white ash was left, dissolved by water acidulated with an acid. When pressed in blotting paper between heated metallic plates, no oily stain was noticed, but it will afterwards be seen

* 1841, p. 614.

that a little fixed oil was obtained by the agency of solvents. Distilled with water, no trace of any volatile oil was noticed. Heated with hot water, or allowed to stand some time in contact with cold water, some soluble matter was taken up which was obtained again by evaporation. Rubbed in a mortar with water, a milky liquid was obtained, which gave only slight traces of coagulable matter by boiling; and yielded a soluble precipitate when heated with acetic acid. Both alcohol and ether, when boiled on the powder, took up a very little matter. Caustic potash ley, when boiled on it, became yellowish in colour, and when supersaturated with muriatic acid, precipitation ensued. Muriatic acid, boiled on the powder, also acquired a yellow tint.

Having, in preliminary trials, satisfied myself as to the general nature of the constituents, the following method of analysis was adopted:—

A portion of the powder was rubbed dry in a mortar to reduce it to a finer state of comminution. It was then rubbed for several minutes at a time, with successive portions of cold distilled water. The milky liquids, after settling for a few minutes, were poured out of the mortar and allowed to subside farther for a night, and what subsided was added to the mass of solid matter. Another portion of water was also left a night on the powder, and well rubbed on it next day, and then strained through two plies of thick muslin, and allowed to subside for half an hour. The whole of the milky liquids were then boiled for a few minutes. A very slight appearance of coagulation ensued, and a very little matter speedily subsided, which, from the manner in which it was obtained, was evidently a trace of vegetable albumen.

The residual solid matter was now triturated with boiling water, and left all night with the liquid: and this process was repeated several times with new portions of boiling water. A milky liquid was again obtained, which remained so, even after every thing mechanically suspended had been deposited by rest.

To the emulsion which had been boiled and separated from the albumen, as well as to that prepared with boiling water, acetic acid was added as long as a precipitate was formed. Next day the precipitate had subsided, and the liquid was clear, or very nearly so. In a day or two it was collected on a weighed filter, and dried by the heat of a salt bath and weighed. Treated with ether, nothing was taken up.

This substance, from the manner in which it was procured, as well from its leading characters with reagents, appeared to be either identical with, or nearly allied to, legumin, or what has lately been called vegetable casein. Its solution as has been seen, is not coagulated by heat, and in this respect it differs essentially from albumen. On the other hand, it is precipitated by acetic acid, precisely as an infusion of peas, which has deposited its starch, is precipitated by that acid. Farther, it is more or less soluble in caustic potash ley, and it is an azotised sub-

stance. All these characters are sufficient to shew its identity or close analogy with legumin or vegetable casein: but in the present state of great doubt whether LIEBIG is correct in holding legumin to be identical with animal casein;* and whether there is only one kind of legumin—Messrs DUMAS and CAHOURS asserting that there are two—it would evidently be premature to attempt to determine, with certainty, whether we are here dealing with a substance identical with, or only closely allied to, the azotised principle of the leguminosæ. Meanwhile, however, as it differs essentially from vegetable albumen, and is procured by the same means as the legumin of peas, and is an azotised body, I shall consider it as legumin or vegetable casein.

The liquid which had yielded the casein was evaporated to dryness, when a brownish-white matter was obtained, which was not soluble in alcohol, nor had any sweet taste, and when slightly moist had a clammy consistence. It contained no nitrogen, and seemed, in short, to have the ordinary properties of gum.

The whole of the original mass of the powder which had been rubbed with cold and hot water, was now boiled with a quantity of water, and the whole allowed to subside for a night. Next day the whole matter had subsided, leaving the liquid nearly quite clear, and acetic acid no longer produced any effect on it. Evaporated to dryness, a mere trace of gum was obtained.

The subsided powder was dried, and then treated with hot alcohol. This alcohol, by evaporation, yielded a small but decided quantity of a yellow fixed oil.

The residual powder was now treated with dilute caustic potash ley, aided by gentle heat, and also by diluted muriatic acid. The former of these liquids took up nothing; the latter, a mere trace of a mixture of two or three different matters, which were too small in amount to have their precise nature determined.

The residual white matter which had thus been successively treated with so many solvents, was free from nitrogen, and was considered as woody matter or lignin.

The powder was examined in many stages of its analysis for starch, but no traces of that substance were found.

To determine the precise quantity of water contained in this substance, the difference of loss of moisture in a dry air at 75° F., and on the sand bath at 240°, was ascertained; the loss at the former temperature being reckoned merely accidental hygrometric moisture, and that at the latter, constituent water.

The amount of ashes was ascertained by incinerating a portion of the powder; and this amount, as well as that of all the solid constituents, was computed for the powder as dried at 75°.

* See Berzelius Jahresbericht, 1842, p. 270, &c.

The result of the analysis was as follows:—

Gum,	6.73
Legumin or vegetable casein,	3.8
Vegetable albumen,	0.42
Fixed oil,	0.73
Ashes,	0.61
Water,	9.37
Lignin or other woody matter,	81.34
	100.00

In the ashes were found phosphate of lime, sulphate of potash, chloride of potassium, carbonate of lime, and a little matter insoluble in acids, and apparently siliceous. There was also a little iron; but this might have proceeded from the tools employed in working the ivory.

It thus appears, that this seed contains between 4 and 5 per cent. of azotised matters, besides a much larger proportion of non-azotised substances; all of which are available for the nourishment of the future plant during germination, as well as for the food of any animals which may partake of it. It is said, that in the young and soft state of the nut, certain wild animals are fond of it. My analysis does not present any substance which we can positively say would prove deleterious to the animal economy; although, of course, it is *possible* that some of them, such as the oil, might be so. But I made no experiments in this point of view, which, of course, it would be essential should be done, before it could be suggested that the powdered nut might, from its azotised and other constituents, be made available, in some shape or other, and to a certain limited extent, as an article of food. It is said that large cargoes of these nuts are now occasionally imported.

The chemical constitution of this substance appears to throw no farther light on its remarkable state of cohesion, than to suggest the idea, that the gum and other soluble constituents may have the effect of agglutinating the ligneous particles to such an extent as to cause its great density and tenacity.

Since this paper was read, it has been stated to me by Professor Johnstone, that Dr Baumhauer of Utrecht had lately found, by digesting the residual matter, which I have considered lignin, in strong caustic potash for many days at the ordinary temperature, the potash took up a matter, precipitated again by acids, which he thought to be a new sort of starch, differing both from common starch and from inulin. In ultimate constitution, it differed little from woody tissue or from starch. This may be all very correct; but I doubt much whether we can

with justice assume the *previous existence* of matters, obtained from organic substances by the agency of strong alkalies or strong acids. In considering the residue as ligneous matter, I followed the directions of Berzelius for obtaining that substance from vegetable matters, and employed no re-agents which could change the nature of the substances acted on. My object did not go farther.

XXXVI.—*Account of a Repetition of several of Dr Samuel Brown's Processes for the Conversion of Carbon into Silicon.* *By* George Wilson, M.D., and John Crombie Brown, Esq. *Communicated by the Secretary.*

(Read April 1. 1844.)

The object of the following paper is to lay before the Royal Society the results of a repetition of several of Dr Samuel Brown's processes for the conversion of carbon into silicon. The greater number of these processes were published in the Society's Transactions for 1840-41; and certain additional ones have since appeared in a separate form. The latter were much simpler, and more readily performed, than those made public at an earlier period; and to one of these we first directed our attention.

Before stating, however, the results of any of our trials, we think it right to mention, that most of the experiments which we now place on record, were regarded at the time of their performance as only tentative and preliminary, and were not registered with the minuteness of detail they would have been, had we expected ultimately to publish them.

We tried the greater number of Dr Brown's processes, and rejected them one after another, without pursuing their investigation farther, on finding they would not yield quantitative proofs of the conversion of carbon into silicon. The limited time which, from various circumstances, we could devote to the subject, obliged us to follow this course; and the confident expectation we entertained, till a recent period, that each new process would supply what the rejected ones had failed to afford, led us to neglect noting many particulars of our early trials, which otherwise we should have recorded.

For the sake of brevity we leave unnoticed many subsidiary points connected with our experiments, and restrict ourselves solely to those which bear upon the question of an anomalous production of silicon, and the source whence it was derived.

We commence with the account of our repetition of the process for the production of silicon from the cyanide of lead. This had the great advantage over most of the others, that it yielded the silicon uncombined, and not, as they did, in combination with oxygen as silica. It consisted in enclosing the cyanide in a glass tube shut at one end, and drawn out at the other into a capillary. Heat was then to be cautiously applied, and ultimately raised as high as was compatible with the glass remaining unfused. Treated in this manner with the precautions

described in his "Two Processes,"* cyanide of lead was found by Dr BROWN to resolve itself entirely into nitrogen, which escaped in the gaseous form, and a bluish-grey powder, which, when digested in dilute nitric acid, yielded its lead to that solvent, and left silicon as an insoluble brown powder. The general precautions indicated as essential to the success of the transmutation were easily attended to, and the only point which was much insisted on, was the necessity for the cyanide of lead being absolutely pure.

The realization of this desideratum proved much more difficult than we had at all expected; so difficult, indeed, that, after six weeks spent in unsuccessful attempts to prepare a pure cyanide of the metal in question, we abandoned, for the time, the process in despair.

The method given by Dr BROWN in his Two Processes, was to precipitate the cyanide of potassium by the neutral acetate of lead. By this process, however, and by another similar in principle, and likewise employed by that gentleman, in which ammonia, supersaturated with hydrocyanic acid, was substituted for cyanide of potassium, we did not succeed in preparing a pure cyanide of lead. The white precipitate which fell when these reagents were made use of, was found on analysis to give a proportion both of lead and of cyanogen, quite at variance with the possibility of its being the pure protocyanide; the average proportion of cyanide of lead present being, as nearly as possible, only a third of its whole weight. When distilled with oil of vitriol in a water bath, it gave off acetic as well as hydrocyanic acid; and when heated alone in a tube, cyanide of ammonium was evolved in large quantity.

We did not determine the exact composition of this compound, as it was sufficient for our purpose to know that it was not the salt we were in search of. But it appeared to consist of cyanide of lead mixed with a large proportion of a hydrated basic acetate of the same metal.

Besides the neutral acetate of lead, we employed that salt acidulated with acetic acid, the tribasic acetate, the nitrate, the basic nitrate, and the nitrite, as precipitants of the alkaline cyanide; and we varied the proportions of hydrocyanic acid and ammonia, and all the minor details of the several processes, without attaining the end we had in view. In every case the tendency of lead to form basic salts, gave rise to the mixture of the cyanide produced, with subacetate and subnitrate of the oxide of the metal.

We also digested cyanide of potassium in solution on ferrocyanide of lead, in the expectation of removing ferrocyanogen, and leaving cyanide of lead, but the process did not succeed. Nor were we more successful with another, where we digested hydrated oxide of lead in dilute hydrocyanic acid.

Finally, we had recourse to the iodide and chloride of lead, which we dissolved

* Two Processes for Silicon, by Dr SAMUEL BROWN. Edinburgh: Adam and Charles Black. 1843.

in water, and added in atomic proportions to solutions of cyanide of potassium. Where the iodide was used, the yellow colour of the precipitate shewed that it was a compound of the iodide and cyanide of lead. The precipitate varied in appearance and properties, where the chloride was employed, according as it or the cyanide of potassium was made the precipitant; but in every case it seemed to be a mixture of cyanide and chloride of lead. It retained moisture with great obstinacy, and could scarcely be deprived of it by heat without decomposition. It was accordingly placed when moist *in vacuo* over sulphuric acid, and afterwards dried completely in a current of heated air passed through chloride of calcium. But this tedious desiccation introduced a new element of impurity; for although the cyanide of potassium employed was originally quite free from carbonate of potass, the precipitate, when dry, gave off carbonic acid when treated with dilute acids. The proportion of chloride present in the precipitates varied much, and the physical characters of the body along with it. Where the cyanide of lead preponderated, it was, when dry, of a primrose yellow colour, and was changed by heat into a bluish crumbling powder. When the chloride of lead was in excess it was cream-coloured; and when heat was applied, fused and adhered to the glass.

At this stage of the inquiry, we abandoned the cyanide of lead as a raw material for transmutation, having exhausted for the time all our resources for its preparation.

We subjected, however, most of the lead precipitates to the treatment prescribed by Dr BROWN for the conversion of the carbon of the cyanide into silicon, in the expectation of procuring sufficient quantities of the latter substance to establish its nature unequivocally.

When the precipitate prepared by his own methods was heated with the precautions indicated in his "Two Processes," we obtained a powder of a bluish-grey colour, closely corresponding in physical characters to the "leaden" product he describes. When this was treated with dilute nitric acid, all the lead of the compound dissolved, and an insoluble brown powder remained, which we trusted was silicon. It was quite soluble in oil of vitriol, and was only partially destroyed by fusion with chlorate of potass. When it was fused with carbonate of potass, and the product of fusion treated with muriatic acid, evaporated to dryness, and redissolved in water, according to the ordinary method for the separation of silica from silicate of potass, an insoluble yellowish-white residue remained, amounting in weight to a fraction of that of the original brown powder. Had the latter been silicon, it should have yielded by this treatment twice its weight of silica.* In one recorded experiment, we find that 4·5 grs. of the brown powder gave, after the treatment described, 0·4 grs. of insoluble residue; in another, 2·1 grs. gave 0·3 grs; and the proportion generally obtained was, as nearly as possible, a tenth of the original powder.

* The atomic weight of silicon, according to Berzelius, is 22·22; that of silica 46·22.

In our earlier experiments, we supposed we had erred in not heating the cyanide sufficiently. But we were not able, by any alteration in the mode of heating, to procure a substance more resembling silicon than the one we have described.

We have now cause to regret that we did not analyse the insoluble residue of these processes more carefully. It was only cursorily examined at the time of our meeting with it, as we were led to suppose it not silica, from its softness and want of grittiness. But we have since had frequent occasion to notice that silica, obtained from its native compounds, may be quite destitute of this property. The body in question was not any compound of lead, for hydrosulphuret of ammonia did not blacken it: it was quite insoluble in aqua regia: bore the full blast of the blowpipe without diminution in bulk; and fused with carbonate of soda into a globule of a greyish colour. On the whole, it seems to us in the highest degree probable that it was silica, but we cannot confidently affirm that it was.

The compound, or mixture of cyanide and chloride of lead, already referred to, which did not fuse, yielded on heating, a bluish-grey powder, which dissolved entirely in very dilute nitric acid, without the separation of any brown powder. When the lead was precipitated by muriatic acid, the chloride removed by filtration, the liquid evaporated to dryness, and the residue plentifully washed with boiling water, a trace of silica remained. The fusible precipitate was not subjected to a particular examination.

Disappointed in the cyanide of lead, we turned our attention to the similar salts of other metals; for it is within the scope of Dr Brown's hypothesis, that every cyanide which can be converted into a paracyanide, may have its carbon transmuted into silicon.

We rejected, after several trials, the cyanides of zinc and copper, and turned our attention to one of the best known of all the compounds of this class, the cyanide of silver. This salt has the great advantage of being quite stable, and easily prepared pure in any quantity. But, on the other hand, it gives off cyanogen at so low a temperature, that it cannot be entirely converted into paracyanide by heat in open tubes; and the variable proportion of metallic silver which is thus mingled with the product, entirely unfits it for yielding quantitative results. By adopting a suggestion of Dr Brown's, however, and heating the cyanide in glass tubes sealed at both ends, or in brass tubes closed by screw stoppers, we prevented the evolution of cyanogen, which otherwise would have occurred.

Pure dry cyanide of silver heated under pressure in this way over a gas flame, slowly changed into a brown powder, which, if the heat were not raised too high, did not fuse or adhere to the glass. This powder, according to Dr Brown's views, is either a combination of two atoms of silver, and one of paracyanogen, a sub or diparacyanide of silver, or a mixture of silver and paracyanogen, in these proportions.

Where glass was used, when the tube was filed across, a slight explosion and

rush of air took place, shewing that some cyanogen, or nitrogen, or other gas, had been evolved in spite of the pressure. But the loss in this way was so slight, that quantities, such as 40 grs., did not lose more than 0·1 gr. by the heating.

The brown powder (diparacyanide of silver?) obtained in this way, was fused with pure carbonate of potass in a platina crucible;* and the product of fusion dissolved in water, filtered, supersaturated with muriatic acid, evaporated to dryness, and redissolved in water. A white, gritty, insoluble powder remained, having all the properties of silica. But we shall return, after recounting the different processes we tried, to the consideration of the proofs by which we satisfied ourselves that what we term silica was really so. The quantity of silica was in every case much less than it should have been, had the whole carbon of the cyanide of silver been transmuted into silicon;† and hydrocyanic acid was evolved abundantly on the addition of the muriatic acid, shewing that much of the paracyanogen had escaped the transformation of which, *ex hypothesi*, it is susceptible, and had decomposed the carbonate of potass, forming cyanide of potassium.

The liability of the sealed glass tubes to explode, which two-thirds of them did, and the difficulty of regulating the temperature where metallic ones were employed, were serious objections to this process. Care and attention, however, might have enabled us to overcome these, but the formation of cyanide of potassium, in variable quantity, destroyed the possibility of obtaining proofs that the silicon had been yielded by the carbon. On this account, accordingly, we relinquished the cyanide of silver.

From the cyanides we passed to the ferrocyanides, in the expectation that, in imitation of Dr Brown, we should be able to obtain silica in such abundance as to disprove, by its very quantity, the objections that have been made to his conclusions, on the ground that the silica was derived from the vessels or reagents made use of.

No process for this purpose could be simpler than that given by Dr Brown for producing silica from the ferrocyanide of potassium. That salt, thoroughly dried, was to be reduced to powder, and mixed with three or four times its weight of pure carbonate of potass. The mixture was then to be "ignited in a shut crucible, made of hammered iron, during the space of four hours, and at a full white heat."‡ The saline mass obtained at the end of the heating, when treated with muriatic acid, &c., as if it were soluble silicate of potass, was found by Dr Brown to yield silica so abundantly, that in one experiment 5 grs. were procured from

* A platina crucible was used at the risk of destroying it by the fusion of the silver; porcelain being objectionable as containing silica.

† In the only experiment of which the exact quantities are recorded, 17·9 grs. of the brown powder (diparacyanide of silver?) gave of silica 0·5 gr.

‡ Transactions of the Royal Society of Edinburgh, vol. xv. p. 244.

30 grs. of the ferrocyanide; and the collected product of several others gave 1240 grs. of silica from 9334 grs. of the prussiate.*

We repeated this experiment many times, both in platina and iron crucibles, but never could obtain more than traces of silica, although we employed large quantities of material. The proportion of silica procured in our early trials was so insignificant, that we did not attempt to ascertain its quantity. But in a recent experiment, where 480 grs. of ferrocyanide of potassium were fused with 3 ounces of bicarbonate of soda (which in this case was substituted for potass on account of its purity), the quantity of silica obtained was only 0·3 gr., or less than a tenth of a grain for each ounce of material, soda included.

We tried a single experiment with the variety of Prussian blue, formed by exposing to the air and washing the salt left in the retorts, when ferrocyanide of potassium is decomposed by sulphuric acid in the process for the preparation of prussic acid. 619 grs. of this body were fused with three times their weight of carbonate of soda, and treated otherwise like the ferrocyanide of potassium. At the stage of the process where silica should have appeared, we obtained 12 grs. of a soft yellowish-white insoluble powder, which, in the belief that it would prove to be silica, we ignited for two hours in a platina crucible. It was not silica, however, but probably some organic compound; for the crucible, on being opened, was found quite empty.

From the ferrocyanide of lead, on which we made several experiments, we obtained traces of silica.

From the ferrocyanide of copper fused with alkaline carbonates, and treated otherwise like the similar salt of potassium, we obtained a larger amount of silica than from any other salt of the same class. From 266 grains of a parcel of this salt, prepared by one of us with great care, and fused with three times its weight of white flux, we obtained 4·2 grs. of silica; and other portions of the same specimen yielded us considerable, though variable, quantities.

These experiments were made early in the winter, at a time when we were only anxious to ascertain if silica could be procured from such materials. In many of them, accordingly, unweighed quantities of the ferrocyanide and the alkaline carbonate were employed. As, however, we preserved nearly all the silica we procured in this way, and are able to form an estimate of the quantity of the ferrocyanide made use of, from the space it occupied in a bottle of known capacity, we can make a tolerably near approximation to the weight we must have employed. On the whole, we are within the truth when we say, that from about three ounces of ferrocyanide of copper, fused with twelve of alkaline carbonate, we obtained ten grains of silica. The carbonate was ascertained by very careful analyses to be quite free from silica, except in one case, where, owing to an accidental oversight,

* Transactions of the Royal Society of Edinburgh, vol. xv. p. 245.

a parcel of carbonate of potass was employed which had not been tested. This remark applies to a single experiment, all the others having been made with carbonate of certified purity.

Encouraged by our success, comparative as it was, with the ferrocyanide of copper, and anxious to procure quantitative results as to the amount of silica yielded, we recently prepared several ounces of this salt, and washed and dried it with great care. On fusing it with carbonate of soda, however, we obtained only traces of silica. Four specimens of the ferrocyanide were employed, two prepared from ferrocyanide of potassium and sulphate of copper, and two from the former salt and the nitrate of copper. All were washed and dried, and otherwise prepared in the same way, and with the same care.

The following are the quantitative results we obtained :—

Ferrocyanide of Copper	(from Nitrate), grs.	318·7, fused with Bicarbonate of Soda, grs.	1200,	gave of Silica, gr.	0·3.
Do.	do.	325·5	do.	1200	do.	1·3.
Do.	(from Sulphate),	303.5	do.	1200	do.	1·0.
Do.	do.	280·6	do.	1000	do.	1·3.
Do.	do.	100·	do.	200	do.	0·7.

From 1328·3 grains of ferrocyanide of copper, we thus obtained only 4·6 grains of silica.

The last experiments we have to record, are those performed with paracyanogen. The paracyanogen employed was prepared by heating the cyanide of mercury in iron tubes closed by screw stoppers, according to the process described by Dr Brown in his paper in the Society's Transactions for 1840.*

Paracyanogen prepared in this way, when freed from the cyanogen which, according to his view, was mechanically condensed in it, was found by him to be entirely converted into silicon and nitrogen by different modes of treatment. The simplest of all these was to heat the paracyanogen alone out of contact with air, when its nitrogen passed away in the gaseous form, and its carbon became silicon. In two remarkable experiments recorded by Dr Brown,† paracyanogen was found to give off a weight of nitrogen less by 8 per cent. than it should have given; and the weight of silicon left behind was "conformable to the combining proportions of paracyanogen and carbon."

Satisfied that, could we shew a weight of silicon equivalent to that of the carbon, and one of nitrogen only 8 per cent. less than it should by calculation have been, we should convince every chemist of the exceeding probability, if not the absolute truth of the proposition, that carbon is transmutable into silicon, we carefully repeated this experiment some ten or twelve times.

We were foiled, however, on the very threshold, by finding that paracyanogen when heated alone, instead of yielding only cyanogen and nitrogen, as it had done to Dr Brown, gave off also carbonic acid and carbonic oxide.

* Transactions of the Royal Society of Edinburgh, vol. xv. p. 174.

† Ibid. pp. 238, 239.

We heated the paracyanogen in tubes of glass, of brass, and of iron, alone and mixed with spongy platina, and at various temperatures from 600° up to a white heat. The tubes were of small bore, and the included air was expelled as much as possible by a gentle heat, before they were raised to the temperature at which the paracyanogen decomposed. In spite, however, of every precaution calculated to prevent the paracyanogen coming in contact with any source of oxygen, we invariably procured carbonic oxide and carbonic acid.

The black matter left after the gases had been evolved, which should have been silicon or a siliciuret, in all the cases where it was examined, except in one very remarkable one which will be mentioned hereafter, was found to diminish rapidly in bulk and weight before the blowpipe, and to consist chiefly of carbon.

Later researches have shewn us, that it was useless to look for quantitative results from so impure and variable a body as crude paracyanogen. As it comes from the iron tubes, it contains, according to Dr Brown, absorbed cyanogen, and frequently also silicon. We have always found metallic mercury present in a state of very fine division, a soluble salt of mercury (which is not the cyanide, but, perhaps, as Professor Johnston has suggested, the cyanate), and often also carbon.

We have lately attempted to purify the paracyanogen so procured, by boiling it with water for several hours to aggregate the mercury; washing it on a filter to remove the soluble salt of that metal; and, according to the process given by Dr Brown, for depriving paracyanogen of its absorbed cyanogen,* afterwards boiling it with carbonate of potass and water. From paracyanogen purified in this way, and heated in brass and iron tubes, we procured nitrogen, carbonic acid, and carbonic oxide; and the residue (carbon) was almost entirely dissipated before the blowpipe. In one experiment, 3·3 grains of crude paracyanogen heated in an iron tube, gave off 5·75 cubic inches of gases, of which 66 per cent. was absorbed by potass; the remainder burned with the blue flame of carbonic oxide, and was not farther examined as to the presence of nitrogen. The potass, when examined by Scheele's test, gave much Prussian blue, shewing that the absorbed gas was chiefly cyanogen. In another experiment 3 grs. of purified paracyanogen gave 6 cubic inches of gas, whereof potassa absorbed 20 per cent., which, on examination, proved to be carbonic acid with a trace of cyanogen. The residual gas, which burned with the characteristic flame of carbonic oxide, was mingled with an equal volume of oxygen, and exploded by the electric spark. After the carbonic acid had been withdrawn, and phosphorus had ceased to cause contraction, there remained 65 per cent. of nitrogen, shewing, of course, 15 per cent. of carbonic oxide.

Unable to confirm Dr Brown's results with paracyanogen heated alone, we turned to his experiments on the fusion of that body with carbonate of potass. Crude paracyanogen, when fused with that substance in a closed platina crucible

* Trans. of Royal Soc., vol. xv. p. 168.

for two hours, at a full white heat, was found by him to yield silica, when treated with muriatic acid in the manner already referred to. "The weight of silicon was never less than an 11th, and never more than a 12th, under the calculable weight of the constituent carbon."*

We have repeated this experiment many times, both with crude and purified paracyanogen, and in the greater number of cases have obtained silica. In several trials, however, and even with paracyanogen prepared by that gentleman himself, we obtained no silica; in others, the quantity from paracyanogen of our own preparing was very small; and, in all, it was much more than an 11th under the weight of the constituent carbon. The exact quantities of silicon we procured are given in the subjoined table, from which it will be seen that, whereas the proportion of silicon obtained by Dr Brown was between 91 and 92 per cent. of the weight of the carbon, the proportion we obtained, even from purified paracyanogen, was never more than 15, and sometimes less than 1 per cent.

Paracyanogen.	Gave of Silica.	Containing Silicon equivalent to	Should have given of Silicon.
4·8 grs. crude,	0·3 grain,		
Do.	0·1 ...		
3 purified,	A trace,	less than one per cent.	2·9
2·5 Do.	Do.	Do.	2·4
3 Do.	0·3 grain,	10 per cent.	2·9
3 Do.	0·45 ...	15 ...	2·9

We endeavoured to make this a quantitative process, by collecting and examining the gases evolved during fusion. According to Dr Brown's views, paracyanogen, when fused with carbonate of potash, should give off all its nitrogen in the gaseous form, and have all its carbon transmuted into silicon. The latter should then become silica at the expense of the carbonic acid, reduced thereby to carbonic oxide; while the silica and caustic potash contemporaneously evolved should unite to form silicate of potash. There should thus be calculable volumes of carbonic oxide and nitrogen, as well as a calculable weight of silica produced. And all three should agree (within the limits of error in experiment) with the anticipated numbers, if a perfect transformation of paracyanogen into silicon and nitrogen occurred.

We made this experiment several times, but always found carbonic oxide and

* Transactions of the Royal Society, vol. xv. p. 236. The numbers, we suppose, have here been accidentally inverted, an 11th being more than a 12th.

nitrogen; and the quantity of the two last was quite at variance with the volumes they should have given, as the succeeding table will shew:—

Paracyanogen.	Gave of Carbonic Oxide and Nitrogen.	Whereof Carbonic Oxide.	Nitrogen.
4·8 grs. crude,	5·5 cubic inches.	66 per cent.	33 per cent.
3 ...	3 ...	71 ...	29 ...
3 purified,	2·75 ...	68 ...	32 ...

We have reserved to the last the account of one remarkable experiment with paracyanogen heated alone in an iron crucible. Dr Brown, described in his second communication to the Royal Society, an experiment, in which a quantity of paracyanogen closely shut up in a Berlin crucible, and kept twenty days in a sand bath, at a temperature of about 800° or 900° F., became converted into silicon.* In imitation of this experiment, we enclosed 10·5 grains of crude paracyanogen in a Berlin crucible, and kept it at the temperature prescribed for four days. An accident led to the crucible being opened at the end of this time, when the original quantity was found diminished to 1·8 gr. This residue was of a light brown colour, much lighter than paracyanogen, and gritty. It was fused with carbonate of soda, and treated as if for silica, but gave only a trace of insoluble matter.

This experiment was repeated with a small crucible of malleable iron weighing about 200 grains, containing 18·5 grains of crude paracyanogen. The lid being luted on, and the whole crucible coated with stucco, it was placed over an argand gas flame, and heated continuously for three days. At the end of this period it was opened, and found to contain 4·2 grains of a nut-brown soft powder. 3·9 grains of this powder were fused with carbonate of potass, and the product treated with muriatic acid as if it were silicate of potass. 10·4 grains of a reddish-brown powder were obtained, which, when boiled with muriatic acid, washed and ignited, left 8·4 grains of pure white silica. Had the original brown powder been silicon, and by fusion with the carbonate become silica, it should have given 8·11 instead of 8·4 grains of the latter. There is thus an excess of 0·29 grain, or 3·6 per cent. of silica. In spite of this considerable excess, we believe that few will refuse to acknowledge that the original body was silicon.

Had we been aware at the time of making this experiment, that our subsequent trials in other directions would prove so unsatisfactory as they have done, we should have repeated it many times. But, as the object of our inquiry was to ascertain whether carbon was transmutable into silicon or not, we left unrepeated

* Transactions of Royal Society, vol. xv. p. 233.

an experiment which, however often successful, could not have established the truth of that proposition. We did, however, repeat it once with 10 grains of paracyanogen, which were heated for three days in a small iron crucible. But on opening it at the end of that time it was found quite empty.*

Throughout our paper, we have taken for granted that what we have named silica was really so. The body to which we give this name was a white powder, which was occasionally soft when first produced, but invariably became gritty when exposed for some time to a high temperature. In several cases it was as sharp and gritty as builders' sand. It could be boiled for hours in aqua regia, or kept at a white heat in a platina crucible for a similar period, without a perceptible loss in weight. It bore the full blast of the blowpipe without change; and when fused before it with carbonate of soda, formed a transparent bead. It dissolved in alkaline carbonates with effervescence, and could be recovered from them by muriatic acid unchanged in all its properties. Several of our specimens were fused in this way again and again, without varying in their deportment from true silica. These characters would suffice to prove the body possessing them silica, and completely distinguish it from hydromellonic acid, or any other organic compound, which might be supposed to be formed by the reaction of compounds of cyanogen on alkaline carbonates.

We were unwilling, however, to omit testing our supposed silica by its power of forming fluosilicic acid when distilled with oil of vitriol and fluor spar. With no little trouble we succeeded in providing ourselves with pure fluor, which was ascertained, by repeated careful analyses, to be quite free from silica. The body we were testing we distilled with the spar in a small leaden alembic, with its beak dipping into water. The characteristic membranous tubes of silica formed rapidly, and sank in gelatinous flakes to the bottom. After so decisive tests, we are quite certain we were not mistaken in believing we had obtained silica.

We may farther mention, that potassium heated with what we may now term silica, liberated a black powder quite undistinguishable from the body it separates from common silica. We may also add, that we ascertained the specific gravity of the silica we obtained in our earliest experiments with ferrocyanide of copper. It was 2·25—that given in the text books is 2·69. The difference is probably not greater than that between different specimens of ordinary silica.

In conclusion, we need scarcely say, that we have been unable to supply any proof of the transmutability of carbon into silicon. The utmost we may have done,

* When our paper was read, another repetition of this experiment was in progress, which has since been completed. Twenty grains of purified paracyanogen were heated in an iron crucible for three days. On examination, the powder was found so little changed in bulk and colour, that the lid of the crucible was replaced, and the heating continued for three days more. A very pale brown powder was left, amounting to 0·5 gr., which, when fused with carbonate of potass, left a trace of what appeared to be silica.

is to have proved an unequivocal anomalous appearance or production of the latter body. Our experiments, moreover, throw no light on the source of that silicon. Taking the experiment with the iron crucible as the simplest in its conditions of all those we have made, and not multiplying hypotheses unnecessarily, we shall, nevertheless, be obliged to admit as equally tenable three views of the origin of the silicon.

Paracyanogen, a compound of carbon and nitrogen, disappeared, and was replaced by silicon.

We may say with Dr Brown, that the latter came from the carbon; or with Mr Knox,* that it came from the nitrogen; or, for anything the experiment betrays to the contrary, that it came partly from both. Great difficulties lie in the way of all of these hypotheses, which we feel it quite unnecessary to discuss, unless so far as to acknowledge that Mr Knox's theory stands on a broader basis of alleged fact than either of the others, as he professes to have established his view of the relation of silicon to nitrogen both by analytic and synthetic proofs. No one, however, has repeated or confirmed his experiments.

We have, in the meanwhile, relinquished the farther trial of Dr Brown's processes, because the experience of four months' failure has satisfied us that his experiments cannot be repeated at will; that the conditions essential to their success have not been satisfactorily ascertained; and that none of his processes are sufficiently wrought out in detail, to afford the means of establishing the transmutability of carbon into silicon on quantitative grounds, the only grounds on which such a proposition can be based.

In particular, we have been deterred from farther trial, by finding that paracyanogen which, according to Dr Brown, is a "true cyanide of cyanogen, decomposed neither by heat, because its constituents are equally volatile, nor by electrolysis and reagents,"† is in reality susceptible of such a decomposition.

This is not the place or time for entering on this matter. But we may mention, that paracyanogen, purified from adhering or absorbed cyanogen with the utmost care, has been found by us to pass back or revert into cyanogen.‡ Fused with carbonate of potass, or heated with potassium, it has given cyanide of potassium abundantly. In one case where two grains of paracyanogen were heated with potassium, and precipitated by nitrate of silver, acidulated with nitric acid, they gave 4·4 grs. of cyanide of silver, containing of cyanogen 0·86 gr., and there re-

* An abstract of Mr Knox's paper, which has not, so far as we know, yet been published, will be found in the London and Edinburgh Philosophical Magazine for 1843.

† Trans. of Royal Soc., vol. xv. p. 175.

‡ We offer no opinion as to the constitution of paracyanogen. By the substance which we so designate, we mean the black powder obtained by heating cyanide of mercury; and in speaking of its reversion into cyanogen, we purposely use the language of Dr Brown, who supposes it to be a combination of two atoms of cyanogen.

mained of insoluble matter, which was found to be carbon, 0·4 = 0·88 gr. cyanogen, making together 1·74 gr. of the original weight of paracyanogen.*

This possibility of paracyanogen passing back into cyanogen, strikes at the root of all the processes with paracyanogen, cyanides, ferrocyanides, &c., where fusion with an alkali is prescribed; and explains the uniform appearance of hydrocyanic acid, when acids were added to the products of fusion. So long as much of the carbon and nitrogen of the paracyanogen is spent in forming cyanide of potassium, quantitative proofs of the conversion of carbon into silicon, even should it occur, cannot be secured by any fusion process.

Whether or not this remark applies also to paracyanogen heated alone we cannot decide. By simply heating it, we have never been able to resolve it entirely into cyanogen, nor, so far as we know, has any chemist. But it may resolve itself into nitrogen and carbon, which would as effectually interfere with the end in view.

* This fact has been already noticed by Professor JOHNSTON, Transactions of the Royal Soc. of Edinburgh, vol. xiv. p. 37; and by Messrs SMITH and BRETT, London and Edinburgh Philosophical Magazine, vol. xx. p. 29; but we have mentioned our experiment particularly, because it was made with paracyanogen purified in the way already mentioned, and the quantity of cyanide of silver produced was ascertained.

XXXVII.—*On the Development, Structure, and Economy of the Acephalocysts of Authors; with an Account of the Natural Analogies of the Entozoa in General.* By Harry D. S. Goodsir, Conservator of the Museum of the Royal College of Surgeons in Edinburgh.

(Read 1st April 1844.)

The Acephalocyst, or simple Hydatid, is composed of a vesicle, containing a watery fluid, which, in the normal state of the creature, is quite transparent and colourless (Pl. XIV., fig. 1.) The internal surface of the vesicle is generally studded with numerous cells of various sizes, many of which are found detached and floating loose in the fluid contained in the vesicle. These are the young Hydatids.* Their development will be described when we come to that portion of the present paper, which has been set apart for that purpose.

Hydatids, like the other entozoa, are incapable of sustaining an independent existence, although, as independent creatures, they are similar to the other species of entozoa. They infest all parts of the body, and are commonly lodged in cavities containing fluid in which the hydatids float.

From two circumstances, the true nature of hydatids has been very much misunderstood. The first, depending on imperfect observation, has arisen from specific characters being unattended to, and consequently, from external resemblance alone, these animals have been erroneously classed with other and very different pathological appearances, such as serous cysts.† The second cause of misunderstanding has arisen from the limited number of known species preventing naturalists from arriving at any general views with regard to their proper relations. Both of these circumstances have doubtless retarded our progress towards proper conclusions relative to their true nature, and, at the same time, afforded reasons in support of the views advocated by those who denied the animality of the Acephalocyst.

I am indebted to the kind attention of Dr Gairdner, for an opportunity of examining a species of hydatid, which appears to have been hitherto undescribed, the study of which has enabled me to detect several important circumstances relative to the economy of the Acephalocysts, and also to trace out generally the

* *Vide* Monro's Morbid Anatomy of the Gullet, Stomach, and Intestines, p. 193; also Dr John Hunter's paper, in the 1st vol. of the Transactions of a Society in London for the Advancement of Medical and Chirurgical Knowledge.

† Hodgkin. Transactions Medico-Chirurgical Soc. London, vol. xv. p. 266. Hodgkin. Lectures on Morbid Anatomy, vol. i. p. 180, Lecture VII.

analogies of the Entozoa. The patient from which this particular form of hydatid was obtained, had been labouring for some time under great distension of the abdomen. After death the cavity was found to be full of them, containing from three to four gallons. On a superficial examination, they appeared to float free in the fluid of the peritoneum; but, on further dissection, they were found to be attached to the lining membrane of the cavity, by narrow pedicles or more extended bases. They were globular, of various sizes, from that of a pin-head up to a small apple, and of a bright-straw colour, resembling in appearance the yolks of eggs.* Their external surfaces were rough, as if covered by a false membrane. The membrane, however, was ultimately discovered to have been produced, not by any inflammatory action, originating in the presence of the hydatids, (as was supposed), but by the animal itself. When observed attentively with the naked eye, its surface was found to be roughened in consequence of a great number of striæ, disposed in a regular manner, so as to form small irregular interspaces of an angular shape. It covered closely all the hydatids up to the roots of the pedicles in those which were insulated, and dipped deeply between those which were pressed together. It also spread over the peritoneal surface to a short distance from the general mass. Under this latter part, all the hydatids were generally small but became enlarged as they approached the parent group.† (Plate XV., fig. 2.)

On two portions of peritoneum, to which neither the membrane nor hydatids had yet extended, it was observed that the membrane became thinner and thinner as it receded from the original stock. (Pl. XIV., fig. 2 B.)

When observed under a high power, the membrane was found to be covered at short intervals by numerous disks of various sizes. Larger disks, however, were occasionally seen with smaller ones on their surfaces, and numerous tubuli which arose by open mouths from the edges of each of them were ramified freely over and throughout the membrane (Pl. XIV., fig. 5.) Several of these stomata seemed to open into one tube, and to be arranged round the aperture of the tube.‡ (Pl. XIV., fig. 5 C.) This occurred most frequently in the larger disks, and always upon the edges of the same; but in those of a smaller size, three or four small tubes proceeded from the disk, all of which apparently opened by one mouth only, the mouths being situated round its edge. (Pl. XIV., fig. 7, B.) A large disk, therefore, might be said to represent a congeries of smaller ones, arranged together in a particular form. The tube at the part near to its diskoid origin, was always of

* A cluster of these Hydatids, where there were large and small ones grouped together, resembled very much the ovaria of the common fowl when in a state of activity.

† The disease had proceeded to such an extent, and the abdomen was so distended, that this fact could only be observed in two places.

‡ In cases like that mentioned in the text, what, on a superficial examination, appeared to be one tube only, was afterwards found to be a fascicle of smaller tubes.

a considerable size, but decreased very much as it gave off smaller branches in its outward course. Sometimes a tube of considerable magnitude, or rather a fasciculus of tubes, was seen connecting two neighbouring disks.

Immediately underneath that already described, was another membrane of a much more delicate texture. (Pl. XV., fig. 5, G.) It was connected with the former by means of condensed cellular texture, and sent off numerous very fine septa, which traversed and intersected the body of the Hydatid, for the purpose, apparently, of rendering it support. The body of the animal itself was composed of a homogeneous gelatinous mass, of the colour and consistence of calf's-foot jelly. The open stomata and tubes, which were seen in the external membrane, appeared to be the organs of nutrition. They could not, however, be traced into the gelatinous mass, so that, probably, they only existed in that one membrane.

The mode of generation and of development in these animals is very simple. When the internal surface of the vesicle of the common Hydatid is examined, it will be found studded all over with numerous smaller vesicles of different sizes. (Plates XIV. and XV., figs. 6 and 4.) These, as already stated, are young Hydatids. A simple cell makes its appearance under the internal lining membrane of the parent vesicle, which gradually increases in size, without any cellular development whatever, but by dilatation alone from the increase of the quantity of matter within it. (Pl. XV., fig. 6.) In this way it increases to such a size as to burst through the internal membrane, escape into the cavity of the parent vesicle, and thus become an independent creature. This is the reason why we find the internal surface of the vesicle so frequently broken up. The finest example of the kind which I have seen, is one in the possession of Dr Monro, and which he has been so kind as allow me to examine. A very fine drawing of this may be seen in his work already referred to, on the Morbid Anatomy of the Gullet, Stomach, and Intestines, at Pl. IV., and fig. 18.

When a small portion of the external or tubular membrane of the new form of Hydatid was placed under a powerful glass, its internal surface was found to be studded with a number of small shining bodies or vesicles. In general, these vesicles were compound, containing from one to four young cells in their interior (Pl. XIV., fig. 7, FFF); which cells, however, were occasionally seen free and detached from the parent one. (Pl. XV., fig. 3, C.) I considered them to be the gemmules of this Hydatid, which, like the other Acephalocystic Entozoa, is gemmiparous.

The tubular membrane, as it spreads over the healthy peritoneum, and apparently after it has reached a certain stage of growth, developes the cells, just described, from its attached surface, and invariably from spots in the neighbourhood of the large tubes. (Pl. XIV., fig. 9, AA.) These gemmules enlarge without any apparent cellular development; but, like the simple Acephalocyst, by dilatation from the addition of new matter within the cell. It varies, however, from

the former, inasmuch as the simple Hydatid was from its first appearance composed of one cell only; whereas, in this species, and particularly in this mode of its growth, the cells, when first observed, contain a number of younger ones within them, all of which afterwards become the separate and individual vesicle. (Pl. XV. fig. 2, D G.) During this process, the tubular membrane increases in density around and in the neighbourhood of the gemmules, owing to the increased number of tubes necessary for their nourishment. It will be observed, that the young original gemmule of this species resembles in its structure and functions, the adult simple Acephalocyst.

This species of Entozoon has two modes of propagation, viz., the one which we have just described, for the purpose of increasing the size and extent of its own individual group; and another, for the purpose of extending the species to uninfested portions of the infested animal. In the last, the mode of propagation would appear to proceed in the following manner:—The cells which have been already described, in the preceding page, as occasionally seen detached from the parent cell, and floating free in the gelatinous mass of the body of the parent Hydatid, reach the healthy tissues which lie at some distance from the general parasitic mass, (Pl. XV., fig. 2, C, D, and fig. 3, C), by some means which I have been hitherto unable to detect. In general, they are no deeper than the subserous tissue; but when this has been already occupied, they are found much deeper, where, as they increase in size, they tend always towards the surface of the infested cavity, and at length burst from their confinement, adhering, at the same time, to the bottom of their former, containing cellules by pedicles. (Pl. XV., fig. 2, F.) In this manner a peculiar honey-comb appearance was produced, (Pl. XV., fig. 5, C and D), by the breaking up of the tissues, which became much more apparent when the Hydatids were removed from the affected surface.

For the purpose of illustrating the series as completely as possible, I will now describe the characters and mode of development of another form of Cystic Entozoon. The Cænurus cerebralis is generally met with in the brain of the sheep, and occasionally in the other ruminants. It consists of a vesicle full of a transparent watery fluid. The cyst is double, the internal layer is very delicate, while the external is much stronger, acquiring additional strength and thickness, in consequence of a great number of striæ, which run through it in all directions, and presenting, when seen under the microscope, all the characters, with the exception of the disks, of the tubular membrane of the new Acephalocyst, although not so strongly marked. Unlike the simple Hydatid, and the parasite already described, the Cænurus possesses numerous heads (Pl. XVI., fig. 13), arising at right angles from its external surface in groups, but apparently without any regularity. Each head consists of a pedicle (Pl. XVI., fig. 13, C, H), and head proper (Pl. XVI. fig. 3, A), and is covered by a thin layer of the external membrane of the vesicle. (Pl. XVI. fig. 13, E.) The head proper, and the pedicle,

are separated by a constriction, across which a diaphragm stretches (Pl. XVI., fig. 13, C), formed of two layers, of very fine cellular substance, derived from, or continuous with, the cellular substance, and forming the walls of the head and pedicle. This cellular substance is confined between two layers of membrane, namely, that already described as derived from the external membrane of the vesicle, and another which forms the internal cavitary surface of the pedicle. (Pl. XVI., fig. 13, F.)

The head is armed superiorly with a double circle of long bent teeth (Pl. XVI., fig. 12, A), which are barbed on one edge, and arise from one common disk. Around this double coronet of teeth, and on the sides of the head, are four transversely oval suckers, which are surrounded by two concentric bands. (Pl. XVI., fig. 12, B.)

The pedicle to which this head is attached contains layers of gemmules, by which this animal propagates. (Pl. XVI., fig. 13, G H.) I have been unable to detect any organ by which these gemmules pass off from the pedicle to the place where they are developed, *i. e.*, between the layers of the cyst of the parent.

In many cases which have come under my observation, young heads have been observed sprouting from the side of the parent pedicle. In this case, however, the cells from which these young heads derived their origin were precocious; for in general the young cell never put on an active character till it reached a proper nidus between the layers of the parent vesicle, which was generally near the base of the parent pedicle, but sometimes the nidus was at a greater distance. These circumstances would lead us strongly to suspect that there is no efferent vessels for conveying the young gemmules out of the pedicle. (Pl. XVI., fig. 1.)

The gemmule, in its earliest stage, consists of a germinal vesicle (C), containing a germinal spot (D), a yelk with its proper membrane (B), and a thin layer of albumen, enclosed in a strong covering or shell (A.)* After it has escaped from the parent, that is, after it has left the pedicle, the development commences.

During the second stage, the nucleus has increased very much in size (D).

During the third stage the nucleus has become nodulated (Pl. XVI., fig. 3, D), much larger, and a clear central space, which was observed before, has also increased in size.

During the fourth stage the nodules of the nucleus have assumed the form of cells, and have become arranged in a circle round a central cell. (Pl. XVI., fig. 4, E.)

During the fifth stage the young cells have gradually increased in size, and have filled the germinal vesicle (Pl. XVI., fig. 5, C D); the central cell has also become larger, and its nucleus has acquired a clear central spot. (Pl. XVI., fig. 5, F G.)

* These different parts of the ovule are probably only analogous to those of the higher animals.

During the sixth stage the central cell has disappeared, and has apparently become the external covering of another family of young cells. (Pl. XVI., fig. 6, D G.) This little mass is nodulated, its component cellules not having yet separated from their common centre. There are now within the original germinal vesicle two concentric circles of cells, the innermost of which are scarcely formed. During the next stage the central nucleus for the third circlet of cells makes its appearance, with its accompanying clear central space (Pl. XVI., fig. 6, F G), soon becomes nodulated, and shortly throws off a third circlet of young cells.

The formation proceeds in this way for a longer or shorter period; the young cells are always formed from a central nucleus (Pl. XVI., fig. 7, A); and as they increase in size, arrange themselves in concentric circles, pushing the previous generations always farther from the centre of production. While this mode of growth is proceeding, the external membrane of the gemmule increases in size and thickness, apparently by a deposition of new matter, as the young cells are produced.

Up to this period the development has proceeded in one plane, the gemmule being a flat disk with a germinal spot in its centre. (Pl. XVI., fig. 7, A.) It is situated, as has been already stated, between the two layers of the cyst of the parent Hydatid, and has now acquired such a size as to project slightly from the surface (Pl. XVI., fig. 9) of the cyst, and to push, as it proceeded in the lateral direction, a fold of the external membrane before it; which membrane also becomes thicker in the neighbourhood of the gemmule, in consequence of the addition of new folds and fresh deposition. (Pl. XVI. fig. 11, B.)

It has already been stated that the development of the gemmule had proceeded in one plane only from one central germinal spot. In this way a base is formed for the future pedicle and head (Pl. XVI., fig. 11, B), for at this stage the gemmule projects very slightly from the surface of the Hydatid. The development now ceases in the lateral direction, and commences in a direction perpendicular to the original plane. (Pl. XVI., fig. 11, A.) For the sake of clearness, I have termed the former of these the *discoidal*, and the latter the *vertical period* of development; although each of these may be again divided into intermediate stages. Along with this change of direction additional germinal centres appear. (Pl. XVI. fig. 8, D.) I have not observed more than three of these centres, and have been unable to ascertain their actual number; probably they vary as to number, seeing that all the cells which are henceforth formed are productive, and, of course, all tend to form centres. From these additional centres fresh families of cells are constantly being produced, which again, in their turn, afford new centres; the increase of the mass being kept within certain limits apparently by the solution of the peripheral cells of each centre. It will be observed that the similarity between this development and that observed by Dr Martin Barry, in the early stage of the mammalian ovum, is very remarkable.

We have thus in the Cænuri a much more complicated mode of development than in the Acephalocysts. There must be, therefore, many forms of Cystic Entozoa which have hitherto escaped observation, for in nature all changes from one form to another are gradual.

In the course of my observations on the structure and economy of the species in the order of Cystic Entozoa, I was much struck with the analogies which existed between the various forms of the class, and those of other classes of the animal kingdom; and as I look upon these to be particularly important in establishing my views relative to the animal nature of the simple Hydatid, as well as in determining the limits of the class, I shall now submit them to the consideration of the Society.

Beginning with what I conceive to be the lowest form of Entozoon at present known, the simple Hydatid, I find in it the analogue, in its own class, of the typical forms of the Infusoria as the Volvocinæ.

Proceeding to the new form of Hydatid, which has been described in the preceding part of this paper, I consider it as the analogue of the Polypifera, and of such forms as have Alcyonidium for their type. In both we find the same general basal mass, and the same mode of nutrition, in the Hydatid, by means of disk-bearing stomata—each disk analogous to a polype—and in the Alcyonidium by tentaculated heads with stomach cavities. Both forms also are compound, the general group deriving nourishment from the individuals, and the individuals deriving support from the group; so that, in both cases, the general mass and individual stomata or polyps mutually tend to support one another. Both have two modes of propagation—one for the extension of the original group, the other for the establishment of other groups.

The Echinodermata are represented among the Entozoa in a curious and interesting manner, by the suctorial forms of that class; that is, by those forms of Entozoa which are endowed with these organs as a means of adhesion or progression, such as Distoma, Tristoma, &c. The lowest form in this suctorial tribe is the Diplozoon Paradoxum of Nordman. I am inclined to consider Diplozoon as inferior to Distoma and other suctorial forms, not from its analogies, but from this circumstance, among others, that its whole organization is double, and consequently less centralized. The Asteriadæ, among the Echinodermata, are represented in the Entozoa by Diplozoon and other similar forms, which undoubtedly exist. The Tristomæ (Pl. XV., fig. 9) are represented by the flat Echinidæ, as the Scutellæ. (Pl. XV., fig. 8.) In both the Tristoma and its Echinodermatous analogue, the Scutella, we find the disk imperfect in certain parts of its edge, indicating the remains of a more divided or asteroid condition of the body. The Distomæ are the analogues of the true Echinidæ. A starfish folded up upon itself, so that the tip of its rays meet at one central point, constitutes that form of the Echinodermata known as the Echinus. In like manner, among the En-

tozoa Diplozoon holds the same relation to Distoma. The Diplozoon has two intestinal tubes, and two mouths, one for each body. The Distoma has two intestinal tubes, and only one mouth. In like manner also, the reproductive organs are similar. It thus appears that the Distoma is only a Diplozoon folded on itself, as Echinus is an Asterias folded back. There are certainly some few points of material difference between these two animals, a circumstance we naturally look for; but these, if properly observed, must be traced to the difference of centralization. Distoma is, therefore, superior to Diplozoon, as Echinus is to Asterias, having a more centralized organization.

The Acanthocephalous Entozoa of RUDOLPHI are the analogues of the Crustacea. The Echinorhynci are typical of this group among the Entozoa. On comparing an Echinorynchus (Pl. XV., fig. 11) with a Crustacean, such as a Lernean (Pl. XV., fig. 10), the relation between them is so like that of affinity, that they were at one time grouped together in the same class. When the Lernean Crustaceans have passed their period of locomotive existence, and have become permanently fixed, their habits are exactly similar to those of the Echinorynchi, the only difference being, that the former adheres to the external, and the latter to the internal surface of the body of the infested animal. The Echinorynchi have a number of short extremities or limbs near their head, analogous to similar organs, or the atrophied limbs of the Lerneæ. There is this difference, however, between these organs in the two sets of animals, namely, that in the one they have never become developed at any period of life so as to suit the purposes of locomotion, whereas in the other, and during its early stage of existence, they were fully developed and employed as organs of prehension and progression, but have only become atrophied during the stationary or parasitic period of life.

The next, and the highest forms of Entozoa, are the Cœlelmintha, which, on examination, will be found analogous to the Annelida.

It is a remarkable circumstance, that looking on them collectively as classes —the Crustacea and Annelida are the first in the animal series—possessing a truly diæceous mode of generation. So is it with the analogues of these classes in the Entozoa, viz. the Acanthocephala and Cœlelmintha, the only groups in the class which are truly bisexual.

ANALOGIES.

INFUSORIA.

ENTOZOA.	ANALOGUES.
I. Acephalocytis simplex.	I. Volvox globator.

POLYPIFERA.

II. Diskostoma acephalocystis . . .	II. Alcyonidium.
(III. Tænia.	III. Nemertes ?)

PLATE XIV. Royal Soc. Trans. Edin., Vol. XV. p. 561.

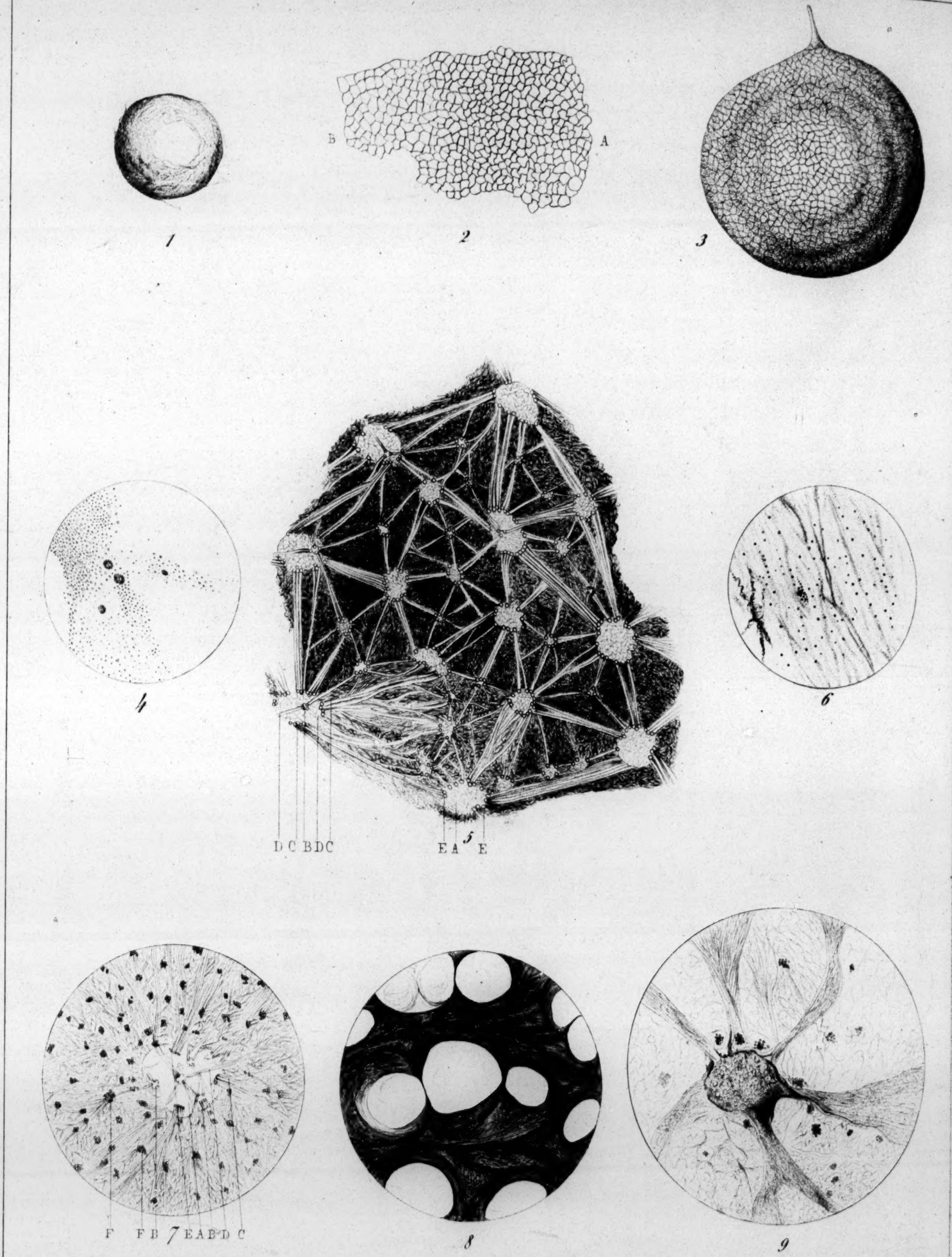


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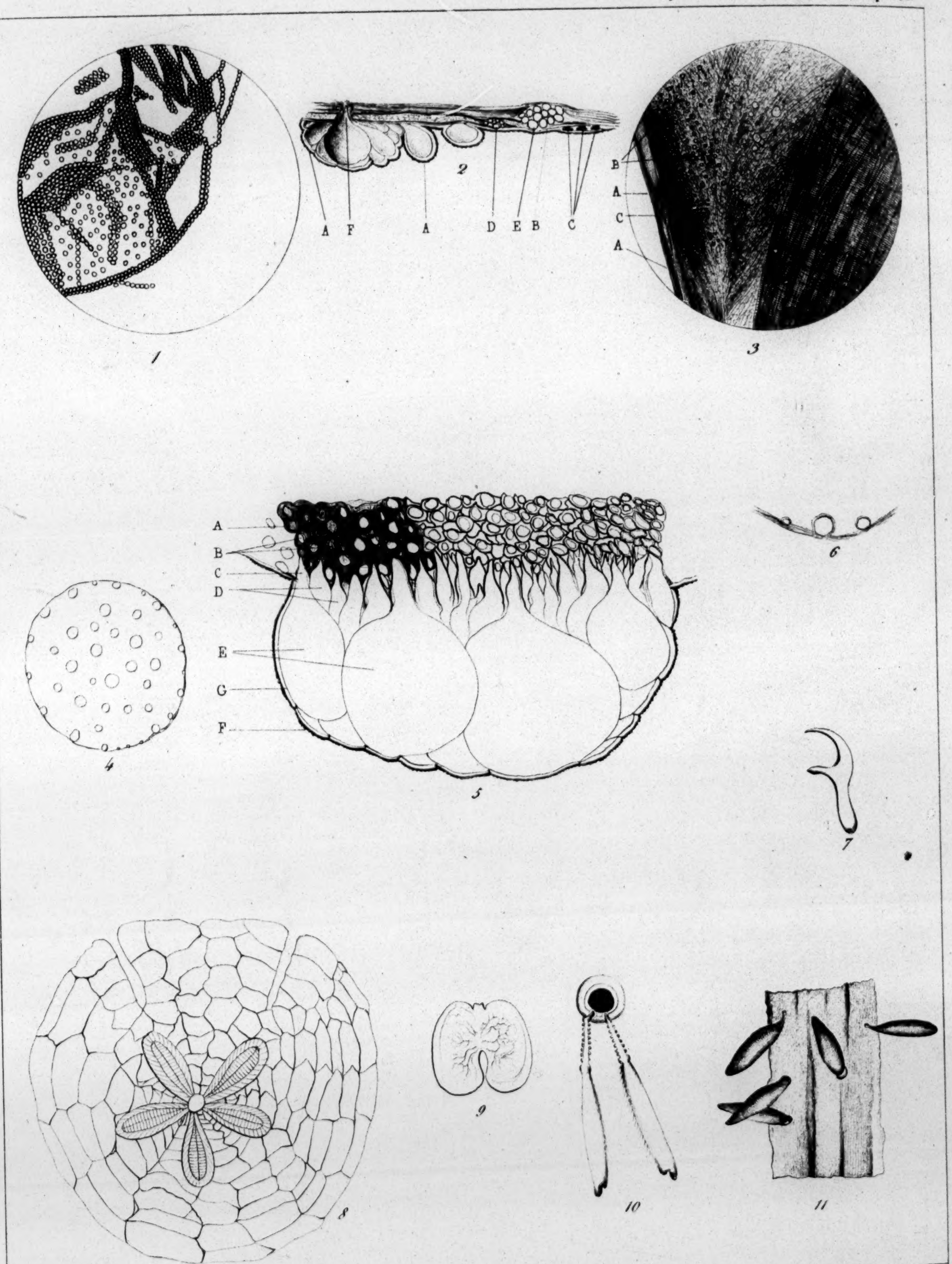
PLATE II.



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ECHINODERMATA.

ENTOZOA.		ANALOGUES.
IV. Diplozoon.		IV. Asterias.
V. Scutella.		V. Tristoma.
VI. Distoma hepaticum.	Authors. .	VI. Echinus.

CRUSTACEA.

VII. Echinorynchus.	VII. Lernea.

ANNELIDA.

VIII. Ascaris.	VIII. Lumbricus.

EXPLANATION OF THE PLATES.

PLATE XIV.

Fig. 1. Acephalocystis simplex; natural size.

Fig. 2. Portion of the external tubular membrane of Diskostoma acephalocystis, very slightly magnified. The part at A was situated near the Hydatid; the other part at B was on the peritoneum at a little distance from the Hydatid, and had not arrived at a full state of growth.

Fig. 3. Diskostoma acephalocystis of the natural size. The tubular or external membrane is drawn rather boldly for the purpose of shewing its structure.

Fig. 4. Small portion of the substance of an artheromatous tumour, very much magnified. It is stated by some authors that Hydatids, after a certain time, change into such tumours.

Fig. 5. Portion of the external tubular membrane of Diskostoma acephalocystis, very much magnified; A A large disks; B smaller disk on the surface of a larger one; C tubes running to the edge of large disk; D tubes running to the edge of smaller disk; E stomata.

Fig. 6. Internal surface of a small portion of the Acephalocystis simplex, very much magnified, to shew young cells being developed between the two membranes of the vesicle.

Fig. 7. Small portion of the external or tubular membrane of Diskostoma acephalocystis, very highly magnified. A large disk; B smaller disks upon its surface; C tubes running to the edge of large disk; D tubes running to the edge of smaller disks; E stomata; F gemmules containing young vesicles or cells.

Fig. 8. Small portion of a thin transverse section of omentum, very highly magnified, shewing the young of Diskostoma acephalocystis before they have got into the abdominal cavity.

Fig. 9. Another small portion of the tubular membrane of Diskostoma, shewing the structure of the disk.

PLATE XV.

Fig. 1. Small portion of the gelatinous substance from Diskostoma, very much magnified, shewing the manner in which the blood globules arrange themselves when effused into that part of the parasite's body.

Fig. 2. Small portion of abdominal parietes—vertical section—shewing the young gemmules of Diskostoma in various stages of growth. A A external membrane; B peritoneum; C C gemmules thrown

off between the tubular membrane and peritoneum, shewing the first mode of generation; D gemmules being developed in the subserous tissue, shewing the other mode of generation; E gemmules of the second mode farther advanced; F burst from their nidus through the peritoneum, and have obtained a covering of the tubular membrane.

Fig. 3. Thin transverse section of abdominal parietes, very much magnified, shewing blood globules, and ovules of Diskostoma in the subserous tissue. A peritoneum; B blood globules; C gemmules of solistoma.

Fig. 4. Internal surface of the vesicle of Acephalocystis simplex, with numerous young ones of various sizes being developed and thrown off.

Fig. 5. Group of Diskostoma acephalocystis dependent from the omentum. A omentum; B B young of the Diskostomata; C peritoneum, forming walls of the peduncular cavity; D D peduncle; E E Adult specimens of Diskostoma; F external membrane.

Fig. 6. Section of the vesicle of Acephalocystis simplex, very much magnified, shewing three cells or young ones lying between the membranes.

Fig 7. Tooth of Cænurus cerebralis.

Fig. 8. Scutella.

Fig. 9. Triotoma.

Fig. 10. Lerononeme, monilaris.

Fig. 11. Echinorynchus, balanarum.

PLATE XVI.

Fig. 1. Ovule of Cænurus cerebralis, first stage. A external covering or shell; B membrane of yelk; C membrane of germinal vesicle; D germinal spot; E clear central space.

Fig. 2. Second stage of ovule of Cænurus. A shell; B membrane of yelk; C germinal vesicle; D germinal spot enlarged; E clear space.

Fig. 3. Third stage of ovule. D germinal spot nodulated; E clear space enlarged.

Fig. 4. Fourth stage of ovule. D germinal spot has thrown off the nodules, which have become cells; E central cell; F germinal spot for the second generation of cells; G clear central space.

Fig. 5. Fifth stage of ovule. C primary germinal vesicle; D primary circle of cells; E central cell of primary circle of cells become larger; F its nucleus, become larger and nodulated.

Fig. 6. Sixth stage of ovule. A external covering or shell; B membrane of yelk, with probably the primary germinal vesicle, which has been distended, lying underneath it or within it; C primary circlet of cells; D central cell of primary circlet very much distended; E secondary circlet of cells, not yet finally formed; F central cell of secondary circlet; G its nucleus and accompanying clear space.

Fig. 7. Ovule of Cænurus cerebralis considerably advanced in the discoidal period of development; A central productive nucleus.

Fig. 8. One of the first stages of the vertical period of development of ovule. A primary series of stages in the discoidal period; B secondary series in the discoidal period; C cells of the vertical period; D D D productive centres; E central nucleus of one of the centres.

Fig. 9. Ovule of Canurus, very far advanced in the discoidal period of developement. The concentric circlets of cells are seen; and the central circlet near to the lower edge as it was pressed between the plates of glass, shewing that there is elevation, to a certain extent, during the latter stages of this period. A external covering of gemmule; B some of the concentric circles of young cells; C central cell of last formed circlet; D its nucleus and clear space.

PLATE XVI. Royal Soc. Trans. Edin., Vol. XV. p. 561.

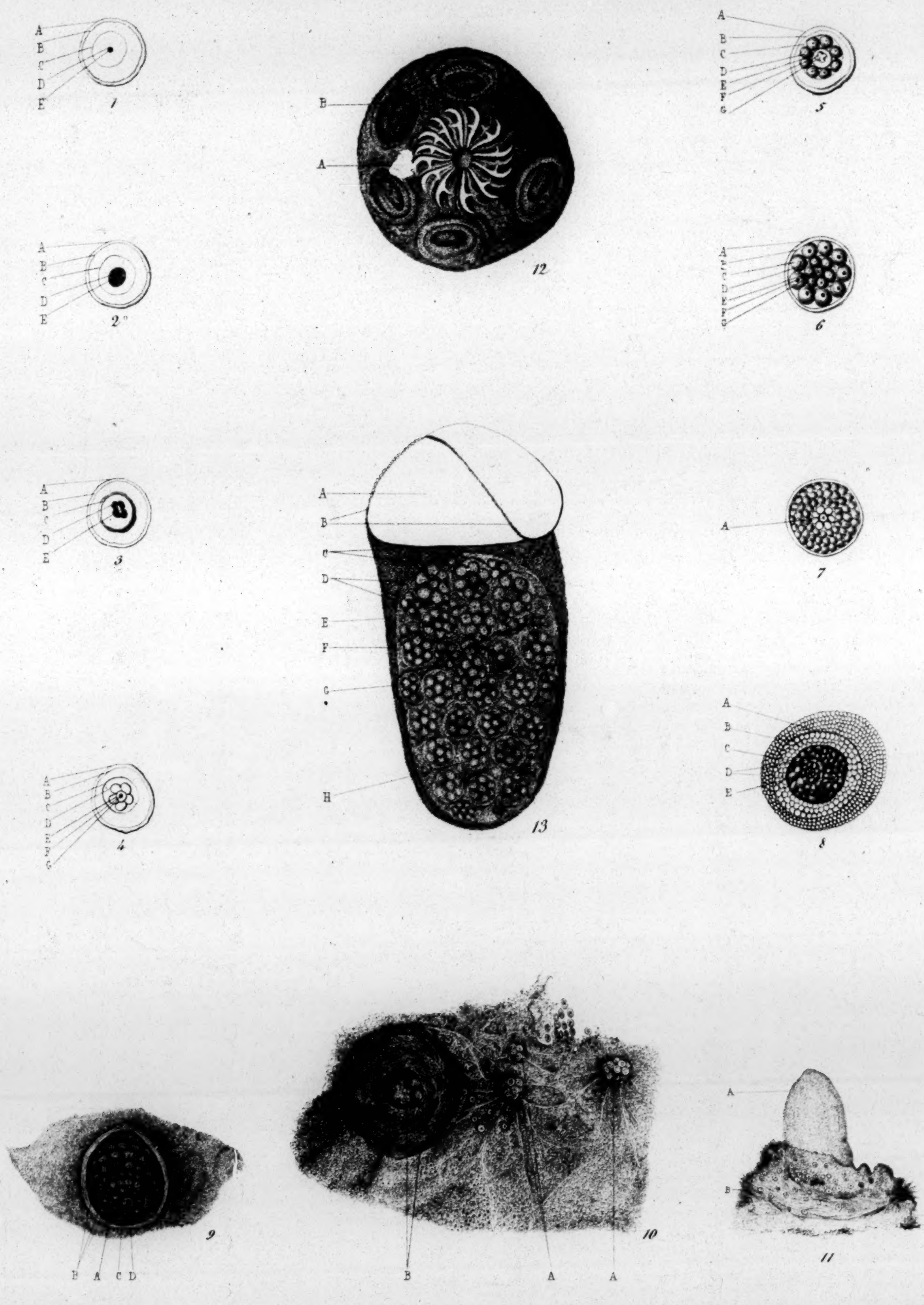

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Fig. 10. Portion of membranes of the vesicle of Cænurus, highly magnified, shewing several ovules in different stages of discoidal development; A A A ovules in the first, second, and third stages of discoidal development, shewing vessels running to them from all directions, for the purpose of supplying them with nourishment; B B another ovule much more advanced, shewing how the external membrane is pushed out by the increasing ovule, and forms a kind of base.

Fig. 11. Young pedicle and head of Cænurus cerebralis, after it has properly assumed the verticle period of growth. A The young pedicle; B the base, shewing how the external membrane is pushed together and folded upon itself.

Fig. 12. Greatly magnified view of the head of Cænurus cerebralis seen from above. A double circlet of teeth; B acetabula, or suckers, which are situated round the head. In this instance there were five of these, the general number being four.

Fig. 13. Pedicle and head of Cænurus. A head; B B cut edges of the external walls of head; C diaphragm of cellular tissue separating head from pedicle; D D cellular tissue forming walls of pedicle; E thin layer of external membrane of vesicle, which covers the head and pedicle; F internal lining membrane; G H ovaries, or ovule cells.

XXXVIII.

An Analytical Discussion of Dr Matthew Stewart's *General Theorems.* By Thomas Stephens Davies, Esq., F.R.S.L. & Ed., F.A.S., Royal Military Academy, Woolwich.

(Read April 1. 1844.)

PART FIRST.

During the century which has nearly elapsed since Dr Matthew Stewart published his *General Theorems,* many eminent geometers, both English and Foreign, have attempted to discover their solutions. Those attempts have, however, been rewarded with but limited success, and by far the most general and the most difficult of them remain still without a single published remark in the way of discussion or solution. Dr Stewart did not, as far as I know, make allusion to them himself in any of his subsequent writings, though he describes them as "of considerable use in the higher parts of mathematics;" and we learn from the preface to Mr Glenie's Demonstration of the 42d Proposition (*Tract,* 1813), that in conversation, Professor Dugald Stewart, in 1805, stated that, "he had not been able to find amongst his father's posthumous papers one word respecting them; that he had, oftener than once, observed mention made of them, in terms of admiration and respect, by some of the first mathematicians on the continent of Europe; but that as both they and the geometers in this country had tried their strength on them without success, and they had so long remained without demonstrations, he never expected to see them demonstrated." This circumstance, of neither any demonstrations nor even memoranda, on the subject being found amongst Dr Stewart's papers, is readily accounted for by Professor Playfair, in his biography of that distinguished geometer (*Edin. Trans.* vol. i. p. 74), in the description which he gives of the habits of study of Dr Stewart. "He rarely wrote down any of his investigations till it became necessary to do so for the purpose of publication. When he discovered any proposition, he would put down the enunciation with great accuracy, and on the same piece of paper would construct very neatly the figure to which it referred. To these he trusted for recalling to his mind, at any future period, the demonstration or the analysis, however complicated it might be."

It thus appears that no ground exists for our hoping to discover the means by which Dr Stewart originally investigated these theorems; whilst we cannot but be surprised at the powers of attention and invention of that mind which could carry on, without the aid of writing, such extended and complicated inquiries as are implied in the discovery of them. Neither can we be surprised if some oversights should occur in their investigations, conducted in such a manner. That oversights do exist, will, however, presently appear.

It is curious enough that, among the geometers who have written concerning these theorems, very few have seen their true character. In fact, Professor PLAYFAIR is the only one who has distinctly stated that "they are, for the most part, *porisms*," but he nowhere enters into any discussion of them, either under this or any other aspect. In one place, however, (*Ed. Rev.* vol. xvii. p. 129,) he recommends to the attention of geometers these propositions, as fitting subjects for the employment of the *trigonometrical analysis*, and speaks of "the difficulties which they will present even to those who come armed with that powerful instrument." Amongst the other authors who have spoken of these propositions as to logical character, it may be sufficient to quote two merely; but the scientific rank and high acquirements of these two will prove that very precise views are not entertained by mathematicians, even in this country, respecting these propositions. Mr BABBAGE says, that "many of them are *capable of forming, with a slight alteration in their enunciations*, the most beautiful porisms," (*Quarterly Journal of Science*, vol. i.; and Mr ELLIS affirms that, "whether they are in reality porismatic, is a question on which it would not be worth while to enter." (*Cambr. Journal*, May 1841.) Adopting SIMSON's definition, however, of the porism, it will be quite clear that a considerable number of them—that is, all which are really porismatic —have the *strictly porismatic form of enunciation*. Of the remaining ones, a very small number are local theorems; and the rest are given in the ordinary form of indeterminate theorems.

In all the attempts at solution of the porismatic part of these propositions that I have met with, they have invariably been treated as indeterminate theorems, the porismatic constructions being first supplied; and in supplying these, the authors, having no mode of analysis adapted to their object (except from conjecture), had to encounter difficulties which would inevitably render their success impossible. In fact, the skill and address manifested by Dr SMALL (*Ed. Trans.* vol. ii.), and Messrs LOWRY and SWALE (*Leyb. Repos.*, *O.S.*, vols. i. ii.), manifest the most profound geometrical sagacity, and will reflect a lasting honour on their names: but, at the same time, it must always be regretted that their degree of success was not proportioned to the labour and ability employed in their researches. (Note A.)

At a very early period of my own studies, the porisms engaged much of my attention, and excited a deep interest in the inquiry. This interest, in the outset, was created by the paper of Professor PLAYFAIR, in the Edinburgh Transactions, —one of the most luminous and philosophical discussions of a mathematical subject it has ever been my good fortune to read. His suggestion of an algebraical analysis of the porism, which unfortunately he never published, led me to attempt such an application myself; and it could not long escape notice, under these circumstances, that the method of treatment must be identical with that employed in the "method of indeterminate co-efficients;" in fact, that this latter method always occurs in the shape of a porism, and all the propositions in

which it can be applied are strictly, in form and essence, porisms. At the same time, I saw that, in most geometrical porisms, the co-ordinate method would give considerable facility in conducting the actual solution; and having applied this method to a considerable number of porisms which had been treated geometrically, its application to Dr STEWART's general propositions became natural. In this way, by the use of *rectangular* co-ordinates, nearly the whole of the propositions which had been discussed by Dr SMALL, and Messrs LOWRY and SWALE, were readily established, together with the last five porisms of Dr STEWART respecting the circle. A few of these were sent to a periodical work; but some circumstances connected with that paper, induced me to lay aside the subject altogether, till a recent period. The views to which I was at that time led, have been since explained in the "MATHEMATICIAN" (Nos. 1 and 2), to which I must refer for details which would be unsuitable to the present paper. I had intentionally omitted from this latter paper all reference to Dr STEWART's theorems, for two reasons:—*first*, That I had found the insufficiency of the *rectangular* co-ordinate system to meet the object of the more general propositions, from its always giving a redundancy of conditional equations, arising out of a peculiarity in the expressions; and, *secondly*, that I had found the method of *polar* co-ordinates free from this embarrassing objection in all the cases I had tried, and hoped to find it so in all cases whatever. Having now found that such is the case, and having likewise discovered a method of solving the equations to which Dr STEWART's porisms give rise, I am desirous of laying the results before the Royal Society of Edinburgh.

Many reasons induce this wish. Dr STEWART's position in the University of Edinburgh, and his being one of the most distinguished of the original Fellows of the Royal Society, are reasons, however, paramount to all others; and I am led to believe that an interest will be felt (even in a subject purely relative to speculative mathematics) by that Society, to which I ought to pay respect. Another is, that the *polar equation of the straight line*, of which so much use is made in this discussion, was first given, incidentally, in the Edinburgh Transactions (vol. xii.); and the present is the first application made of that system of equations, except to comparatively elementary inquiries. The subject of these equations has, however, been more amply developed in my recent edition of Dr HUTTON's *Mathematics* (12th edit.), to which reference may be made in any case where the first sketch, already referred to, may be considered incomplete.

Adopting, as I do, without modification, Dr SIMSON's definition of the porismatic proposition, and taking into account that the point from which lines are drawn (either to points or perpendicular to lines), is arbitrary, the following statement of the process which I employ will appear both simple and obvious. It will, however, be necessary to remark, that the points, lines, or other entities, which the proposition affirms to be determinable, are called, for precision, *poris-*

matic points, porismatic lines, etc.; and that these points, lines, *etc.*, are said to be *porismatised*, instead of *given*, as usually expressed.

The arbitrary point is invariably denoted by the polar co-ordinates $r\,\theta$; the porismatic are denoted by the *unknown co-ordinates* of the point, if a point be porismatised; and by the equation of a locus with *unknown co-efficients*, if a line or any other locus. The equation of the porism is then formed by means of these co-ordinates of points, or equations of lines, the several data of the proposition, and the arbitrary point $r\,\theta$.* Then, since $r\,\theta$ is perfectly arbitrary, the general equation of the porism can only be fulfilled by the co-efficients of the several combinations of r and θ which appear in it, being separately and simultaneously equal to zero. This equating to zero of those several co-efficients, gives a number of conditional equations, involving the several porismatic unknowns; and we must have as many equations, independent of each other, as there are unknowns porismatised in the statement of the proposition. Should the number of these conditional equations be in excess or defect of the number of porismatised unknowns, the porism is incorrectly stated. However, it is always easy to correct the enunciated porism so as to fulfil these conditions, either by abstraction from the number of porismatised entities, or by addition to them, as the case may require.

The number of conditional equations may, however, be correct, and yet the porism not true: for if there be not corresponding *real* values for each of the unknowns deducible from these equations, it will follow that the conditions of the porism are inconsistent with each other. The complete algebraical solution of a porism requires, therefore, that the conditional equations shall be either actually resolved, or at least that it shall be shewn that the roots of the final equations in each of them, from which all the others have been eliminated, are real.

In the first part of this discussion, I have, in the main, attended to the formation of the conditional equations of the porisms, and the correct determination of the number of porismatic points and lines: but still I have resolved the equations themselves in a great number of cases, including those belonging to several porisms that have not been before established by any method. The equations, however, that result are of a peculiar class and admit of easy discussion by one general method. The preliminary discussions which force themselves upon us in the solution of these, would occupy so much space, that I have thought it better to defer them to the second, or concluding part of the paper. I have, however, judged it proper to give, in one case, a separate proof of the erroneous number of porismatic entities, enunciated by Dr STEWART, in order to remove any latent suspicion that certain of the equations were vir-

* When the point is not entirely arbitrary (as in most porisms is the case), r and θ will be connected by an equation which defines the locus of the partially arbitrary point. Any detail upon this head would, however, be altogether irrelevant in this place. See "The Mathematician," as above.

tually contained in the others. (*Note* E.) A formal proof of the correctness of my own determination will be contained in the actual solution of the general conditional equations. It will there appear, not only that this determination is correct, but also that, under these modified conditions, the values of all the porismatic entities are *essentially real.*

The *indeterminate theorems* except those established by Dr STEWART, are also proved in the present part of the discussion. As these have been established already in different ways, it was not necessary to dwell upon them at any considerable length, the mere indication of the application of our lemmas to this purpose, being deemed sufficient for the object in view.

Of the few propositions regarding *loci*, it is sufficient to remark, that this branch of the subject is too well understood at the present day, to need discussion here; and, at the same time, that some of them are true, *only* under special relations amongst the data, instead of universally true, as Dr STEWART has enunciated them.

The five properties of the circle which form the concluding ones of the *General Theorems*, are deferred to the next part of this paper, not from any peculiarity in the manner of treating them, but to equalise, as much as possible, the extent of the two parts into which the discussion is divided.

In the enunciations, the forms are altered to suit the view under which the method of solution here employed would give them in the most convenient shape for use. An abbreviated notation, too, is employed in the expression of the theorems: but the principle of that notation is so simple, and, indeed, the notation itself is so commonly in use for analogous purposes, as to render it almost needless to distinctly specify it. It is

$$a_1 + a + \ldots\ldots + a_m = S\, a_m,$$
$$a_1 r_1^n + a_2 r_2^n + \ldots\ldots a_m r_m^n = S\,(a_m r_m^n)\,;\ etc.$$

The classification, also, of the *General Theorems* is here added.

1. *Indeterminate Theorems.* 1, 2, 3, 4, 5, 6, 7, 8, 22, 23, 26, 27, 28, 29, 34, 39, 40, 41, 42, 45.

2. *Porismatic, relating to points and lines.* 9, 10, 11, 12, 13, 14, 15, 16, 17, 18, 19, 20, 21, 24, 25, 30, 31, 32, 33, 35, 36, 37, 38, 43, 44, 46, 47, 48, 49, 50, 51, 52, 53.

3. *Loci.* 54, 55, 56, 57, 58, 59.

4. *Porismatic, relating to the circle.* 60, 61, 62, 63, 64.

A few notes are added on some articles in the paper, as it appeared more convenient to place these remarks in such a form than in *scholia* or foot-notes.

ROYAL MILITARY ACADEMY, WOOLWICH,
March 14. 1844.

SECTION I.—LEMMAS TO BE USED IN THE DISCUSSION.

LEMMA I.

To expand $(r^2 - 2\,r\,r_1 \cos\omega + r_1^2)^n$ in multiple cosines, n being a positive integer.

$$\text{Put } 2\cos\omega = u + \frac{1}{u}: \text{ then,}$$

$$(r^2 - 2\,r\,r_1 \cos\omega + r_1^2)^n = (r - r_1 u)^n \left(r - \frac{r_1}{u}\right)^n.$$

Expand both these by the binomial theorem, writing $t_0, t_1, t_2, \dots t_{n-1}, t_n$ for the co-efficients of the first, second, third, . . ., nth, $(n+1)$th terms of the expansion $(1+1)^n$. We thus have the two series.

$$t_0 r^n - t_1 r^{n-1} r_1 u + t_2 r^{n-2} r_1^2 u^2 - t_3 r^{n-5} r_1^3 u^3 + t_4 r^{n-4} r^4_1 u^4 - \dots$$

$$t_0 r^n - t_1 r^{n-1} \frac{r_1}{u} + t_2 r^{n-2} \frac{r_1^2}{u^2} - t_3 r^{n-2} \frac{r_1^3}{u^3} + t_4 r^{n-4} \frac{r_1^4}{u^4} - \dots$$

The following simple arrangement of the order of multiplication will enable us to obtain $(r_1 u)^k$ and $\left(\frac{r_1}{u}\right)^k$ simultaneously, and thence the value of $\cos(k\,\omega)$ for the successive values of k from 0 to n.

1. Multiply each term of the first series by that one of the second which stands immediately beneath it: the sum of these products is that term of the expansion which is clear of cosines, or that in which $k=0$.

2. Multiply each term of the first series by that one of the second which stands immediately to the right of it, which will be the co-efficient of $\frac{1}{u}$: then multiply each term of the second series by that one of the first which stands immediately to the right of it, which gives the co-efficients of u. These co-efficients will evidently be identical. Whence we shall have the compound co-efficient of $u + \frac{1}{u}$ or of $2\cos\omega$.

3. Multiply cross-ways as before, the multipliers being in this case two steps to the right, instead of one, as in the preceding case: the result will be the compound co-efficient of $u^2 + \frac{1}{u^2}$ or of $2\cos 2\,\omega$.

4. Taking, in like manner, the multipliers three steps to the right of the terms multiplied, we shall obtain the compound co-efficient of $u^3 + \frac{1}{u^3}$ or of $2\cos 3\,\omega$.

Proceeding thus, we shall have every term of the first series multiplied by every term of the second, without any repetition of the same factors: and the required expansion will be found to be (R^2 denoting the vinculated trinomial),

$$R^{2n}=\{t_0^2 r^{2n}+t_1^2 r^{2(n-1)} r_1^2+t_2^2 r^{2(n-2)} r_1^4+\ldots\ldots+t_n^2 r_1^{2n}\}$$
$$-2 r r_1 \cos\omega\{t_0 t_1 r^{2(n-1)}+t_1 t_2 r^{2(n-2)} r_1^2-t_2 t_3 r^{2(n-3)} r_1^4+\ldots\ldots+t_{n-1} t_n r_1^{2(n-1)}\}$$
$$+2 r^2 r_1^2 \cos 2\omega\{t_0 t_2 r^{2(n-2)}+t_1 t_3 r^{2(n-3)} r_1^2+t_2 t_4 r^{2(n-4)} r_1^4+\ldots\ldots+t_{n-2} t_n r_1^{2(n-n)}\}$$
$$-2 r^3 r_1^3 \cos 3\omega\{t_0 t_3 r^{2(n-2)}+t_1 t_4 r^{2(n-4)} r_1^2+t_2 t_5 r^{2(n-5)} r_1^4+\ldots\ldots+t_{n-3} t_n r_1^{2(n-3)}\}$$
$$\cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot$$
$$\mp 2 r^{n-1} r_1^{n-1} \cos(n-1)\omega\{t_0 t_{n-1} r^2+t_1 t_n r_1^2\}$$
$$\pm 2 r^n r_1^n \cos n\omega\{t_0 t_n\}$$

The upper or lower signs of the concluding terms applying as n is even or odd.

It will be obvious that a slight simplification of this result may be obtained from the consideration that n being integer, $t_0=t_n$, $t_1=t_{n-1}$, *etc.* Our present object will not, however, require any special attention to this circumstance.

For example, let $n=2$, then

$$(r^2-2 r r_1 \cos\omega+r_1^2)^2=r^4+4 r^2 r_1^2+r_1^4$$
$$-2 r r_1 \cos\omega\{2 r^2+2 r_1^2\}+2 r^2 r_1^2 \cos 2\omega.$$

Let $n=3$: then

$$(r^2-2 r r_1 \cos\omega+r_1^2)^3=r^6+9 r^4 r_1^2+9 r^2 r^4+r_1^6$$
$$-2 r r_1 \cos\omega\{3 r^4+9 r^2 r_1^2+3 r_1^4\}$$
$$+2 r^2 r_1^2 \cos 2\omega\{3 r^2+3 r_1^2\}$$
$$-2 r^3 r_1^3 \cos 3\omega.$$

Lemma II.

To expand $(p-r\cos\omega)^n$ in multiple cosines, n being a positive integer.

By the binomial theorem we have

$$(p-r\cos\omega)^n=t_0 p^n-t_1 p^{n-1} r\cos\omega+t_2 p^{n-2} r^2\cos^2\omega-t_3 p^{n-3} r^2\cos^3\omega+\ldots\ldots$$

For the powers of $\cos\omega$ put their values in multiple cosines: then the expansion becomes,

$$\left\{t_0 p^n+\frac{t_2 p^{n-2} r^2}{2^1}+\frac{3 t_4 p^{n-4} r^4}{2^3}+\frac{10 t_6 p^{n-6} r^6}{2^5}+\ldots\ldots:\right\}$$
$$-\left\{\frac{t_1 p^{n-1} r}{2^0}+\frac{3 t_3 p^{n-3} r^3}{2^2}+\frac{10 t_5 p^{n-5} r^5}{2^4}+\frac{35 t_7 p^{n-7} r^7}{2^6}+\ldots\ldots\right\}\cos\omega$$
$$+\left\{\frac{t_2 p^{n-2} r^2}{2^1}+\frac{4 t_4 p^{n-4} r^4}{2^3}+\frac{15 t_6 p^{n-6} r^6}{2^5}+\ldots\ldots\ldots\ldots\right\}\cos 2\omega$$
$$-\left\{\frac{t_3 p^{n-3} r^3}{2^2}+\frac{5 t_5 p^{n-5} r^5}{2^4}+\frac{21 t_7 p^{n-7} r^7}{2^6}+\ldots\ldots\ldots\ldots\right\}\cos 3\omega$$
$$\&c. \qquad \&c. \qquad \&c.$$

The law of the numerical co-efficients of the several powers of p and r are

already known: but as (except those of the first line, which is clear of the cosines) we shall not require them in these inquiries, it will be unnecessary to discuss them, beyond the extent which our present purpose demands.

It will be observed, that all the terms, after the first, of the first line, arise from the expansion of the *even powers* of $\cos \omega$, and are, in fact, the absolute terms of those expanded even powers. Let these powers be denoted generally by 2μ: then the general form of these terms is

$$\frac{1}{2^{2\mu}} \cdot \frac{2\mu(2\mu-1)(2\mu-2)\ldots\ldots(\mu+1)}{1.2.3.\ \ldots\ldots\ \mu}.$$

This will take, very readily and obviously, the following forms in succession:

$$\frac{1}{2^{2\mu}} \cdot \frac{1.3.5.7\ldots.(2\mu-1)\times 2.4.6\ldots.2\mu}{(1.2.3\ldots..\mu)^2}$$

$$=\frac{1}{2^{2\mu}} \cdot \frac{1.3.5.7\ldots.(2\mu-1)\times 2^{\mu}.1.2.3\ldots.\mu}{(1.2.3\ldots..\mu)^2}$$

$$=\frac{1}{2^{2\mu}} \cdot \frac{1.3.5.7\ldots..(2\mu-1)}{1.2.3\ldots..\mu}$$

$$=\frac{1.3.5.7\ldots..(2\mu-1)}{2.4.6.8\ldots..2\mu}.$$

Giving to μ the successive values which are applicable to the successive cases viz. 1, 2, ... n, we have the form of the first line changed to

$$p^n+\frac{1}{2}.t_2 p^{n-2} r^2+\frac{1.3}{2.4} t_4 p^{n-4} r^4+\frac{1.3.5}{2.4.6} t_6 p^{n-6} r^6+\ldots.$$

For examples of the general expansion, let $n=2$: then

$$(p-r\cos\omega)^2=(p^2+\frac{1}{2}r^2)-2pr\cos\omega+\frac{1}{2}r^2\cos 2\omega,$$

Let $n=3$: then

$$(p-r\cos\omega)^3=p^3+\frac{3pr^2}{2}-3(p^2r+\frac{1}{4}r^3)\cos\omega+\frac{3}{2}pr^2\cos 2\omega-\frac{1}{4}r^3\cos 3\omega.$$

Let $n=4$: then

$$(p-r\cos\omega)^4=p^4+3p^2r^2+r^4-(4p^3r+3pr^3)\cos\omega$$
$$+(3p^2r^2+\frac{1}{2}r^4)\cos 2\omega-pr^3\cos 3\omega+\frac{1}{8}r^4\cos 4\omega.$$

Lemma III.

To expand $(1-\cos\omega)^n$ *in multiple cosines of* ω, n *being an integer.*

This is, in fact, but a particular case of each of the preceding lemmas, and its expansion might be deduced from either of them by making the requisite modifications in the formulæ.

In the first lemma, by making $r_1 = r$, we should get

$$(r^2 - 2\,r\,r_1 \cos \omega + r_1^2)^n = (2\,r^2)^n\,(1 - \cos \omega)^n\,;$$

and in the second, by making $p = r$, we should get

$$(p - r \cos \omega)^n = r^n (1 - \cos \omega)^n.$$

We should thus obtain two different forms: but the expansion will be better adapted to our present object when obtained as follows :—

Since $(1 - \cos \omega)^n = 2^n \sin^{2n} \frac{1}{2}\,\omega$; the ordinary formula, making the requisite change in the form of the last term, gives

$$\sin^{2n} \frac{1}{2}\,\omega = \frac{1}{(-1)^n\,2^{2n-1}} \cdot \left\{ \cos 2\,n\,\frac{1}{2}\,\omega - \frac{2\,n}{1} \cos (2\,n-2)\,\frac{1}{2}\,\omega + \ldots\ldots \right.$$
$$\left. + (-1)^n \cdot \frac{1}{2} \cdot \frac{2\,n\,(2\,n-1) \ldots\ldots (n+1)}{1\,.\,2\,.\,3 \ldots\ldots n} \right\}$$
$$= \frac{1}{(-1)^n\,2^{2n-1}} \cdot \left\{ \cos n\,\omega - \frac{2\,n}{1} \cos (n-1)\,\omega + \ldots\ldots \right.$$
$$\left. + (-1)^n \cdot \frac{1}{2} \cdot \frac{2\,n\,(2\,n-1) \ldots\ldots (n+1)}{1\,.\,2\,.\,3 \ldots\ldots n} \right\}$$

(for $2\,n$ is always even, when n is an integer.)

$$= \frac{1}{(-1)^n\,2^{2n-1}} \left\{ \cos n\,\omega - \frac{2\,n}{1} \cos (n-1)\,\omega + \ldots\ldots \right\}$$
$$+ \frac{1}{2^{2n}} \cdot \frac{2\,n\,(2\,n-1) \ldots\ldots (n-1)}{1\,.\,2\,.\,3 \ldots\ldots n}\,.$$
$$= \frac{1}{(-1)^n\,2^{2n-1}} \left\{ \cos n\,\omega - \frac{2\,n}{1} \cos (n-1)\,\omega + \ldots\ldots \right\}$$
$$+ \frac{1}{2} \cdot \frac{1\,.\,3\,.\,5 \ldots\ldots (2\,n-1)}{1\,.\,2\,.\,3 \ldots\ldots n}\,.$$

Lemma IV.

If θ_1, θ_2, θ_3, ... θ_n, be n angles in arithmetical progression, whose common difference is $\frac{2\pi}{n}$ (or, which is the same thing, if they be the angles formed by lines from the summits of a regular polygon with any arbitrary line, the centre of the polygon being the origin of co-ordinates), we shall have simultaneously,

$$\begin{array}{llllll}
\cos \theta_1 & + \cos \theta_2 & + \cos \theta_3 & + \ldots\ldots & + \cos \theta_n & = 0 \\
\cos 2\,\theta_1 & + \cos 2\,\theta_2 & + \cos 2\,\theta_3 & + \ldots\ldots & + \cos 2\,\theta_n & = 0 \\
\cos 3\,\theta_1 & + \cos 3\,\theta_2 & + \cos 3\,\theta_3 & + \ldots\ldots & + \cos 3\,\theta_n & = 0 \\
\cdot & \cdot & \cdot & \cdot & \cdot & \cdot
\end{array}$$
$$\cos (n-1)\,\theta_1 + \cos (n-1)\,\theta_2 + \ldots\ldots + \cos (n-1)\,\theta_n = 0$$

and

$$\begin{array}{l} \sin\theta_1 + \sin\theta_2 + \sin\theta_3 + \ldots + \sin\theta_n = 0 \\ \sin 2\theta_1 + \sin 2\theta_2 + \sin 2\theta_3 + \ldots + \sin 2\theta_n = 0 \\ \sin 3\theta_1 + \sin 3\theta_2 + \sin 3\theta_3 + \ldots + \sin 3\theta_n = 0 \\ \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \\ \sin(n-1)\theta_1 + \sin(n-1)\theta_2 + \ldots + \sin(n-1)\theta_n = 0. \end{array}$$

These properties are already known, and hence only require to be put down, without investigation.

Section II. THE PORISMATIC PROPOSITIONS.

Propositions IX., X. Porisms.

Let there be given in a plane the m points $A_1, A_2, \ldots A_m$, *and as many magnitudes* $a_1, a_2, \ldots a_m$: *then a point* X *may be found, such, that if we draw* $A_1X, A_2X, \ldots A_mX$, *and likewise to any point* Z *in the same plane we draw* $A_1Z, A_2Z, \ldots A_mZ$, *and join* XZ, *we shall always have*

$$a_1 . A_1Z^2 + a_2 . A_2Z^2 + \ldots = a_1 . AX^2 + a_2 . A_2X^2 + \ldots + (a_1 + a_2 + \ldots) XZ^2:$$

that is,

$$S(a_m . A_mZ^2) = S(a_m . A_mX^2) + Sa_m . XZ^2.$$

For, let the given points be denoted by $r_1\theta_1, r_2\theta_2 \ldots r_m\theta_m$; the porismatic one by $r_0\theta_0$, and the arbitrary one by $r\theta$. Then the general type of the component parts of the equation of the porism are

$$\begin{aligned} a_m . A_mZ^2 &= a_m\{r^2 - 2rr_m\cos(\theta - \theta_m) + r_m^2\}, \\ a_m . A_mX^2 &= a_m\{r_0^2 - 2r_0r_m\cos(\theta_0 - \theta_m) + r_m^2\}, \\ Sa_m . XZ^2 &= Sa_m\{r^2 - 2rr_0\cos(\theta - \theta_0) + r_0^2\}. \end{aligned}$$

Inserting these in the general equation, cancelling common terms from the equation, and equating to zero, the co-efficients of $r\cos\theta$, and $r\sin\theta$ (the only forms in which the arbitraries appear in the expression), we shall have the following conditional equations:—

$$Sa_m . r_0\cos\theta_0 = S(a_mr_m\cos\theta_m) \quad \ldots \ldots \quad (1)$$

$$Sa_m . r_0\sin\theta_0 = S(a_mr_m\sin\theta_m) \quad \ldots \ldots \quad (2)$$

$$Sa_m . r_0^2 = \left.\begin{array}{l} r_0\cos\theta_0 . S(a_mr_m\cos\theta_m) \\ + r_0\sin\theta_0 . S(a_mr_m\cos\theta_m) \end{array}\right\} \quad \ldots \quad (3)$$

The first and second are the equations of the *centroïd*, and the third is involved in the other two, as is obvious. Wherefore the porismatic point is the centroïd of the system.

[See, also, the note on these propositions.]

PROPOSITIONS XI., XII. PORISMS.

Let there be given m points and m magnitudes, as in the preceding: then there may be found a circle, and likewise a point, such, that drawing any line through the point found to cut the circle in X and Y, and that Z be any point whatever in the same plane, we shall always have

$$S(A_m Z^2) = \frac{1}{2} S a_m \, . \, (XZ^2 + YZ^2)$$

For, let the given points, referred to the centroïd as origin, and any axis whatever be $r_1 \theta_1$, $r_2 \theta_2$, . . . , $r_m \theta_m$; and denote the points X, Y by $u_1 \omega_1$, $u_2 \omega_2$, and the radius of the circle by ρ; and let Z be $r\theta$.

Then, expressing the lines concerned in terms of these quantities, cancelling $S a_m \, . \, r^2$ from both sides, and equating to zero the co-efficients of r,

$$S(a_m r_m^2) = \frac{1}{2} S a_m (u_1^2 + u_2^2) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

$$u_1 \cos(\theta - \omega_1) + u_2 \cos(\theta - \omega_2) = 0 \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

But since also θ is arbitrary, (2) becomes

$$u_1 \cos \omega_1 + u_2 \cos \omega_2 = 0 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

$$u_1 \sin \omega_1 + u_2 \sin \omega_2 = 0 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

These two equations are satisfied by *any* two points in a line passing through the origin, and equidistant from it, on opposite sides; or $u_1 = u_2$, and $\omega_1 = \pi + \omega_2$.

The point required to be found is hence the centroïd, and the circle has that point for its centre.

It also follows, that $u_1 = u_2 = \rho$ the radius of the circle; and hence from (1) we have

$$\rho^2 = \frac{S(a_m r_m^2)}{S a_m}.$$

Whence the circle is entirely determined.

Dr STEWART porismatises, not a circle, but two points, X and Y, to be found. The equations to which in such form the proposition gives rise, are precisely the same as those above: wherefore there would be given only the three equations (1, 2, 3) for the determination of four quantities u_1, u_2, ω_1, ω_2, which obviously leaves one of the quantities indeterminate. Dr SMALL notices, in another form, this indeterminateness.

[See also note on these.]

PROPOSITIONS XIII., XVIII. PORISMS.

Let there be given m parallel lines and m magnitudes a_1, a_2, . . . , a_m: then there can be found another line parallel to these, and likewise a space s^2, such, that

if from any point whatever Z lines ZP_1, ZP_2, ... ZP_m be drawn perpendicular to the m given lines, and ZP to the line found, we shall always have

$$S(a_m \cdot ZP_m^2) = S a_m (ZP^2 + s^2).$$

Let the given lines be referred to any line perpendicular to them, as polar axis; and let the origin be the controïd of the points in which the axis cuts the given lines. Denote the distances of these respective points from the centroïd by $p_1, p_2, \ldots p_m$; and by p, the distance of the porismatic line from the centroïd. Also, let $r\,\theta$ denote the arbitrary point Z.

Then the equations of the given lines will be

$$p_1 = r\cos\theta,\ p_2 = r\cos\theta, \ldots p_m = r\cos\theta.$$

And (see HUTTON'S *Course*, ii. p. 268, 12th ed.) the perpendiculars will be expressed by

$$ZP_1 = \pm(p_1 - r\cos\theta),\ ZP_2 = \pm(p_2 - r\cos\theta);\ etc.$$

Wherefore the equation of the porism becomes

$$a_1(p_1 - r\cos\theta)^2 + a_2(p_2 - r\cos\theta)^2 + \ldots = S a_m\{(p - r\cos\theta)^2 + s^2\};$$

or expanding, cancelling, and equating the co-efficients of $\cos\theta$ to 0, we have simply

$$p \cdot S a_m = S(a_m p_m) \quad \ldots\ldots\ldots \quad (1)$$

$$S(a_m p_m) = S a_m \cdot (p^2 + s^n) \quad \ldots\ldots\ldots \quad (2)$$

Now, since the origin is the centroïd, we have from (1)

$$S(a_m p_m) = 0;\ \text{and hence } p = 0, \quad \ldots\ldots \quad (3)$$

or the line sought passes through the centroïd.

Again, from (2) and (3) we get the porismatic space

$$s^2 = \frac{S(a_m p_m^2)}{S a_m} \quad \ldots\ldots\ldots\ldots \quad (4)$$

PROPOSITIONS XV., XIX. PORISMS.

Let there be given m lines all meeting in one point, and m magnitudes $a_1, a_2, \ldots, a_m$: then there can be found two other lines, also passing through the same point, such, that if from any point Z there be drawn perpendiculars ZP_1, ZP_2...., ZP_m, to the given lines, and likewise ZQ_1, ZQ_2, to those found, we shall always have

$$2S(a_m ZP_m^2) = S a_m \cdot \{ZQ_1^2 + ZQ_2^2\}.$$

Let the points in which all the lines meet be taken as polar origin, the axis being any whatever. Let the angles made by the perpendiculars to the given lines with the axis be $\theta_1, \theta_2, \ldots, \theta_m$, and those made by the perpendiculars to the porismatic lines be ω_1, ω_2: then if $r\,\theta$ be the arbitrary point Z, we shall have

$$ZP_1 = \pm r\cos(\theta - \theta_1),\quad ZP_2 = \pm r\cos(\theta - \theta_2),\ etc.$$

$$ZQ_1 = \pm r\cos(\theta - \omega_1),\quad ZQ_2 = \pm r\cos(\theta - \omega_2).$$

Insert these in the equation of the porism (putting the expansion in multiple cosines), cancel, and equate to zero the co-efficients of $\cos 2\theta$ and $\sin 2\theta$. Then there results,

$$\cos 2\omega_1 + \cos 2\omega_2 = \frac{2S(a_m \cos 2\theta_m)}{Sa_m},$$

$$\sin 2\omega_1 + \sin 2\omega_2 = \frac{2S(a_m \sin 2\theta_m)}{Sa_m}.$$

The solution of these equations gives

$$\cos 2\omega_1 = \frac{S(a_m \cos 2\theta_m) \pm \mathrm{R}\,.\,S(a_m \sin 2\theta_m)}{Sa_m},$$

$$\cos 2\omega_2 = \frac{S(a_m \cos 2\theta_m) \mp \mathrm{R}\,.\,S(a_m \sin 2\theta_m)}{Sa_m};$$

$$\text{where } \mathrm{R}^2 = \frac{(Sa_m)^2 - \{S(a_m \cos 2\theta_m)\}^2 - \{S(a_m \sin 2\theta_m)\}^2}{\{S(a_m \cos 2\theta_m)\}^2 + \{S(a_m \sin 2\theta_m)\}^2}.$$

Since the angles are symmetrically involved in the general expression, we see that the double sign does not imply different possible solutions, but merely that the sign of $\cos 2\omega_2$ depends upon that which we select as belonging to $\cos 2\omega_1$.

That it is always real, is at once obvious: for the denominator of the radical is essentially positive; and the numerator is convertible into

$$\begin{aligned}
&2a_1 a_2\{1-\cos 2(\theta_1-\theta_2)\} + 2a_1 a_3\{1-\cos 2(\theta_1-\theta_3)\} + \ldots\ldots \\
&+2a_2 a_3\{1-\cos 2(\theta_2-\theta_3)\} + 2a_2 a_4\{1-\cos 2(\theta_2-\theta_4)\} + \ldots\ldots \\
&+\ .\ .\ .\ .\ .\ .\ .\ .\ .\ .\ . \\
&+2a_{m-1}\,.\,a_m\{1-\cos 2(\theta_{m-1}-\theta_m)\}
\end{aligned}$$

which is, also, obviously positive.

Again, the expressions for the single angles ω_1, ω_2 are found from the preceding (3, 4), by means of the familiar relation,

$$\cos\omega_1 = \pm\sqrt{\frac{1+\cos 2\omega_1}{2}} \text{ and } \cos\omega_2 = \pm\sqrt{\frac{1+\cos 2\omega_2}{2}}.$$

But it is easy to see that these double signs, in each case, only refer to the two opposite branches of the same line in respect of the origin: so that, on the whole, the solutions are found to be single, and that there is one, and only one, pair of lines which fulfils the conditions of the porism.

Propositions XVI., XX. Porisms.

Let there be given m lines, which are neither all parallel nor all meet in one point, and m magnitudes, $a_1, a_2 \ldots, a_m$: then there can be found two other straight lines and a space s^2, such, that if perpendiculars $ZP_1, ZP_2, \ldots ZP_m$ be drawn to all the given lines, and others ZQ_1, ZQ to those found, we shall always have

$$2\,S\,(a_m \;\; Z\;P_m{}^2) = S\,a_m\,.\,\{ZQ_1{}^2 + ZQ_2{}^2 + s^2\}.$$

For, let the several given lines be

$$p_1 = r\cos(\theta-\theta_1),\ p_2 = r\cos(\theta-\theta_2), \ldots, p_m = r\cos(\theta-\theta_m);$$

and let the porismatic lines be denoted by

$$q_1 = r\cos(\theta-\omega_1),\ \text{and}\ q_2 = r\cos(\theta-\omega_2).$$

Then the perpendiculars upon these from the arbitrary point Z, or $r\,\theta$, will be (Hutton, ii. p. 268),

$$\pm\{p_1 - r\cos(\theta-\theta_1)\},\ \pm\{p_2 - r\cos(\theta-\theta_2)\},\ etc.;\ \text{and}$$
$$\pm\{q_1 - r\cos(\theta-\omega_1)\},\ \text{and}\ \pm\{q_2 - r\cos(\theta-\omega_2)\}.$$

Insert these values in the equation of the porism, and arrange the results in terms $r\cos\theta$, $r\cos 2\theta$, $r\sin\theta$, and $r\sin 2\theta$; then, as r^2 cancels, we have

$$2\,S\,(a_m\,p_m{}^2) = S\,a_m\,.\,\{q_1{}^2 + q_2{}^2 + s^2\} \quad \ldots\ldots\ldots\ldots \quad (1)$$
$$2\,S\,(a_m\,p_m\cos\theta_m) = S\,a_m\,.\,\{q_1\cos\omega_1 + q_2\cos\omega_2\} \quad \ldots\ldots\ldots \quad (2)$$
$$2\,S\,(a_m\,p_m\sin\theta_m) = S\,a_m\,.\,\{q_1\sin\omega_1 + q_2\sin\omega_2\} \quad \ldots\ldots\ldots \quad (3)$$
$$2\,S\,(a_m\cos 2\,\theta_m) = S\,a_m\,.\,\{\cos 2\,\omega_1 + \cos 2\,\omega_2\} \quad \ldots\ldots\ldots \quad (4)$$
$$2\,S\,(a_m\sin 2\,\theta_m) = S\,a_m\,.\,\{\sin 2\,\omega_1 + \sin 2\,\omega_2\} \quad \ldots\ldots\ldots \quad (5)$$

From (4, 5) will be found, as in the preceding porism, the values of $\omega_1\ \omega_2$; from these results, combined with (2, 3), we shall obtain, by a simple equation, the values of q_1, q_2; and from these values inserted in (1) we finally obtain the value of the space s^2.

Propositions XVII., XXI. Porisms.

Let there be given m lines, which are neither all parallel nor all meet in one point, and m magnitudes, $a_1, a_2, \ldots, a_m$: then there can be found three lines, such, drawing from any point, Z, the perpendiculars $ZP_1, ZP_2, \ldots, ZP_m$ to the given lines, and ZQ_1, ZQ_2, ZQ_3 to those found, we shall always have

$$3\,S\,(a_m\,Z\,P_m{}^2) = S\,a_m\,.\,(Z\,Q^2).$$

Such is Dr STEWART'S statement; but forming the equations of condition, as in the preceding Propositions, we have

$$3\,S\,(a_m\,p_m^{\,2}) \quad = S\,a_m\,.\,\{q_1^{\,2}+q_2^{\,2}+q_3^{\,2}\} \quad \ldots \ldots \quad (1)$$

$$3\,S\,(a_m\,p_m\,\cos\theta_m) = S\,a_m\,.\,\{q_1\cos\omega_1+\ldots+q_3\cos\omega_3\} \quad \ldots \quad (2)$$

$$3\,S\,(a_m\,p_m\,\sin\theta_m) = S\,a_m\,.\,\{q_1\sin\omega_1+\ldots+q_3\sin\omega_3\} \quad \ldots \quad (3)$$

$$3\,S\,(a_m\cos 2\,\theta_m) \quad = S\,a_m\,.\,\{\cos 2\,\omega_1+\ldots+\cos 2\,\omega_3\} \quad \ldots \quad (4)$$

$$3\,S\,(a_m\sin 2\,\theta_m) \quad = S\,a_m\,.\,\{\sin 2\,\omega_1+\ldots+\sin 2\,\omega_3\} \quad \ldots \quad (5)$$

Now, in the present case we have only five equations for the determination of six quantities, ω_1, ω_2, ω_3, q_1, q_2, and q_3. The condition of the porism cannot, therefore, be fulfilled without another condition.

This indeterminateness, in respect of this proposition, has been noticed by Dr SMALL, *Ed. Trans.*, ii. p. 46. In the discussion of Props. 46-53 of this Series, will be noticed again.

PROPOSITIONS XXIV., XXV. PORISMS.

Let there be given m lines and m magnitudes as before: then p straight lines can be found, such, that if we draw the perpendiculars from any point Z to all the lines given, and to all the lines found, we shall have

$$p\,\{S\,(a_m\,.\,\mathrm{Z\,P}_m^{\,3})\} = S\,a_m\,.\,S\,(\mathrm{Z\,Q}_4^{\,3}).$$

The general form of the component terms of this equation is, *Lemma* ii.,

$$\mathrm{Z\,P}_m^{\,3} = (p_m^{\,3}+\frac{3}{2}p_m\,r^2) - 3\,(p_m^{\,2}\,r+\frac{1}{4}r^3)\cos(\theta-\theta_m)+\frac{3}{2}p_m r^2\cos 2(\theta-\theta_m)-\frac{1}{4}r^3\cos 3(\theta-\theta_m)$$

Whence, forming the equations of condition, we have the following series,

$$p\,S\,(a_m\cos 3\,\theta_m) \quad = S\,a_m\,.\,S\,(\cos 3\,\omega_4)$$

$$p\,S\,(a_m\sin 3\,\theta_m) \quad = S\,a_m\,.\,S\,(\sin 3\,\omega_4)$$

$$p\,S\,(a_m\,p_m\cos 2\,\theta_m) = S\,a_m\,.\,S\,(q_4\cos 2\,\omega_4)$$

$$p\,S\,(a_m\,p_m\sin 2\,\theta_m) = S\,a_m\,.\,S\,(q_4\cos 2\,\omega_4)$$

$$p\,S\,(a_m\cos\theta_m) \quad = S\,a_m\,.\,S\,(\cos\omega_4)$$

$$p\,S\,(a_m\sin\theta_m) \quad = S\,a_m\,.\,S\,(\sin\omega_4)$$

$$p\,S\,(a_m\,p_m^{\,2}\cos\theta_m) = S\,a_m\,.\,S\,(q_4^{\,2}\cos\omega_4)$$

$$p\,S\,(a_m\,p_m^{\,2}\sin\theta_m) = S\,a_m\,.\,S\,(q_4^{\,2}\sin\omega_4)$$

$$p\,S\,(a_m\,p_m) \quad = S\,a_m\,.\,S\,(q_4)$$

$$p\,S\,(a_m\,p_m^{\,3}) \quad = S\,a_m\,.\,S\,(q_4^{\,3})$$

Now, as there are ten equations, all independent of each other, it follows that $p=5$; for there will be five qs and five ωs to be determined from this system, and these are the requisite conditions for finding five lines. Whence the number of lines is incorrectly given by Dr Stewart who porismatises only *four* lines; but the Porism is evidently possible with the condition altered as here proposed.

Propositions XXX., XXXI. Porisms.

Let there be given m points $A_1, A_2, \ldots A_m$, and m magnitudes, $a_1, a_2, \ldots a_m$: then there can be found two lines OX, OY, and a point P, together with two magnitudes a, b; such, that if from any point whatever, Z, there be drawn lines to all the given points, and to the point found, together with perpendiculars ZX, ZY to the lines found, we shall always have

$$S\,(a_m\,.\,A_m Z^4)=S\,a_m\,.\,\{PZ^4+a^2(ZX^2+ZY^2+b^2)\}.$$

Let, as before, the given points be referred to the centroïd as origin of polar co-ordinates, and denoted by $r_1\,\theta_1$, $r_2\,\theta_2$, ... r_m, θ_m; also denote P by $r_0\,\theta_0$, Z by $r\,\theta$, and the lines OX, OY by

$$p_1=r\cos(\theta-\omega_1)$$
$$p_2=r\cos(\theta-\omega_2)$$

Then the perpendiculars on these from $r\,\theta$ will be expressed as before by

$$ZX=\pm\{p_1-r\cos(\theta-\omega_1)\}$$
$$ZY=\pm\{p_2-r\cos(\theta-\omega_2)\}$$

Also, the lines from Z to the several points will be expressed in power by

$$A_m Z^4=r^4+4\,r^2\,r_m^{\,2}+r_m^{\,4}-4\,r\,r_m\,(r_m^{\,2}+r^2)\cos(\theta-\theta_m)+2\,r_m^{\,2}\,r^2\cos 2\,(\theta-\theta_m)$$

$$PZ^4=r^4+4\,r^2\,r_0^{\,2}+r_0^{\,4}-4\,r\,r_0\,(r_0^{\,2}+r^2)\cos(\theta-\theta_0)+2\,r_0^{\,2}\,r^2\cos 2\,(\theta-\theta_0)$$

$$ZX^2=(p_1^{\,2}+\frac{1}{2}r^2)-2\,p_1\,r\cos(\theta-\omega_1)+\frac{1}{2}r^2\cos 2\,(\theta-\omega_1)$$

$$ZY^2=(p_2+\frac{1}{2}r^2)-2\,p_2\,r\cos(\theta-\omega_2)+\frac{1}{2}r^2\cos 2\,(\theta-\omega_2)$$

With these values form the equation of the porism, cancel common terms, and equate to zero the co-efficients of the arbitrary quantities which remain; then we get the following system of conditional equations:

$$S\,(a_m\, r_m{}^2) = S\, a_m \,.\, \{r_0{}^2 + \frac{1}{2} a^2\} \quad . \quad . \quad . \quad (1)$$

$$S\,(a_m\, r_m{}^4) = S\, a_m \,.\, \{r_0{}^4 + a^2\,(p_1{}^2 + p_2{}^2 + b^2)\} \quad . \quad . \quad . \quad (2)$$

$$S\,(a_m\, r_m \cos\theta_m) = S\, a_m \,.\, r_0 \cos\theta_0 \quad . \quad . \quad . \quad (3)$$

$$S\,(a_m\, r_m \sin\theta_m) = S\, a_m \,.\, r_0 \sin\theta_0 \quad . \quad . \quad . \quad (4)$$

$$S\,(a_m\, r_m{}^3 \cos\theta_m) = S\, a_m \,.\, \{r_0{}^3 \cos\theta_0 + \frac{1}{2} a^2\,(p_1 \cos\omega_1 + p_2 \cos\omega_2)\} \quad . \quad . \quad (5)$$

$$S\,(a_m\, r_m{}^3 \sin\theta_m = S\, a_m \,.\, \{r_0{}^3 \sin\theta_0 + \frac{1}{2} a^2\,(p_1 \sin\omega_1 + p_2 \sin\omega_2)\} \quad . \quad . \quad (6)$$

$$S\,(a_m\, r_m{}^2 \cos 2\,\theta_m) = S\, a_m \,.\, \{r_0{}^2 \cos 2\,\theta_0 + \frac{1}{4} a^2\,(\cos 2\,\omega_1 + \cos 2\,\omega_2)\} \quad . \quad . \quad (7)$$

$$S\,(a_m\, r_m{}^2 \sin 2\,\theta_m) = S\, a_m \,.\, \{r_0{}^2 \sin 2\,\theta_0 + \frac{1}{4} a^2\,(\sin 2\,\omega_1 + \sin 2\,\omega_2)\} \quad . \quad . \quad (8)$$

Now, since the left sides of (3, 4) are zero, we find that $r_0 = 0$, and that θ_0 is indeterminate. The point P is, therefore, the origin of co-ordinates, or the centroïd of the given system itself.

Insert this value of r_0 in (1): then we get

$$a^2 = \frac{2\, S\,(a_m\, r_m{}^2)}{S\, a_m} \quad . \quad . \quad . \quad (9)$$

Insert it in (7, 8): then these become

$$\cos 2\,\omega_1 + \cos 2\,\omega_2 = \frac{4\, S\,(a_m \,.\, r_m{}^2 \cos 2\,\theta_m)}{a^2 \,.\, S\, a_m} \quad . \quad . \quad . \quad (10)$$

$$\sin 2\,\omega_1 + \sin 2\,\omega_2 = \frac{4\, S\,(a_m\, r_m{}^2 \sin 2\,\theta_m)}{a^2\, S\, a_m} \quad . \quad . \quad . \quad (11)$$

from which ω_1 and ω_2 may be found as in (xv. xix); and which, as in that place, are real.

Put the value 0 of r_0 and the values of ω_1, ω_2 in (5, 6): then we get the values of p_1, p_2.

Finally, from the insertion of the values of p_1, p_2 and r_0 in (2), we obtain the value of b^2.

Propositions XXXII., XXXIII. Porism.

Let there be given m points A_1, A_2, ..., A_m, *and m magnitudes* a_1, a_2, ..., a_m: *then there may be found four points* B_1, B_2, B_3, B_4, *such, that if from any point,* Z, *we draw lines to all the given points, and likewise lines to all the points found, we shall always have*

$$4\, S\,(a_m \,.\, A_m Z^4) = S\, a_m \,.\, S\,(B_4\, Z^4).$$

[Dr STEWART says *three* points B_1, B_2, B_3, and the multiplier 3 on the left side of the equation.]

Taking the expressions for the fourth powers of the lines concerned, as before, and equating to zero the co-efficients of the arbitrary quantities, we get the following conditional equations :—

$$4\,S\,(a_m\,r_m \cos\theta_m) = S\,a_m \,.\, S\,(u_4 \cos\omega_4) \quad \ldots \quad (1)$$
$$4\,S\,(a_m\,r_m \sin\theta_m) = S\,a_m \,.\, S\,(u_4 \sin\omega_4) \quad \ldots \quad (2)$$
$$4\,S\,(a_m\,r_m^2 \cos 2\,\theta_m) = S\,a_m \,.\, S\,(u_4^2 \cos 2\,\omega_4) \quad \ldots \quad (3)$$
$$4\,S\,(a_m\,r_m^2 \sin 2\,\theta_m) = S\,a_m \,.\, S\,(u_4^2 \sin 2\,\omega_4) \quad \ldots \quad (4)$$
$$4\,S\,(a_m\,r_m^3 \cos\theta_m) = S\,a_m \,.\, S\,(u_4^3 \cos\omega_4) \quad \ldots \quad (5)$$
$$4\,S\,(a_m\,r_m^3 \sin\theta_m) = S\,a_m \,.\, S\,(u_4^3 \sin\omega_4) \quad \ldots \quad (6)$$
$$4\,S\,(a_m\,r_m^2) = S\,a_m \,.\, S\,(u_4^2) \quad \ldots \quad (7)$$
$$4\,S\,(a_m\,r_m^4) = S\,a_m \,.\, S\,(u_4^4) \quad \ldots \quad (8)$$

in which, as *eight* conditional equations inevitably result from the porism, there must be *four* points porismatised to be determined from them.

Since the origin is the centroïd, the left sides of (1, 2) become 0, and we see that the porismatic points have the same centroïd as the given system.

PROPOSITIONS XXXV TO XXXVIII. PORISMS.

Let there be given m lines and m magnitudes, a_1, a_2, ..., a_m : then there can be found p other right lines, such, that if from any point, Z, there be drawn perpendiculars Z P_1, Z P_2, ..., Z P_m, to the given lines, and Z Q_1, Z Q_2, ..., Z Q_p, to those found, we shall always have

$$p\,S\,(a_m \mathrm{Z\,P}_m^4) = S\,a_m \,.\, S\,(\mathrm{Z\,Q}_p^4)$$

Forming the conditional equations, as in the former cases, we have

$$p \,.\, S\,(a_m\,p_m^4) = S\,a_m \,.\, S\,(q_p^4) \quad \ldots \quad (1)$$
$$p \,.\, S\,(a_m\,p_m^2) = S\,a_m \,.\, S\,(q_p^2) \quad \ldots \quad (2)$$
$$p \,.\, S\,(a_m\,p_m^3 \cos\theta_m) = S\,a_m \,.\, S\,(q_p^3 \cos\omega_p) \quad \ldots \quad (3)$$
$$p \,.\, S\,(a_m\,p_m^3 \sin\theta_m) = S\,a_m \,.\, S\,(q_p^3 \sin\omega_p) \quad \ldots \quad (4)$$
$$p \,.\, S\,(a_m\,p_m \cos\theta_m) = S\,a_m \,.\, S\,(q_p \cos\omega_p) \quad \ldots \quad (5)$$
$$p \,.\, S\,(a_m\,p_m \sin\theta_m) = S\,a_m \,.\, S\,(q_p \sin\omega_p) \quad \ldots \quad (6)$$
$$p \,.\, S\,(a_m\,p_m^2 \cos 2\,\theta_m) = S\,a_m \,.\, S\,(q_p^2 \cos 2\,\omega_p) \quad \ldots \quad (7)$$
$$p \,.\, S\,(a_m\,p_m^2 \sin 2\,\theta_m) = S\,a_m \,.\, S\,(q_p^2 \sin 2\,\omega_p) \quad \ldots \quad (8)$$
$$p \,.\, S\,(a_m \cos 2\,\theta_m) = S\,a_m \,.\, S\,(\cos 2\,\omega_p) \quad \ldots \quad (9)$$
$$p \,.\, S\,(a_m \sin 2\,\theta_m) = S\,a_m \,.\, S\,(\sin 2\,\omega_p) \quad \ldots \quad (10)$$

$$p \cdot S(a_m p_m \cos 3\theta_m) = S a_m \cdot S(q_p \cos 3\omega_p) \quad \ldots \ldots \quad (11)$$
$$p \cdot S(a_m p_m \sin 3\theta_m) = S a_m \cdot S(q_p \sin 3\omega_p) \quad \ldots \ldots \quad (12)$$
$$p \cdot S(a_m \cos 4\theta_m) = S a_m \cdot S(\cos 4\omega_p) \quad \ldots \ldots \quad (13)$$
$$p \cdot S(a_m \sin 4\theta_m) = S a_m \cdot S(\sin 4\omega_p) \quad \ldots \ldots \quad (14)$$

which giving *fourteen* independent equations furnishes data for finding *seven* lines, instead of *five*, as stated by Dr STEWART: that is $p=7$, instead of $p=5$.

The preceding statement is that of *Prop.* 38, of which the others are particular cases. It becomes, *Prop.* 37, when $a_1=a_2=\ldots\ldots=a_m$. When all the lines are parallel it becomes the first case of *Prop.* 36. In this instance we have

$$\theta_1=\theta_2=\ldots\ldots=\theta_m=\frac{1}{2}\pi.$$

Substituting these values in the preceding equations, we have, by transposition,

$$S a_m \cdot S(q_p^4) = p \cdot S(a_m p_m^4) \quad \ldots \ldots \quad (15)$$
$$S a_m \cdot S(q_p^2) = p \cdot S(a_m p_m^2) \quad \ldots \ldots \quad (16)$$
$$S a_m \cdot S(q_p^3 \cos \omega_p) = 0 \quad \ldots \ldots \quad (17)$$
$$S a_m \cdot S(q_p^3 \sin \omega_p) = p \cdot S(a_m p_m^3) \quad \ldots \ldots \quad (18)$$
$$S a_m \cdot S(q_p \cos \omega_p) = 0 \quad \ldots \ldots \quad (19)$$
$$S a_m \cdot S(q_p \sin \omega_p) = p \cdot S(a_m p_m) \quad \ldots \ldots \quad (20)$$
$$S a_m \cdot S(q_p^2 \cos 2\omega_p) = -p \cdot S(a_m p_m^2) \quad \ldots \ldots \quad (21)$$
$$S a_m \cdot S(q_p^2 \sin 2\omega_p) = 0 \quad \ldots \ldots \quad (22)$$
$$S a_m \cdot S(\cos 2\omega_p) = -p \cdot S a_m \quad \ldots \ldots \quad (23)$$
$$S a_m \cdot S(\sin 2\omega_p) = 0 \quad \ldots \ldots \quad (24)$$
$$S a_m \cdot S(q_p \cos 3\omega_p) = 0 \quad \ldots \ldots \quad (25)$$
$$S a_m \cdot S(q_p \sin 3\omega_p) = -p \cdot S(a_m p_m) \quad \ldots \ldots \quad (26)$$
$$S a_m \cdot S(\cos 4\omega_p) = p \cdot S a_m \quad \ldots \ldots \quad (27)$$
$$S a_m \cdot S(\sin 4\omega_p) = 0 \quad \ldots \ldots \quad (28)$$

Now of these fourteen equations, eight (viz. 17, 19, 22, 23, 24, 25, 27, 28) are fulfilled by $\omega_1=\omega_2=\ldots\ldots=\omega_p=\frac{1}{2}\pi$, giving the lines to be found, parallel to the given lines. The other six equations are, with this condition, reduced to the four following ones:—

from (20, 26) .... $S a_m \cdot S(q_p) = p \cdot S(a_m p_m)$ . . . . (29)

from (16) .... $S a_m \cdot S(q_p^2) = p \cdot S(a_m p_m^2)$ . . . . (30)

from (18, 21) .... $S a_m \cdot S(q_p^3) = p \cdot S(a_m p_m^3)$ . . . . (31)

from (15) .... $S a_m \cdot S(q_p^4) = p \cdot S(a_m p_m^4)$ . . . . (32)

In this case we have $p=4$, instead of $p=3$, as stated by Dr STEWART.

[See also, NOTE E.]

Again, for the second case of *Prop.* 36, all the lines meet in one point; and this being the origin of co-ordinates, we shall have $p_1 = p_2 = \ldots\ldots = p_m = 0$; and hence from (1, 2) we have $q_1 = q_2 = \ldots\ldots = q_p = 0$, and all the porismatic lines pass through the origin.

For this reason, also, only those of the subsequent equations which do not involve $q_1, q_2, \ldots\ldots q_p$ have an existence in this case. These are (9, 10, 13, 14); and under these circumstances they become

$$S\,a_m \,.\, S\,(\cos 2\,\omega_p) = p \,.\, S\,(a_m \cos 2\,\theta_m) \quad \ldots \quad (33)$$

$$S\,a_m \,.\, S\,(\sin 2\,\omega_p) = p \,.\, S\,(a_m \sin 2\,\theta_m) \quad \ldots \quad (34)$$

$$S\,a_m \,.\, S\,(\cos 4\,\omega_p) = p \,.\, S\,(a_m \cos 4\,\theta_m) \quad \ldots \quad (35)$$

$$S\,a_m \,.\, S\,(\sin 4\,\omega_p) = p \,.\, S\,(a_m \sin 4\,\theta_m) \quad \ldots \quad (36)$$

These, again, give $p=4$ instead of $p=3$, as the proposition is stated by Dr Stewart.

Propositions XLIII,, XLIV. Porisms.

Let there be given m points $A_1, A_2, \ldots, A_m$, *and as many magnitudes,* $a_1, a_2, \ldots, a_m$; *and let n be any number (subject to conditions to be hereafter determined): then there can be found p points* $B_1, B_2, \ldots, B_p$, *such, that if from any point, Z, lines be drawn to all the given points, and likewise to the points found, we shall always have*

$$p \,.\, S\,(a_m \,.\, A_m Z^{2n}) = S\,a_m \,.\, S\,(B_p Z^{2n}).$$

Let $r_1\,\theta_1, r_2\,\theta_2, \ldots, r_m\,\theta_m$ be the given points;

$u_1\,\omega_1, u_2\,\omega_2, \ldots, u_p\,\omega_p$ the porismatic ones; and

$r\,\theta$, the arbitrary point Z. Then,

$$A_m Z^2 = r^2 - 2\,r\,r_m \cos(\theta - \theta_m) + r_m{}^2;$$

$$B_p Z^2 = r^2 - 2\,r\,u_p \cos(\theta - \omega_p) + u_p{}^2;$$

which are the general types of the squares of the lines to be raised to the nth power.

Let them be so raised by *Lemma* i.: then the terms in r^{2n} cancel from the equation being respectively

$$p\,(a_1\,r^{2n} + a_2\,r^{2n} + \ldots\ldots + a_m\,r^{2n}), \text{ and}$$

$$(a_1 + a_2 + \ldots\ldots a_m)\,(r^{2n} + r^{2n} + \ldots\ldots):$$

the latter being carried to p terms.

In the next place, equate the co-efficients of the several terms in r^μ, $r^\mu \cos \nu\,\theta$, and $r^\mu \sin \nu\,\theta$, for all values of μ and ν within the degrees expressed by the

expansion, to zero; and we shall thus obtain the conditional equations of the porism, viz.:

First, from terms clear of θ.

$$r^0 \ldots . p\,S\,(a_m\,r_m{}^{2n}) \quad = S\,a_m\,.\,S\,(u_p{}^{2n})$$

$$r^2 \ldots . p\,S\,(a_m\,r_m{}^{2(n-1)}) = S\,a_m\,.\,S\,(u_p{}^{2(n-1)})$$

$$r^4 \ldots . p\,S\,(a_m\,r_m{}^{2(n-2)}) = S\,a_m\,.\,S\,(u_p{}^{2(n-2)})$$

$$\cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot$$

$$r^{2(n-1)}\,.\,p\,S\,(a_m\,r_m{}^2) \quad = S\,a_m\,.\,S\,(u_p{}^2)$$

Secondly, from terms in cos θ and sin θ.

$$r \ldots . p\,S\,(a_m\,r_m{}^{2n-1}\cos\theta_m) = S\,a_m\,.\,S\,(u_p{}^{2n-1}\cos\omega_p)$$

$$r^3 \ldots p\,S\,(a_m\,r_m{}^{2n-3}\cos\theta_m) = S\,a_m\,.\,S\,(u_p{}^{2n-3}\cos\omega_p)$$

$$r^5 \ldots p\,S\,(a_m\,r_m{}^{2n-5}\cos\theta_m) = S\,a_m\,.\,S\,(u_p{}^{2n-5}\cos\omega_p)$$

$$\cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot$$

$$r^{2n-1}\,.\,p\,S\,(a_m\,r_m\cos\theta_m) \quad = S\,a_m\,.\,S\,(u_p\cos\omega_p)$$

and

$$r \ldots . p\,S\,(a_m\,r_m{}^{2n-1}\sin\theta_m) = S\,a_m\,.\,S\,(u_p{}^{2n-1}\sin\omega_p)$$

$$r^3 \ldots . p\,S\,(a_m\,r_m{}^{2n-3}\sin\theta_m) = S\,a_m\,.\,S\,(u_p{}^{2n-3}\sin\omega_p)$$

$$r^5 \ldots p\,S\,(a_m\,r_m{}^{2n-5}\sin\theta_m) = S\,a_m\,.\,S\,(u_p{}^{2n-5}\sin\omega_p)$$

$$\cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot$$

$$r^{2n-1}\,.\,p\,S\,(a_m\,r_m\sin\theta_m) \quad = S\,a_m\,.\,S\,(u_p\sin\omega_p)$$

Thirdly, from terms in cos 2 θ and sin 2 θ.

$$r^2 \ldots . p\,S\,(a_m\,r_m{}^{2n-2}\cos 2\,\theta_m) = S\,a_m\,.\,S\,(u_p{}^{2n-2}\cos 2\,\omega_p)$$

$$r^4 \ldots . p\,S\,(a_m\,r_m{}^{2n-4}\cos 2\,\theta_m) = S\,a_m\,.\,S\,(u_p{}^{2n-4}\cos 2\,\omega_p)$$

$$\cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot$$

$$r^{2n-2}\,.\,p\,S\,(a_m\,r_m{}^2\cos 2\,\theta_m) \quad = S\,a_m\,.\,S\,(u_p{}^2\cos 2\,\omega_p)$$

and

$$r^2 \ldots . p\,S\,(a_m\,r_m{}^{2n-2}\sin 2\,\theta_m) = S\,a_m\,.\,S\,(u_p{}^{2n-2}\sin 2\,\omega_p)$$

$$r^4 \ldots . p\,S\,(a_m\,r_m{}^{2n-4}\sin 2\,\theta_m) = S\,a_m\,.\,S\,(u_p{}^{2n-4}\sin 2\,\omega_p)$$

$$\cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot$$

$$r^{2n-2}\,.\,p\,S\,(a_m\,r_m{}^2\sin 2\,\theta_m) \quad = S\,a_m\,.\,S\,(u_p{}^2\sin 2\,\omega_p)$$

Proceeding in this way, we arrive at,

Lastly, from terms in cos n θ and sin n θ.

$$r^n \ldots . p\,S\,(a_m\,r_m{}^n\cos n\,\theta_m) \quad = S\,a_m\,.\,S\,(u_p{}^n\cos n\,\omega_p)$$

$$r^n \ldots . p\,S\,(a_m\,r_m{}^n\sin n\,\theta_m) \quad = S\,a_m\,.\,S\,(u_p{}^n\sin n\,\omega_p)$$

Now, to consider these in relation to the porism, we must find the number of equations which are produced in this series. The forms of them are given above; but it will be more convenient to recur to the original development in *Lemma* i., in order to discover this object of our inquiry.

1. From the terms clear of the θ, we have the alternate terms of a binomial (disregarding the special co-efficients in all cases, which do not affect the actual *number* of terms) of the $2n$th degree: that is, exclusive of the term r^{2n} which cancels in the porism, we have n terms.

2. In that which involves $\cos\theta$, we have the terms which were omitted in the first line, viz. n terms: and a similar number in that which involves $\sin\theta$.

3. In each of those involving $\cos 2\theta$ and $\sin 2\theta$ we have all those of the first line except the first and last; or $n-1$ in each case.

4. In each of those involving $\cos 3\theta$ and $\sin 3\theta$ we have all those of the second line except the first and last; or $n-2$ in each case.

Proceeding thus, we find at each successive step a diminution of one in the number of terms belonging respectively to the cosines and the sines of the multiple arcs: till in the last line we get one term involving $\cos n\theta$ and another involving $\sin n\theta$.

Again, the number of lines which involve cosines and sines is the same as the number of multiples of θ which are involved: that is, there are n lines which give the double of the number of powers of r in the co-efficients. Whence, including the first line, there will be $2(1+2+3+\ldots+n)+n=n(n+1)+n=n(n+2)$ equations of condition involved in the statement of the porism.

Now, the determination of a point involves two conditions: viz. such as will enable us to find u_p and ω_p. Whence these $n(n+2)$ equations will require that $\frac{1}{2}n(n+2)$ points should be porismatised, instead of $n+1$ as stated by Dr STEWART.

Again, *except n be even*, the number of conditional equations will be *odd;* and hence there will not be the requisite conditions for the determination of any number of points—either giving one condition too few, which would render the first point indeterminate, or one too many, which may be (and, generally, would be) contradictory amongst themselves.

We thus see that except n be even, the porism cannot be true, which is a limitation not laid down in the '*General Theorems*': and we see that when n is even, the number of lines to be porismatised is $\frac{1}{2}n(n+1)=p$, and not $n+1$ as there stated. The relation to be observed between m and n, will be discussed hereafter, when we come to consider the structure and solution of the equations of conditions themselves. It will hence be unnecessary to discuss this question further in this place; though we may remark, that this result, when applied to a former case (*Props.* 34, 35), is in keeping with the conclusions there obtained. For, in that case $n+1$ instead of $\frac{1}{2}n(n+2)$, n being equal to 2, or 3 instead of 4 points, are stated to be determinable.

The next eight porismatic propositions (46 to 53 inclusive) which close the series of porisms respecting points and lines, are, in fact, but varieties of the same general proposition, according to the positions of the given lines, the form of the number n, and the relative values of the magnitudes $a_1, a_2, \ldots, a_m$.

Propositions XLVI. to LIII. Porisms.

Let there be given m lines, and as many magnitudes $a_1, a_2, \ldots a_m$: *then there can be found p other lines, such, that if from any point whatever* Z, *there be drawn perpendiculars* $ZA_1, ZA_2, \ldots ZA_m$ *to the given lines, and* $ZB_1, ZB_2, \ldots ZB_p$ *to those found, we shall have, for values of n, subject to conditions which may be determined*

$$p\,S(a_m \,.\, ZA_m{}^n) = S\,a_m \,.\, S(ZB_p{}^n)$$

The perpendiculars being respectively,

$$ZA_m = \pm\{p_m - r\cos(\theta - \theta_m)\}$$
$$ZB_p = \pm\{q_p - r\cos(\theta - \omega_p)\}$$

we shall form the expression by means of *Lemma* ii. Also, as the process is general, we may, in conformity with previous practice, omit the double sign of these perpendiculars.

Taking, then, the conditional equations furnished by the developments of *Lemma* ii., we shall have in succession :—

First, from terms clear of θ.

$$r^0 \ldots . p \,.\, S(a_m p_m{}^n) = S\,a_m \,.\, S(q_p{}^n)$$
$$r^2 \ldots . p \,.\, S(a_m p_m{}^{n-2}) = S\,a_m \,.\, S(q_p{}^{n-2})$$
$$r^4 \ldots . p \,.\, S(a_m p_m{}^{n-4}) = S\,a_m \,.\, S(q_p{}^{n-4})$$
$$\ldots\ldots\ldots$$

Second, from terms in cos θ and sin θ.

$$r \ldots . p \,.\, S(a_m p_m{}^{n-1}\cos\theta_m) = S\,a_m \,.\, S(q_p{}^{n-1}\cos\omega_p)$$
$$r^3 \ldots . p \,.\, S(a_m p_m{}^{n-3}\cos\theta_m) = S\,a_m \,.\, S(q_p{}^{n-3}\cos\omega_p)$$
$$\ldots\ldots\ldots$$

and

$$r \ldots . p \,.\, S(a_m p_m{}^{n-1}\sin\theta_m) = S\,a_m \,.\, S(q_p{}^{n-1}\sin\omega_p)$$
$$r^3 \ldots . p \,.\, S(a_m p_m{}^{n-3}\sin\theta_m) = S\,a_m \,.\, S(q_p{}^{n-3}\sin\omega_p)$$
$$\ldots\ldots\ldots$$

Proceeding thus through $2\theta, 3\theta, \ldots\ldots n\theta$ we get at last to

$$r^{n-1} \ldots S\left(a_m\, p_m \cos(n-1)\,\theta_m\right) = S\, a_m \,.\, S\left(q_p \cos(n-1)\,\omega_p\right)$$

$$r^{n-1} \ldots S\left(a_m\, p_m \sin(n-1)\,\theta_m\right) = S\, a_m \,.\, S\left(q_p \sin(n-1)\,\omega_p\right)$$

and, lastly;

$$r^{n} \ldots\ldots S\left(a_m \cos n\,\theta_m\right) \qquad = S\, a_m \,.\, S\left(\cos n\,\omega_p\right)$$

$$r^{n} \ldots\ldots S\left(a_m \cos n\,\theta_m\right) \qquad = S\, a_m \,.\, S\left(\sin n\,\omega_p\right)$$

Our business is, in the first place, to find the value of p corresponding to the different forms of n. Now, it is familiarly known that for all the discussions regarding multiple arcs, all integer values of n may be considered under the forms of 4μ, $4\mu+1$, $4\mu+2$, and $4\mu+3$: but our purpose in the present instance will be effected with equal completeness by considering n to exist under the forms 2ν and $2\nu+1$ whilst the length of the discussion will be diminished about one-half.

1. *Let* $n=2\nu$.

The first line has all the even powers of r (r^0 included): the second has all the odd powers: the third has all the even powers but the lowest, r^0: the fourth all the odd powers but the lowest, r^1: the fifth has all the even powers but the two lowest, r^0 and r^2: the sixth has all the odd powers but the two lowest, r^1 and r^3: and so on.

Now, in the case supposed ($n=2\nu$) the first line will have $\nu+1$ terms, r^0 being even. But by the general structure of these theorems, the last term is cancelled from both sides of the equation of the porism: whence the number of terms in the first line is ν.

Pursuing the enumeration in the same manner as was done in the preceding proposition, we shall find that:

the second line has		ν	terms
... third	......	ν	...
... fourth	......	$\nu-1$	...
... fifth	......	$\nu-1$	...
... sixth	......	$\nu-2$	...
... seventh	......	$\nu-2$	...

and so on.

Again, there will be $2\nu+1$ lines; for all the multiple cosines are found in them from 0 to 2ν inclusive. Leaving out of view, for the moment, the first line, we shall have 2ν lines, which, in pairs, contain the same number of terms. The last two of these will be

$$\mathrm{L}\, r^{2\nu-1}\, p \cos(2\nu-1)\,(\theta-\theta_m),$$

$$\mathrm{M}\, r^{2\nu}\, p \cos 2\nu\,(\theta-\theta_m).$$

and each of these several equations will be doubled by the expansion of the cosines. Whence, for all the lines except the first we have $2\,(1+2+\ldots\ldots\nu)\,\frac{2\nu}{2}$ $=2\,(\nu+1)\,\nu$: or for the whole number of conditional equations $2\,\nu\,(\nu+1)+\nu=\nu(2\,\nu+3)$.

We are, therefore, in this case, led to the conclusion, that *except ν be even*, there will be either an indeterminateness or contradiction in the results of the hypotheses of the porism: that is, n if even must be of the form $4\,\mu$; and when this is fulfilled, $p=\mu\,(4\,\mu+3)$, instead of $4\,\mu+1$ as stated in the '*General Theorems*.'

When $\mu=1$ or $\nu=2$, we have $1\,\{4+3\}=7$, as before determined in reference to *Props*. 35–38.

2. *Let* $n=2\,\nu+1$.

The first line will have		$\nu+1$	terms
... second	......	$\nu+1$	...
... third	......	ν	...
... fourth	......	ν	...
... fifth	......	$\nu-1$	...
... sixth	......	$\nu-1$	...

and so on.

Whence, as there are $2\,\nu+2$ lines, having all the multiple cosines from 0 to $2\,\nu+1$, we shall have in all $(\nu+1)\,(\nu+2)$ terms. Also, with the exception of the first, they are doubled by the expansions of the cosines, and the entire number of equations of condition will be

$$2\,(\nu+1)\,(\nu+2)-(\nu+1)=(\nu+1)\,(2\,\nu+3)$$

When ν is *even*, this condition cannot hold in reference to Dr STEWART'S theorem; for it will give either an indeterminate or an impossible condition, as before. That is, the form $n=4\,\mu+1$ is precluded from the enunciation.

When ν is *odd*, the condition is capable of fulfilment: that is, when $n=4\,\mu+3$. In this case, if we denote ν by $2\,\varkappa+1$, we shall have $p=(\varkappa+1)\,(4\,\varkappa+5)$.

When $\nu=1$, the determination agrees with what was found in *Props*. 24–25; for then $\frac{1}{2}\,(1+1)\,(2\,.\,1+1)=5$.

We have thus obtained the number of equations to be fulfilled for the general forms of n, as well as the general forms of those equations themselves: and have shewn that the number of lines porismatised is erroneously in all the cases, and impossibly in some of them, laid down by Dr STEWART. For the case of n odd, we see that there can be no number of lines porismatised, except $n=4\,\mu+3$, in Propositions 50–53: and in 46–49, except $n=4\,\mu$.

Particular conditions will lessen this number. In 46–47 this takes place in the two cases in each proposition.

First, let the given lines be all parallel.

The equations in this case reduce to

$$p \,.\, S\,(a_m\, p_m^{\,2n}) \quad = S\, a_m \,.\, S\,(q_p^{\,2n})$$
$$p \,.\, S\,(a_m\, p_m^{\,2n-1}) = S\, a_m \,.\, S\,(q_p^{\,2n-1})$$
$$p \,.\, S\,(a_m\, p_m^{\,2n-2}) = S\, a_m \,.\, S\,(q_p^{\,2n-2})$$
$$. \quad . \quad . \quad . \quad . \quad . \quad . \quad .$$
$$p \,.\, S\, a_m\, p_m) \quad = S\, a_m \,.\, S\,(q_p)$$

which are $2\,n$ in number, determining $p=2\,n$, and all the lines found, parallel to the given ones.

Second, let the given lines meet in a point.

In this case $p_1=p_2= \ldots . \, p_m=0$, and we have only those terms left which involve the absolute terms of the co-efficients of the multiple cosines. Whence the equations become

$$p \,.\, S\,(a_m \cos 2\,n\,\theta_m) \quad = S\, a_m \,.\, S\,(\cos 2\,n\,\omega_p)$$
$$p \,.\, S\,(a_m \cos 2\,(n-1)\,\theta_m) = S\, a_m \,.\, S\,(\cos 2\,(n-1)\,\omega_p)$$
$$p \,.\, S\,(a_m \cos 2\,(n-2)\,\theta_m) = S\, a_m \,.\, S\,(\cos 2\,(n-2)\,\omega_p)$$
$$. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad .$$
$$p \,.\, S\,(a_m \cos 2\,\theta_m) \quad = S\, a_m \,.\, S\,(\cos 2\,\omega_p)$$

and

$$p \,.\, S\,(a_m \sin 2\,n\,\theta_m) \quad = S\, a_m \,.\, S\,(\sin 2\,n\,\omega_p)$$
$$p \,.\, S\,(a_m \sin 2\,(n-1)\,\theta_m) = S\, a_m \,.\, S\,(\sin 2\,(n-1)\,\omega_p)$$
$$p \,.\, S\,(a_m \sin 2\,(n-2)\,\theta_m) = S\, a_m \,.\, S\,(\sin 2\,(n-2)\,\omega_p)$$
$$. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad .$$
$$p \,.\, S\,(a_m \sin 2\,\theta_m) \quad = S\, a_m \,.\, S\,(\sin 2\,\omega_p)$$

Thus again, we have in this case $p=2\,n$; all the porismatic lines passing through the same point as the given ones.

The two conclusions in the latter particular cases agree with those which were found in the still more special case of *Props.* 15 *and* 19. For then $n=1$; and either Dr STEWART'S general form, or the one here deduced, as applied to that case, is precisely the same. In his view, $1+1=2$: in what is here found, $2\times 1=2$.

SECTION III. INDETERMINATE THEOREMS.

These are divisible, with a few miscellaneous exceptions already proved by DR STEWART in the work, into three Classes; and which we shall take in order.

1. Perpendiculars on the sides of a regular circumscribed polygon.

2. Lines drawn to the angular points of a regular inscribed polygon.

3. Perpendiculars upon lines which pass through a point and make equal angles with each other.

CLASS I.—*Regular Circumscribed Polygons.*

PROPOSITIONS V., XXIX., XL.

Let there be a regular polygon circumscribed about a circle whose radius is ρ, and let n be any number less than m; also, from any point Z, whose distance from the centre of the circle is r, let perpendiculars ZA_1, ZA_2, ... ZA_m be drawn to the sides of the polygon: then, if $t_0, t_1, t_2, \dots t_n$ denote the co-efficients of a binomial of the n^{th} degree, we shall have

$$S(ZA_m^{2n}) = m\left\{t_0\rho^{2n} + \frac{1}{2}\cdot t_2\rho^{2n-2}r^2 + \frac{1\,.\,3}{2\,.\,4}\cdot t_4\rho^{2n-4}r^4 + \dots\dots\right\}.$$

Without sacrificing generality, we may somewhat simplify our expressions by taking the line from Z to the centre of the circle as angular origin, and the centre itself as polar origin. Then, for the formation of the equations of the sides we shall obviously have,

$$p_1 = p_2 = \dots\dots = p_m = \rho$$

$$\theta_2 = \frac{2\pi}{m} - \theta_1,\ \theta_3 = \frac{4\pi}{m} - \theta_1,\ \dots\dots\ \theta_m = \frac{2(m-1)\pi}{m}$$

or the angles $\theta_1 . \theta_2, \dots\dots \theta_m$ in arithmetical progression whose common difference is $\frac{2\pi}{m}$.

Whence, since the general form of ZA_k^{2n} is

$$ZA_k^{2n} = \left\{\rho - r\cos\left(\frac{2k\pi}{m} - \theta_1\right)\right\}^{2n}$$

we shall find upon forming the sum of them by the expansion in *Lemma* ii., that all the terms involving the cosines disappear by virtue of *Lemma* iv. This sum is, therefore, reduced to the first line of the expansion for each of the perpendiculars; and these are all equal, and as many in number as there are perpendiculars; that is, the sum is m times the first line of the development, as stated in the proposition.

The conclusion just obtained is Dr STEWART's 40th proposition. If we put $n=2$ it becomes the 29th; and if also in this case $r=\rho$, we obtain the 26th. Also, finally, if $n=1$, we obtain the 5th proposition.

[See also note F.]

PROPOSITIONS XXII., XXVIII., XXXIX.

Let there be a regular polygon of m sides circumscribed about a circle, whose radius is ρ, and let n be any number less than m: then, if from any point in the circumference of the circle perpendiculars $Z A_1$, $Z A_2$ . . . . $Z A_m$ be drawn to the sides of the figure, we shall have

$$S\,(Z A_m{}^n) = m \,.\, \frac{1\,.\,3\,.\,5\,.\,.\,.\,.\,(2n-1)}{1\,.\,2\,.\,3\,.\,.\,.\,.\quad n} \,.\, \rho^n\,.$$

For, in this case, we have the general form of the nth power of the perpendicular

$$Z A_k{}^n = \rho^n \left\{1 - \cos\left(\frac{2k\pi}{m} - \theta_1\right)\right\}^n.$$

Expand each term, for all values of k from 1 to m, by *Lemma* iii., and we find from *Lemma* iv. that all the terms involving the cosines vanish of themselves, and the expansion is reduced for each perpendicular to its absolute term. As these are all equal, and m in number, we get at once the general form given by Dr STEWART.

The proposition just proved is Dr STEWART's 39th. When $n=4$, it becomes the 28th; and when $n=3$, the 22d.

PROPOSITION III.

Let perpendiculars $Z A_1$, $Z A_2$ . . . . $Z A_m$ be drawn from any point Z to the sides of a regular polygon of m sides described about a circle whose radius is ρ: then if the distance of Z from the centre of the circle be r, we shall have

$$S\,(Z A_m) = m\,\rho.$$

For the general form of $Z A_k$ is $\rho - r\cos\left(\frac{2k\pi}{m} - \theta_1\right)$; and we shall have, as in former cases, all the cosines mutually cancelling, giving the proposition stated.

PROPOSITIONS XXII., XXIII.

From any point Z, $(r\,\theta)$, draw perpendiculars $Z A_1$, $Z A_2$, ... $Z A_m$, to a regular polygon of m sides circumscribed about a circle whose radius is ρ: then we shall have

$$2\,S\,(Z A_m{}^3) = 2\,m\,\rho^3 + 3\,m\,\rho\,r^2.$$

For as before, expanding, and recollecting that $\theta_1, \theta_2, \ldots \theta_m$ are in arithmetical progression, having the common difference $\frac{2\pi}{m}$, the angular functions will vanish of themselves from the expression of the sum of the cubes: hence we have simply $\rho^3 + \frac{3\rho r^2}{2}$ for each perpendicular; and hence again

$$S\,(\mathrm{Z\,A}_m{}^3) = m\left\{\rho^3 + \frac{3\rho r^2}{2}\right\},$$

which is equivalent to STEWART'S form.

This is *Prop.* 23; but putting $r=\rho$ we have another proof of *Prop.* 22, viz.:

$$2\,S\,(\mathrm{Z\,A}_m{}^3) = 5\,m\,\rho^3.$$

CLASS II.—*Regular Inscribed Polygons.*

?XLII

PROPOSITIONS IV., XXVII., XLIV.

Let there be drawn from any point $\mathrm{Z}\,(r\,\theta)$ *lines to all the angular points of a polygon of m sides inscribed in a circle whose radius is* ρ, *namely,* $\mathrm{Z\,A}_1$, $\mathrm{Z\,A}_2$, $\ldots\,\mathrm{Z\,A}_m$ *: then we shall have (n being any integer less than m, and* $t_0, t_1, \ldots t_n$, *as before)*

$$S\,(\mathrm{Z\,A}_m{}^{2n}) = m\,\{t_0{}^2\,\rho^{2n} + t_2{}^2\,\rho^{2n-2}\,r^2 + t_4{}^2\,\rho^{2n-4}\,r^4 + \ldots\}.$$

The general form of these values is

$$\mathrm{Z\,A}_k{}^{2n} = \{\rho^2 - 2\,\rho\,r\cos(\theta - \theta_k) + r^2\}^n.$$

Expand and add; then, since $\theta_1, \theta_2, \ldots \theta_m$ are in arithmetical progression, having the common difference $\frac{2\pi}{m}$, all the angular functions will vanish from the sum. Wherefore only the first line of the expansion in *Lemma* i. will remain, and that one for each Z A. Wherefore the sum of them is m times that line; and the Proposition 42 follows at once.

When $n=2$ it becomes *Proposition* 27 : and when also, $n=\rho$ it becomes *Proposition* 4.

Dr STEWART remarks, p. 38, that Prop. 2 is a particular case of the porismatic *Prop.* 9. This will be apparent if we consider that the origin is, in this case, the centroïd of the angular points; and hence that all the cosines involved in the mutual expressions cancel among themselves. We shall, however, return to this subject hereafter.

Propositions XXVI., XLI.

Let there be a regular polygon of m sides inscribed in a circle whose radius is ρ, and let n be any number less than m; and from any point Z in the circumference of the circle, let lines be drawn to all the angular points of the polygon: then we shall have

$$S\,(\mathrm{Z\,A}_m^{\ 2n}) = m\,\frac{1\,.\,3\,.\,5\,.\,\ldots\,.\,(2n-1)}{1\,.\,2\,.\,3\,.\,\ldots\,.\,n}\,.\,2^n\,.\,\rho^{2n}.$$

For in this case $r=\rho$; and the expression for $\mathrm{Z\,A}_k^{\ 2n}$ takes the form

$$\mathrm{Z\,A}_k^{\ 2n} = \{2\rho^2 - 2\rho^2\cos(\theta-\theta_k)\}^n$$
$$= 2^{2n}\rho^{2n}\{1-\cos(\theta-\theta_k)\}^n.$$

Expanding the binomial factor by *Lemma* iii., and keeping in view that $\theta_1, \theta_2, \ldots\ldots \theta_m$ are in arithmetical progression, having the common difference $\frac{2\pi}{m}$, we see that all the angular functions vanish, by *Lemma* iv. Whence the sum is reduced as before to m times the term clear of the angles, and we thus have

$$S\,(\mathrm{Z\,A}_m^{\ 2n}) = \frac{1}{2^n}\,.\,\frac{1\,.\,3\,.\,5\,.\,\ldots\,.\,(2n-1)}{1\,.\,2\,.\,3\,.\,\ldots\,.\,n}\,.\,2^{2n}\rho^{2n}$$
$$= \frac{1\,.\,3\,.\,5\,.\,\ldots\,.\,(2n-1)}{1\,.\,2\,.\,3\,.\,\ldots\,.\,n}\,.\,2^n\,\rho^{2n}.$$

This is *Proposition* 41, as proposed by Dr Stewart. When $n=2$ it becomes *Prop.* 26.

Class III.—*Lines making equal Angles.*

Propositions XIV., XXXIV., XLV.

Let there be m lines meeting in a point, and making all the angles round the point equal, and let n be any number less than m: then if from any point Z, whose distance from the common point of section is r, perpendiculars be drawn to all the lines, $\mathrm{Z\,A}_1, \mathrm{Z\,A}_2, \ldots \mathrm{Z\,A}_m$, *we shall have*

$$S\,(\mathrm{Z\,A}_m^{\ 2n}) = \frac{m}{2^n}\,.\,\frac{1\,.\,3\,.\,5\,.\,\ldots\,.\,(2n-1)}{1\,.\,2\,.\,3\,.\,\ldots\,.\,n}\,.\,r^{2n}.$$

For simplification, take the line from Z to the given intersection as angular origin, that intersection being the polar origin. Then $\theta_1, \theta_2, \ldots\ldots \theta_m$ of the general equation are the angles made with this line by the perpendiculars to the given lines. Also, in the equations of the given lines $p_1 = p_2 = \ldots = p_m = 0$; and $\theta_1, \theta_2, \ldots\ldots \theta_m$ are in arithmetical progression, having the common difference $\frac{2\pi}{m}$. Wherefore, the general form of the expression for $\mathrm{Z\,A}_m^{\ 2n}$ is

$$\mathrm{Z\,A}_m^{\ 2n} = r^{2n}\cos^{2n}\frac{2k\pi}{m}.$$

Expand, therefore, in the ordinary form, and bearing in mind that $2n$ is even, and the angular functions vanish, we have for each value of k from 1 to m inclusive, the expression

$$\frac{1 \cdot 3 \cdot 5 \ldots (2n-1)}{1 \cdot 2 \cdot 3 \ldots n} r^{2n}.$$

and as these are m lines, we have at once the expression in question.

This is *Prop.* 45: also, when $n=2$, it becomes *Prop.* 34; and when $n=1$ it becomes *Prop.* 14.

NOTES UPON THE PRECEDING DISCUSSION.

Note A, page 574.

The first case of the solution of any one of Dr Stewart's *General Theorems* being published, that I remember to have met with, is that of the 41st Proposition, by Professor Playfair, in his paper on the Arithmetic of Imaginary Quantities, *Phil. Trans.* 1777; and I am not aware, impressed as he was with the great beauty of these propositions, that he published anything more on the subject, and even this one is taken up incidentally.

Dr Small, in the *Edinb. Trans.*, vol. ii., gave solutions of Propositions 9, 10, 11, 12, 13, 14, 15, 16, 17, 19, 30, 31.

Professor Lowry gave solutions of 9, 10, 11, 12, 13, 14, 15, 16, 17, 18, 19, 20, 21, 22, in vols. i. ii., O.S., of Leybourn's Repository; and annexed to them several interesting, but tolerably obvious deductions.

Mr Swale of Liverpool also gave, in the same work, solutions of 15, 16, 17, 18, 19, 20, 21.

The three last-mentioned authors treat the subject by methods purely geometrical; but Dr Small has been inconsiderately censured for a "lack of geometrical purity," merely because he *mentions* the "centre of gravity," notwithstanding he builds nothing upon it deduced from its *physical* character. (*Repos.* i. p. 131.) Geometrical purity, however, is not vitiated by the use of an injudicious term, but by the employment of *methods* which are unrecognised by geometry, or inconsistent with those which are recognised. The standard itself was originally arbitrary; but being once recognised and generally admitted, it is the proper criterion by which to judge of the purity of geometrical processes. Taking, however, the strictest view of the subject, I confess I know of no flaw in Dr Small's argument; and there is no doubt that to Dr Small's paper we are in reality indebted for all that has been effected concerning the porismatic part of Dr Stewart's Theorems. It may be further remarked, too, that the ordinary sense of the word *geometrical* is altogether inapplicable to the greater part of theorems themselves, since they relate to magnitudes which the ancient geometry does not recognise (viz., to fourth and higher powers of lines); and hence, as far as purity is concerned, it seems to savour of the "gnat and camel" character, to affect a rigid adherence to even the forms of the ancient geometry in attempting their solution. Dr

Stewart did not set such an example by attempting to antiquate the forms of their enunciation; and he does not even call them *geometrical theorems*, though, as they relate to the properties of geometrical figures, he might have done so without impropriety. Whether he even *obtained* them by geometrical considerations, in the first place, is open to question, when we turn to what Professor Playfair remarks (*Ed. Trans.*, i. p. 60) concerning the probable origin of his inquiries on this subject. In fact, his being in possession of Simson's definition of the porism (which may be inferred from his formally distinct enunciations of the porismatic part of his propositions), I do not think there is a single step in the present paper, which it would have been at all improbable that Dr Stewart could readily take. Any slight mistake in estimating the number of final equations would create no surprise, when we recollect how much he was in the habit of "thinking out" his conclusions without the aid of writing. Possibly, therefore, Dr Stewart's investigations were not much unlike those of the present paper, in their essential character. But to return:—

Mr Glenie, of the Royal Artillery, gave, in the Edinburgh Transactions (vol. vi.), demonstrations of nearly all the *indeterminate theorems*, except those proved by Dr Stewart himself, and the 41st theorem. Of this last, he afterwards gave a proof in a small tract (1813.)

Mr Glenie's course of investigation is remarkably elegant, and it discloses many curious and interesting properties of the circle, of which uses, generally unsuspected, may yet be made. This able geometer has, however, fallen into the prevailing error on the subject of geometrical purity: that $\frac{r^n}{r^{n-3}}$ expresses an idea recognised by the ancient geometry; but it in no degree vitiates his reasonings as demonstrations, though it throws them, as all demonstrations of these theorems *must* be thrown, into the domain of algebra.

Mr Babbage, after paying a high and deserved tribute to the genius of Dr Stewart, gave (*Quarterly Journal of Science*, vol. i.) investigations of nearly all the *indeterminate theorems*, by means of trigonometry. They differ from the investigations given in this paper mainly in the forms of expansion employed; and I gladly avail myself of the opportunity of acknowledging my obligations to that paper, for some useful modifications of my own primary processes respecting this class of propositions.

The late Mr Thompson published (in the *Newcastle Magazine*, 1826-27-28) investigations of a considerable number of these theorems. He adopted the mixed method—that is, the employment of the ancient geometry, algebra, and trigonometry, as best suited his immediate purpose. These solutions bear ample testimony to the mathematical powers of their author; but they are, unfortunately, so disfigured by an awkward and irregular notation, and bad style of printing, as to be not only generally unintelligible to the ordinary reader, but, in many cases, to almost defy interpretation by those who look into the subject with the greatest care. I was much struck, when I first met with this work (three or four months ago), with the near approach made, in some of his demonstrations, to the use of the principle which forms one of the foundations of this paper—the formation of conditional equations. He seems, however, to use it as a matter of convenience rather than as a principle—as something that may answer a special purpose, rather than as a general method founded in the nature of the algebraic analysis, and applying to all possible cases.

Owing to the circumstances before mentioned, had my own investigations been in a less complete state than they were at that time, Mr Thompson's papers could have afforded me no assistance; but I gladly embrace this opportunity of publicly recording the high estimate

I form of the abilities of the author, as evinced by his varied correspondence with the most valuable English periodicals devoted to mathematical researches.

My old friend and former pupil, Mr Leslie Ellis (in the *Cambridge Mathematical Journal*, May 1841), has proved a considerable number of the indeterminate theorems which relate to inscribed and circumscribed polygons, by means of a very remarkable theorem (which he has there investigated), which reduces the sums of the specified powers of the lines, or perpendiculars, to a definite integral. The following is his theorem; but, for the manner of employing it, reference must be made to the paper itself.

$$f\phi + \ldots + f\left(\phi + \frac{n-1}{n} 2\pi\right) = \frac{n}{2\pi}\int_0^{2\pi} f\phi\, d\phi.$$

In a letter to me, Mr Ellis also suggests the application of the same process to spherical polygons. It may also, evidently, with slight modifications, be applied to regular polyhedrons, inscribed and circumscribed to a sphere.

Should any other English authors have discussed these theorems, their works are unknown to me, and that, after taking much trouble to discover all that had been attempted relative to them. I am not aware of any writer on the continent who has distinctly dwelt upon them, except M. Chasles, in his *Aperçu Historique des Méthodes en Géométrie*, though Lhuillier, Carnot, and many other distinguished continental geometers have made occasional reference to Simson's Porisms and Stewart's Theorems. Both works are, however, by them considered merely as relating to indeterminate theorems. Chasles forms the highest estimate of Stewart's researches; but as his view of the ancient porism is so opposed to Dr Simson's, he was not likely to be led to any method of investigation adapted to the discussion of these propositions:—in fact, he does not offer any. He, however, enunciates the "extension" of two of the general theorems (pages 353–54), namely, the 44th and 53d. These extensions will be true only when the original theorems respecting the numbers of porismatic points and lines, as given by Dr Stewart, are corrected. In this case, all the equations of condition for different values of δ are contained amongst those for $\delta = 0$; and hence the porismatic lines, which fulfil the conditions for the value $\delta = 0$, fulfil those, also, for all higher values within the prescribed limits; although the conditions of the porisms might be fulfilled with a smaller number of porismatic points and lines for those cases.

Chasles's extensions are, in our notation,

$$p.\, S(a_m . \mathrm{ZA}_m{}^{2(n-\delta)}) = S a_m . S(\mathrm{ZB}_p{}^{2(n-\delta)});$$
$$p.\, S(a_m p_m{}^{(n-2\delta)}) \quad = S a_m . S(q_p{}^{(n-2\delta)});$$

the former being that of *Prop.* 44, and the latter that of *Props.* 49 and 53, as n is odd or even; and δ being taken from 0, 1, 2, ... $\frac{n+1}{2}$ in the former case, and from 0, 1, 2, $\frac{n-2}{2}$ in the latter.

Finally, it may be remarked that nearly all the general theorems have analogous ones in respect to points, lines, or planes situated arbitrarily in space, the numbers of porismatic ones being properly chosen. Several of them, too, have corresponding properties (with a different porismatic number, of course), with respect to the hyperboloïd of one sheet; but this is not the proper time for details on any collateral subject.

Note B.

On Lemmas I, II, III, pp. 578–81.

I am not aware that the general forms of the co-efficients of the multiple cosines have been given by any author; and, indeed, since they are not required in any class of inquiries which fall under the general objects of science, it is scarcely likely that they should have been an object of research. Cases, however, having much analogy to them, are sufficiently well known; and the method by which they are treated, naturally pointed in the direction by which these could be obtained when occasion called for them.

By taking $r_1 = r$ in *Lemma* i., and $p = r$ in ii., and comparing the terms in the two cases clear of the cosines, with the corresponding ones in iii., we obtain two elegant formula, first given by Euler in the *Acta Acad. Petropolitanæ*, 1781, viz.:—

$$t_0^2 + t_1^2 + t_2^2 + \ldots + t_{n-1}^2 + t_n^2 = \frac{2n(2n-1)(2n-2)\ldots(n+1)}{1\,.\,2\,.\,3\,.\,\ldots\,.\,n}$$

$$t_0 + \frac{1}{2}\,t_2 + \frac{1\,.\,3}{2\,.\,4}\,t_4 + \frac{1\,.\,3\,.\,5}{2\,.\,4\,.\,6}\,.\,t_6 + \ldots = \frac{2n(2n-1)(2n-2)\ldots(n+1)}{1\,.\,2\,.\,3\,.\,\ldots\,.\,n}$$

Note C.

On Props. IX, X, p. 582.

This theorem is well known, and has been demonstrated in a great variety of ways, *as a property of the centroïd;* though, probably, it has not been before considered in the light of a porism, nor, consequently, investigated as such. It was suggested by Carnot (*Géom. de Pos.* p. 326), that it would offer some advantage to take this point as the origin of co-ordinates; and as it will illustrate the manner in which, under particular circumstances, the porismatic proposition may become an ordinary indeterminate, the same property is here investigated, with the centroïd taken as origin of polar co-ordinates.

$$\begin{aligned} a_1\,.\,A_1Z^2 &= a_1 r^2 - 2a_1 r r_1 \cos(\theta - \theta_1) + a_1 r_1^2 \\ a_2\,.\,A_2Z^2 &= a_2 r^2 - 2a_2 r r_2 \cos(\theta - \theta_2) + a_2 r^2 \\ &\ldots\ldots\ldots \\ a_m\,.\,A_mZ^2 &= a_m r^2 - 2a_m r r_m \cos(\theta - \theta_m) + a_m r_m^2 \end{aligned}$$

Now, since the origin is the centroïd, the middle vertical column on the right side is zero; and hence

$$S(a_m\,.\,A_mZ^2) = S\,a_m\,.\,r^2 + S(a_m r_m^2)$$

Note D.

On Props. XI, XII, p. 583.

These may be proved, as, indeed, most of the *earlier* propositions can be, very neatly, but at greater length, by means of rectangular co-ordinates.

Let $\alpha_1\beta_1$, $\alpha_2\beta_2$, ... $\alpha_m\beta_m$ denote the given points, xy the arbitrary point Z, $\alpha\beta$ the

centre of the porismatic circle, ρ its radius, and K the inclination of the arbitrary diameter to the axis of x. Then we shall have

$$\left.\begin{array}{l} a_1 \,.\, \mathrm{A\,Z}^2 = a_1\{x^2+y^2-2\,\alpha_1\,x-2\,\beta_1\,y+\alpha_1{}^2+\beta_1{}^2\} \\ a_2 \,.\, \mathrm{A}_2\,\mathrm{Z}^2 = a_2\{x^2+y^2-2\,\alpha_2\,x-2\,\beta_2\,y+\alpha_2{}^2+\beta_2{}^2\} \\ \quad . \quad . \quad . \quad . \quad . \quad . \quad . \\ a_m \,.\, \mathrm{A\,Z}^2 = a_m\{x^2+y^2-2\,\alpha_m\,x-2\,\beta_m\,y+\alpha_m{}^2+\beta_m{}^2\} \end{array}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

Again, the equations of the porismatic circle and its arbitrary diameter will respectively be

$$(y-\beta)^2+(x-\alpha)^2=\rho^2 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

$$y-\beta=(x-\alpha)\tan \mathrm{K} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

Denote, for the moment, the points X, Y by $x'y'$ and $x''y''$: then from (2, 3) we get their values

$$x' = \alpha+\rho\cos \mathrm{K} \qquad\qquad x''=\alpha-\rho\cos \mathrm{K}$$

$$y' = \beta+\rho\sin \mathrm{K} \qquad\qquad y''=\beta-\rho\sin \mathrm{K}$$

Whence

$$\mathrm{X\,Z}^2=\{(\alpha-x)+\rho\cos \mathrm{K}\}^2+\{(\beta-y)+\rho\sin \mathrm{K}\}^2,$$

$$\mathrm{Y\,Z}^2=\{(\alpha-x)-\rho\cos \mathrm{K}\}^2+\{(\beta-y)-\rho\sin \mathrm{K}\}^2;$$

And therefore

$$\mathrm{X\,Z}^2+\mathrm{Y\,Z}^2=2\{x^2+y^2-2\,\alpha\,x-2\,\beta\,y+\alpha^2+\beta^2+\rho^2\} \quad . \quad . \quad . \quad . \quad (4)$$

which, as in the former solution, is independent of the value of K.

With the elements (1) and (4) form the equation of the porism; arrange the terms in reference to the arbitrary quantities x and y; cancel $S\,a_m\,.\,(x^2+y^2)$ and equate to zero the remaining co-efficients in respect of x and y; then there are given the three following conditional equations for α, β, and ρ.

$$a_1\,\alpha_1+a_2\,\alpha_2+\ldots.+a_m\,\alpha_m=S\,a_m\,.\,\alpha \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

$$a_1\,\beta_1+a_2\,\beta_2+\ldots.+a_m\,\beta_m=S\,a_m\,.\,\beta \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

$$a_1\,(\alpha_1{}^2+\beta_1{}^2)+\ldots.\;a_m\,(\alpha_m{}^2=\beta_m{}^2)=S\,a_m\,.\,(\alpha^2+\beta^2+\rho^2) \quad . \quad . \quad . \quad (7)$$

From (5), (6), we have

$$\alpha=\frac{S\,(a_m\,\alpha_m)}{S\,a_m},\ \text{and}\ \beta=\frac{S\,(a_m\,\beta_m)}{S\,a_m};$$

which are the co-ordinates of the centroïd; and which point is, therefore, the centre of the porismatic circle. Also, from (7) we have

$$\rho^2=\frac{S\,\{a_m\,(\alpha_m{}^2+\beta_m{}^2)\}-S\,a_m\,.\,(\alpha^2+\beta^2)}{S\,a_m}$$

That this value is always real, may be readily shewn by actual substitution of the values of α, β; but it readily follows from transforming the origin to the centroïd, whose co-ordinates are $\alpha\,\beta$. For since

$$S\,a_m\,.\,\{(\alpha-\alpha)^2+(\beta-\beta)^2\}=0,$$

the value of ρ^2 becomes

$$\rho^2=\frac{S\left\{a_m\,\{(\alpha_m-\alpha)^2+(\beta_m-\beta)^2\}\right\}}{S\,a_m};$$

and every term of this being positive, their sum must be so, and the value of ρ real.

Note E.

On Props. XXXV, XXXVIII, p. 590.

To remove any latent suspicions that may be entertained of the correctness of the determination of the value of p, in consequence of any one of the former equations (29 – 32) being virtually contained in the other three, it will be desirable to examine the consequences of such an hypothesis in detail.

On the hypothesis of $p=3$, and the fourth equation being contained in the three others, we can find the value $q_1^4+q_2^4+q_3^4$ by means of the other three equations; and if that hypothesis be correct, we ought to obtain the same value of this function as is given in (32).

Put S_1, S_2, S_3, S_4, for the sums of the first, second, third, and fourth powers of the roots of the equation which results from the elimination of q_2 and q_3 from (29, 30, 31). Then it is sufficiently well known that

$$6\,S_4=3\,S_1\,S_3+3\,S_2\,(S_2-2\,S_1^2)+S_1^4$$

Moreover, as the origin of co-ordinates is altogether arbitrary in the investigation by which those equations are obtained, we are at liberty to take it so as to fulfil the condition

$$q_1+q_2+q_3=\frac{3\,S\,(a_m\,p_m)}{S\,a_m}=0\text{ ; or }S_1=0.$$

This will convert the equation above into

$$6\,S_4=3\,S_2^2,\text{ or }2\,S_4=S_2^2.$$

Substituting in this the values of S_2 and S_4 from (30, 32), we have

$$2\,S\,a_m\,.\,S\,(a_m\,p_m^4)=\{S\,(a_m\,p_m^2)\}^2$$

an equation which is manifestly incorrect.

The same general result might have been obtained without any hypothesis regarding the origin of co-ordinates; but the expressions would have been more complex, and the examination of the results more troublesome.

We are hence (even without an actual solution of the several equations) entitled to infer, that the case of three lines only (or $p=3$), as propounded by Dr Stewart, fulfilling the conditions of the porism, is inaccurate. Moreover, as a general theorem ought to be true in all its particular cases, it follows that the general statement given amongst the "*Theorems*" is also inaccurate.

Note F.

On Props. XL. and XLII, p. 594.

The connection between the porismatic and indeterminate proposition is capable of a striking exemplification, by a comparison of these propositions with the equations of the corresponding porisms. Owing, however, to the already extended space required for printing the present part of the discussion, that exemplification must be deferred, as well as some other necessary remarks on the porismatic proposition. See, however, Note C., which is a case in point.

XXXIX.—*On a Remarkable Oscillation of the Sea, observed at various places on the Coasts of Great Britain, in the first week of July* 1843. By David Milne, Esquire.

(Read 19th February 1844.)

This phenomenon presented a remarkable interference with those laws which govern the ordinary movements of the ocean. It occurred at one place whilst the tide was flowing, at another whilst it was ebbing; in some cases, producing a sudden retrocession of the waters,—and in others, as sudden an impulse of them on the shore.

The period during which the sea thus continued to retreat and rise respectively, was generally from ten to fifteen minutes. It then made a momentary halt—after which, it began to flow in the opposite direction, and which it continued to do, for about the same period which characterised its previous movement.

In this state of alternate flux and reflux, the sea was at most places observed to continue, for three or four hours together.

The first day on which the phenomenon was observed, was Wednesday the 5th July 1843. During the three following days, the oscillation was at different places perceptible; but in no case so distinctly, as on the 5th July.

The same phenomenon appears to have frequently occurred before, and to have occasionally given rise to discussions as to the cause of it. There will be found a good many instances recorded in the earlier volumes of the Royal Society of London, as well as in the Annual Register, and other such periodicals. In several cases, the oscillation has been distinctly traced to submarine earthquakes. The Lisbon earthquakes of 1755 and 1761 undoubtedly produced on the coasts of Great Britain a flux and reflux of the sea, in many respects similar to that which forms the subject of the present notice; and as, on other occasions, no other apparent cause suggested itself for these oceanic disturbances, there seems to have been a general acquiescence in the opinion that they were produced by the same cause. In a short notice given in the Edinburgh Philosophical Journal, of the oscillations which occurred last July, I observe that earthquakes are suggested as the cause.[1]

Deeming this explanation unsatisfactory, and thinking it of use to preserve some record of a phenomenon, remarkable in itself, and not hitherto satisfactorily explained, I have drawn up an account of the principal facts, and will venture to suggest some views as to the probable cause of them.

[1] Edinburgh New Philosophical Journal for January 1844, p. 188.

FACTS OBSERVED.

I. The following are the places, where I have ascertained that the phenomenon was observed *on the 5th July;* and in mentioning each place, I shall notice the most material circumstances attending it which occurred there:—

At *Newlyn*, in Mountsbay, Cornwall, at noon, about an hour and a half after high water, the sea suddenly retired to the depth of 3 or 4 feet, rushing out to the distance of at least half a mile. It then returned, in the same state of agitation, to its former level. The time occupied in each of these movements, was ten or fifteen minutes.

This ebbing and flowing was observed four times; the duration of each movement being throughout nearly the same.

The current produced by the flux and reflux, was about three miles an hour.

At *Penzance*, Cornwall, about $11^h\ 30'$ A.M., three currents were observed flowing parallel to one another, in opposite directions, and running at the rate of four or five knots an hour. The agitation increased from noon till half-past 12, and at 1 P. M. its violence was not diminished.

At *Marazion*, three miles east of Penzance, one of the influxes was observed about one o'clock. The sea then rushed in from the south to the depth of 4 or 5 feet, and from the distance of about 50 yards, and almost immediately after retired to its previous level, occupying about ten minutes in each movement. Between 2 and 3 P.M., and after the tide had entirely left the causeway at Marazion, the sea returned and covered the central parts of it.

At each of the piers of *Mousehole*, *Newlyn*, the *Mount*, and *Portleven*, a most violent eddying current was observed for two or three hours, so remarkable as to arrest the attention of all, and such as had not occurred during the last fifty years. The boats were whirled about by it, in all directions. The vertical height to which the sea rose and fell at those piers, was from 2 to 4 feet, and each retreat and advance respectively occupied about 10 or 15 minutes.[1]

At *Plymouth*, the oscillation was observed a little earlier in the day. Captain WALKER, R.N., the intelligent harbour-master there, writes to me, "I happened to be standing on the pier, at the entrance of Sutton Pool, I think about 11 o'clock, a little after high water. My people were in the boat at the entrance of the Pool, which is about 90 feet wide. I noticed, that all at once, the water ran out of the Pool, carrying my boat and others along with it, and in a minute or two, it had again ceased to ebb. My coxwain, who was in my boat, tells me, that the water first ran into the Pool, carrying a barge and boats along with it, and again ran out faster than it ran in."

At *Dunbar*, at the mouth of the Firth of Forth, the phenomenon was observed about 6 P.M., or rather later. The sea was observed to flow up in the first

[1] The notices here given in regard to Mountsbay, are abbreviated from an account which appeared in the Literary Gazette of 15th July 1843.

instance, and about 18 inches above its former level. The oscillation was observed for two or three hours.

At *North Berwick*, about 10 miles west of Dunbar, the flux and reflux was observed between one and two o'clock P.M.; and it was noticed twice afterwards in the course of the day, at short intervals of about ten minutes.

At *Arbroath*, the flux and reflux were first observed about 5 P.M.

During the making of the tide (it being high water on the 5th July at $8^h\ 4'$ P.M.), the attention of the harbour master and others was drawn to the unusual motion of the sea, which made the vessels shift from their usual berths, and suggested the propriety of fastening them by additional moorings. This was found necessary, from the manner in which they were driven about by the currents.

When the phenomenon was first observed, the sea flowed in and rose from 18 to 24 inches vertically, and poured a strong current into the harbour. The water stood at the level thus attained, for more than five minutes, and then ran violently out of the harbour during the succeeding ten minutes, till it reached a level, nearly 2 feet lower than that previously reached. There it remained for some time, and then began to rise again as before. Each rise was nearly 1 foot higher, and each fall a foot lower, than the medium level of the tide at the time.

This flux and reflux was at its greatest height about 8 P.M., the time of high water. The sea was then calm, the wind being from the north or NW. The height of the tide was a little greater than ordinary, but not exceeding a foot above its usual level.

The oscillation continued the whole of that tide, and also during the next day; but the rise and fall of the water decreased, being only from 15 to 18 inches. It gradually decreased during that day, and on the 7th July. On the 8th July it became imperceptible.

Allowing 10 minutes for each rise and each fall, and 5 minutes succeeding each rise and fall, it may be estimated, that the fluxes attained their greatest height at intervals of 30 minutes.

These are the only accounts received by me, which can be depended on for the occurrence of the phenomenon, and its attendant circumstances, on the 5th July. It was noticed also at Eyemouth, but at what hour is uncertain.

On the 6th July, besides Arbroath, which has just been mentioned, the oscillation was perceived at the following places:—

At *St Andrews*, in the east coast of Fife, the oscillation was perceived in the morning. The water (according to the account given by a pilot there) rushed into the harbour as if it had been poured out from a number of sluices, and immediately retreated with the same violence—this continuing for a considerable time before and after full tide, which took place at $9^h\ 8'$ A.M. The

salmon fishers on the SE. side of the bay stated, that on the same day their nets were driven in one direction, and instantly after in an opposite one, owing to a sudden flowing and retiring of the sea.

At *Baltasound* (Shetland) Mr EDMONSTONE writes me, that he had been informed of the oscillation having been seen in the harbour there on the 6th July, and also on the day following, though in a less degree.

"On the 6th July it was noticed between 10 and 12 A.M. The tide was ebbing. A boat had grounded, and the men were preparing to get out (of the boat?), when the tide ran rapidly in, floated the boat again, and carried it onwards to land several yards farther."

"The horizontal distance that the water reached, was about 50 to 60 yards; the vertical about $3\frac{1}{2}$ feet."

"The flood retired again speedily to its first limit; and this alternation went on five or six times, perhaps oftener, for the observers did not wait until they ascertained that all irregularity had ceased."

"The wind was NE.; the weather dry and mild through that day and night. The boats went off to the deep sea fishing, and experienced nothing unusual. The sea was calm."

"The appearances I have stated were noticed in other harbours and inlets in this island. My accounts from *Lerwick* are as follows :—'The time the circumstance was first observed was noon, on 5th July,[1] low water or nearly so. The first feature was a flowing or returning of the sea; vertical depth about 3 feet; ebbing and flowing repeated perhaps seven or eight times. Retiring was the last feature. About ten or twelve minutes, might be the periods between the different turns of the tide.'"

At the *Start* Lighthouse (in the Orkneys) the oscillation appears to have been noticed first at 3 A.M. on the 6th July. Mr LYALL, the lighthouse-keeper there, states that "the flowing and ebbing continued for two days, but that after 3 P.M. on the 7th July it diminished. The interval between the ebbing and flowing, was from five to ten minutes. On the 6th July the sea was calm, but on the 7th July it was much agitated. On the 7th July, during the forenoon, some people were going to the cod fishing, who relate that their boats were floated and grounded several times, by the rising and falling of the sea, before they could get out. The same phenomenon was observed in Otterswick. There was a great deal of thunder and rain on the night of the 5th July."[2]

[1] I think that there is a mistake here for the 6th July, for on the 5th July it was low water at $10^h\ 36'$ A.M., and on the 6th July at $11^h\ 41'$ A.M.

[2] For this report, and all the others from lighthouse-keepers, quoted in the subsequent parts of this paper, I am indebted to Mr STEVENSON, the engineer of the Northern Light Commissioners. And I take this opportunity of acknowledging the readiness and liberality with which these authorities undertook to obtain for me returns from all the lighthouse-keepers under their superintendence.

On the 7th July, the flux and reflux were observed at the following places:—

At *Dundee*, about $10^h\ 20'$ A.M., about $5'$ after high water, by which time the tide had ebbed about 3 inches, the sea suddenly returned and flowed 5 inches. It then returned to its former level.

At *Leith*, at $9^h\ 6'$ A.M. (which was an hour and a quarter before high water), the sea suddenly rushed into the harbour, and raised the general level of the water, as shewn by the tide gauge, 4 inches, but as estimated by the spectators at least a foot. The reaction was equally violent, the waters shortly after rushing out with great velocity. At 5 P.M. a similar event took place.

At *Carnoustie*, a few miles south of Arbroath, where the shores are flat and sandy, a gentleman was, about 8 A.M., in a machine bathing; whilst putting on his clothes, he observed the sea suddenly retire, though it was then flood tide, so that the wheels which were previously in water about 2 feet deep, were left almost dry. The sea retired at least 100 yards. In about three minutes afterwards, the sea began to return with great violence, and entered the machine, so as to oblige the occupant of it to leap on the seat, clothes in hand. The machine now ran some risk of being floated; but fortunately the circumstance was perceived by the man in charge of the machine, who, without waiting for the usual signal, rode in with a horse, and drew the alarmed bather ashore. The sea again retired, and again flowed as before. The distance to which it retired was at least 100 yards, and the depth to which it feil, was about 3 feet. At this time the sea was perfectly smooth, and there was little or no wind.

At *Campbelton*, near Fort George, some carters were, at low water (at $2^h\ 24'$ P.M.), loading a vessel lying dry on the beach with timber, when suddenly the tide advanced 50 or 60 yards towards the shore, surrounding men and horses to a depth of about 18 inches. After a short space it as suddenly retired, and then flowed up again as before. This flux and reflux was repeated several times.

At *Lerwick*, and other places along the east coast of Shetland, the phenomenon was noticed. I extract the following accounts from a report made on the subject to Mr Stevenson of the Northern Lights, by David Laughton:—

"An extraordinary rising and falling of the sea was observed at the Docks near Lerwick, on Friday 7th July, near low-water, betwixt 12 and 1 P.M. The water fell rapidly to a very low ebb, and immediately thereafter it rose 2 feet perpendicular, when it again receded twice, and advanced twice, in the space of half an hour." "A person at the Dock, observed it suddenly emptied of water, but which in a few minutes again returned, filling the basin completely. This was repeated several times in the course of an hour."

"At *Riva-head* (a small promontory two miles north of Lerwick), some boats, which were loading with peats, were about the time above mentioned sud-

denly left dry; as suddenly, in a little afterwards, the sea returned, and it was with difficulty that the boats were preserved."

"The same phenomenon appears to have occurred all along the east coast of Shetland, from Unst to Dunrossness, and to have continued for several hours. Its occurrence on the west coast on this day, I have not ascertained. The day was remarkably fine; there was scarcely any wind; it was rather cloudy."

"The fishing-boats, on the commencement of the oscillation, fled to land, with all possible speed, fearing some unheard-of judgment to be at hand. Thomas Stone, carpenter, Lerwick, was on board a vessel, a short distance from the shore. He says that the appearance and action of the water he can scarcely describe. The feelings produced on his mind, were such as might be on the dissolution of nature."

"A similar phenomenon occurred on the west side of Shetland some years ago, attended with loud thunder."

The report from Mr LAWRENCE at Boddam, Dunrossness (Shetland), is as follows:—"On the 7th July, between 2 and 3 P.M., I was standing at the sea-side, observing a boat hauling out to sea. I was astonished to observe her suddenly get aground, and then in a few minutes float again. This occurred three times in the course of half an hour. I marked it particularly, being surprised to see the pool in which the boat was, filled and emptied three times successively, by the action and reaction of the tide."

On the 8*th July*, the oscillation was, so far as I know, observed only

At *Cullercoats*, near *Tynemouth*, on the coast of Northumberland. The sea, at 10 A.M., was in a state of perfect smoothness. There was not the slightest wind which could ruffle the surface. The tide had then flowed about half way, when suddenly the sea receded to a distance of about 12 yards. After remaining in this state for about two minutes, the tide then as suddenly flowed again to the distance of about 2 feet, beyond the previous and regular water mark. The tide thereafter flowed on, in its natural course, without farther interference. The whole disturbance occupied ten minutes.

Whilst the above are the only places where the sea was observed and watched when in a state of oscillation, or alternate flux and reflux, there are some other places where the tide registers indicate a disturbance in the ordinary flow of the tides.

At *Bristol*, where there is a very complete and accurate self-registering tide-gauge, the tidal curve, as traced by the instrument, of which Mr BUNT has kindly sent me a copy, exhibits, about 2 P.M. on the 5th July, several deviations, indicating, first a fall, then a rise, and next a fall, of the oceanic waters

continuously. Mr BUNT farther informs me, that the sea at high water, on the night of the 5th July, or rather at 1 A.M. on the 6th July, was 6 inches higher than it should have been.

The following occurrences, or some of them, are probably connected with the same phenomenon; though some uncertainty on this point prevails, as the precise date could not be ascertained.

At the *Scilly* Isles (as Mr EDMONDS of Penzance writes) the sea rose to an "unusual height" on the 5th July 1843; and a correspondent there informed him, that "the sea was much agitated, as if some violent force from beneath the surface was lifting the body of water above, while the surrounding water was perfectly calm and smooth."

At *Portlogan*, in Galloway, the tide, in the early part of July 1843, rose 3 feet higher than it was ever known to reach there, even at the springs. The weather was then moderate; but it was soon followed by heavy rain, and a gale from the SW.

At *Cockenzie* (ten miles east of Leith), in the Firth of Forth, Mr CADELL writes me, "I recollect perfectly of remarking, during a forenoon, between 10 and 11 o'clock, when on the pier, the singular rise and fall of the tide, about the period you mention, but, from not having taken any note of the phenomenon, I cannot speak positively as to the date."

Mr RITSON, the keeper of the *Little Ross* Lighthouse, wrote as follows to Mr STEVENSON:—"One day last summer, near this place, Mr Ross and his men were washing their nets, in the afternoon, at high water, when the tide receded about 3 feet. It then began to flow again, and continued to do so for about half an hour. During that time, the sea rose $1\frac{1}{2}$ feet above its previous mark. Then it began to ebb again as usual. At this time there was no storm, and the sea was perfectly smooth."

Having mentioned all the places at which the phenomenon was ascertained to have occurred, it is proper to add, that there are various places on the British coasts, where it certainly did not occur.

Liverpool is one of these places; and where, from the flatness of the shores, the multitude of shipping, and the number of tide-gauges, any flux or reflux different from the ordinary tides, would certainly have attracted general attention. Through the kindness of Dr TRAILL, I obtained reports from several intelligent observers at Liverpool, which satisfactorily establish that there was no oscillation in that quarter.

At Sheerness, where there is a most accurate self-registering tide-gauge, the invention of Mr MITCHELL, and the indications of which I have seen for the 5th, 6th, and 7th July, no oscillation appears to have taken place.

The Firth of Clyde is another part of the coast where, most probably, the phenomenon, if it had occurred, would have been noticed; but no extraordinary movement of the waters was perceived there.

Inquiries were also made by me at Carlisle, Whitehaven, Ayr, Inverary, and Belfast, at none of which places was any oscillation perceived.

It would appear, therefore, that the only parts of England where the phenomenon occurred, was on the coasts of Cornwall and Devonshire; and that, on the opposite side of the island, it was seen only along the coast of Northumberland, and the east coast of Scotland.

CAUSE OF PHENOMENON.

II. I proceed now to offer some remarks as to the probable cause of the phenomenon.

That it was produced by submarine earthquakes, I hold to be most improbable. For if such had occurred, we would have obtained from other sources some direct evidence of their occurrence; and, moreover, other effects would have been produced, which certainly did not occur.

The oscillations into which the sea was thrown by the Lisbon earthquakes, consisted of not more than two or three waves, corresponding probably with the subterranean explosions or eruptions which took place; and the flux and reflux of tide thus occasioned, did not, at any of the coasts where it occurred, continue longer than an hour.

Here, however, the oscillations lasted three or four days; so that, on the supposition of their having been caused by submarine earthquakes, it would be necessary to suppose that explosions or eruptions had been going on for several days. But it is scarcely possible to conceive that such could have occurred, without producing shocks felt on the adjoining continents, and other effects equally unequivocal. Besides, it is exceedingly improbable, that eruptions or explosions would continue so long at any one time or place. Farther, it is worthy of remark, that at the time of the Lisbon earthquakes, not only was there an oscillation of the sea, but the water in ponds and lakes was affected, by the concussion transmitted through the earth's solid framework; and some such appearances would undoubtedly have been observed in July last, had the more extensive and prolonged oscillation which then occurred been produced by a similar cause.

Before directly explaining my own views as to the true cause of the phenomenon, I shall refer to its occurrence on former occasions, as some of the accounts appear to me to throw important light on this part of the subject.

FORMER CASES.

(1.) On the 16th July 1749, the sea at Milford Haven flowed and retired seven times in a quarter of an hour.[1]

[1] Gentleman's Magazine.

(2.) On the 13th February 1756, at all the ports in the mouth of the Thames, there were for two days, great irregularities in the tides. On the 13th, the tide ebbed only 2½ feet for four hours after high water, when it flowed again for a few minutes. It then ebbed, but so little, that at low water there was 7 feet of water at Sheerness, which was 5 feet more than usual. In the morning, during the *flood*, it had blown very hard from the south of west. In the afternoon, during the *ebb*, the wind had abated, and veered to the NW. To the force and change of wind, the phenomenon was generally attributed.[1]

(3.) On the 17th or 18th July 1761, at 6 P.M., the sea at Whitby, both in the harbour and on the open sea, rose and fell four times in a quarter of an hour, to the height of nearly 2 feet each time. The sea was then calm.[2]

(4). On 28th July 1761, the sea was at various places in a state of oscillation, continuing for several hours alternately flowing and ebbing. At Falmouth, Fawey, Plymouth, and Penzance, this oscillation was observed about 10 A.M.; there the tide rose suddenly 6 feet. At Carrick, Dungarvon, and Waterford, in Ireland, it was not observed till 4 P.M., and it continued there rising and falling till 9 P.M.[3]

It is related, that the day was calm and very hot; but that, in the evening, the horizon was cloudy, with thunder and lightning. In the account given in the London Philosophical Transactions, it is stated, that near Penzance the storm of thunder and lightning came on about $7^h\ 30'$ P.M.; that it came from the NW.; and that there was the fiercest flash of lightning, and loudest clap of thunder, ever experienced. About 8 P.M. a church there was struck, and partly demolished.

On the following day, there was in Yorkshire, about 6 P.M., a terrible storm of thunder and lightning.

(5.) On the 18th September 1763, the sea at Weymouth suddenly rose 10 feet, and as suddenly went back again.[4]

(6.) On the 11th February 1764, the tide in the Severn suddenly ebbed and flowed.[5]

(7.) On the 5th September 1767, between 7 and 8 P.M., soon after high water, the water in the Liffey at Dublin suddenly sunk 2 feet, and in a moment after, rose above 4 feet, and immediately thereafter rose to its proper level. Much about the same hour, it being low water at Ostend, the tide suddenly rose and

[1] London Philosophical Transactions, vol. xlix. p. 530.

[2] DODDSLEY's Annual Register, vol. iv. p. 137; Gentleman's Magazine.

[3] DODDSLEY's Annual Register, vol. vi. p. 99.

[4] DODDSLEY's Annual Register, vol. iv. p. 142; Gentleman's Magazine; London Philosophical Transactions, vol. lii. p. 508.

[5] London Philosophical Transactions, vol. iv. p. 83; DODDSLEY's Annual Register, vol. vii. p. 50.

stirred up the mud in an extraordinary way. This continued for a quarter of an hour. The air was serene, and the wind moderate.[1]

In the Meteorological Register kept at Carlisle (the only one for that date to which I have obtained access), there is the following entry :—" 6th September 1767.—Thunder, with showers."

(8.) On the 20th February 1766, an unusual tide ebbed and flowed in the Thames; and on the same day, a most violent storm occurred in Bedfordshire.[2]

(9.) On the 9th August 1770, it is mentioned, that " during a violent thunder-storm at Brighthelmstone, the sea flowed at one motion 50 feet. The oldest man living (it is added) never remembered the like."[3]

The following is an extract from the Meteorological Register kept by Dr Borlase near Penzance :—" On 8th August 1770, close and calm in the morning. About 5 A.M., thunder, with much lightning; about 6 A.M., rained violently; at 9 A.M., cleared off, and a fine day. On 9th August, at 2 A.M., thunder and lightning, but not so violent as on preceding day; wind easterly."[4]

(10.) On the 17th July 1793, between 7 and 8 A. M., the tide flowed and ebbed in an extraordinary way at Plymouth. Three times, in less than an hour, it rose and fell about 2 feet perpendicular.[5]

(11.) On the 10th August 1802, the sea at Teignmouth rose and fell nearly 2 feet several times in ten minutes.[6]

(12.) On 31st May 1811, " an alarming and most uncommon flux and reflux of the sea took place" at Plymouth, which is described by Mr Luke Howard.[7] " It commenced about 3 A.M., and did not terminate till 10. The sea fell instantaneously about 4 feet, and immediately rose about 8 feet. Universal consternation pervaded the whole port. The vessels in Catwater were thrown about in the greatest confusion; many dragged their anchors, some drifted, and several lost their bowsprits and yards. About $6^h\ 45'$ the sea rose to the height of 11 feet, and again receded. At $9^h\ 30'$ the tide (half flood) suddenly stopped, and in a moment ebbed $6\frac{1}{2}$ inches; at 10 it ebbed again, in the same most extraordinary manner, and then flowed as usual to high water."

" Two gales from SSW. and E. preceded this astonishing phenomenon; but at the time of its occurrence, the wind was light at SSW."

On consulting Mr Howard's register of the weather for that day, at Plaistow, near London, I observe it stated, that the wind was SE., and that the barometer had sunk on that day nearly two-tenths, being 29.48, which was much lower

[1] Doddsley's Annual Register, vol. x. p. 126; Gentleman's Magazine; London Philosophical Transactions for 1768.

[2] Doddsley's Annual Register, vol. ix. p. 67.

[3] Ibid, vol. xiii. p. 99.

[4] London Philosophical Transactions for 1771.

[5] Doddsley, vol. xxxv. p. 32, and Gentleman's Magazine.

[6] Gentleman's Magazine.

[7] Climate of London, vol. i. Table 57.

than usual. Farther, from the Royal Society's Meteorological Register, kept at Somerset House, I extract the following:—

31st May 1811 at	$8^h\ 30'$ A.M.	Barom. 29.47	Wind NNE.	Rain, thunder, and lightning.	
... ...	$3^h\ 10'$ P.M.	... 29.41	... S. by E.	Rain.	
1st June ...	$8^h\ 30'$ A.M.	... 29.69	... S.	Cloudy.	

(13.) On the 8th June 1811, the following phenomenon is mentioned by Mr Luke Howard, as having occurred at Plymouth:—"About 4 o'clock the tide again flowed and ebbed several feet in as many minutes, which continued at intervals for the space of four or five hours, during which the immense swell, commonly called the *boar*, drove into the harbours of Sutton Pool and Catwater, at the rate of four knots an hour, subjecting the vessels at anchor there to great danger. The wind was variable, but mostly SW. During the operation of the *boar*, it thundered and lightened excessively."

In the Annual Register,[1] I find the same or a similar phenomenon alluded to, as having happened at Plymouth on the 4th June, with these additional particulars,—that the *boar* was accompanied by a violent gust of wind from the SW.; that the *boar* was from 9 to 11 feet high; that it occurred at dead low water; and that the quicksilver in the barometer was observed to sink and rise with a tremulous motion during the progress of the boar."

There is one circumstance mentioned in this last account, which makes it doubtful whether the phenomenon recorded in it, and by Mr Luke Howard, was one and the same occurrence. According to Mr Howard, it continued from 4 till 8 or 9 o'clock; according to the other account, the boar occurred at dead low water. Now, by the Edinburgh Almanac, it appears that, on the 8th June, it was low water at Plymouth about $12^h\ 35'$ P.M.,—not within the limits mentioned by Mr Howard. On the 4th June, it was low water at $9^h\ 35'$ A.M., and $9^h\ 57'$ P.M.; so that, if the phenomenon mentioned in the two accounts was one and the same, it is probable that it occurred on the 4th June.

From the Somerset House Meteorological Register, I extract the following:—

4th June 1811 at	9 A.M.	Bar. 29.93	Wind SW.	Cloudy.	
... ...	4 P.M.	... 29.88	... S. by W.	Cloudy.	
5th	$8^h\ 30'$ A.M.	... 29.63	... S.	Rain.	Blew hard all night.
... ...	3^h P.M.	... 29.63	... W. by N.	Rain.	
8th	$8^h\ 30'$ A.M.	... 29.86	... E.	Fair.	
... ...	$3^h\ 15'$ P.M.	... 29.75	... S.	Rain.	A thunder-storm at 6 P.M.
9th	$8^h\ 45'$ A.M.	... 29.11	... SW.	Cloudy.	

Mr Luke Howard mentions, that on the 8th June, "a severe storm of rain, hail, and lightning, took place in Birmingham and the neighbourhood. The hail, or rather pieces of ice, which fell, are described of prodigious size. At Worcester, the storm of thunder, lightning, and rain, took place about 11 A.M., and was most tremendous."

[1] Doddsley, vol. liii. p. 62.

(14.) On the 19th August 1812, at Folkestone, the sea rose and fell 3 feet perpendicular three times, in less than a quarter of an hour.[1]

(15.) At Portsmouth, on the 4th March 1818, in the evening, there were the highest spring tides (at about 10^h 40′ P.M.) ever remembered. At *Hull*, the following phenomena were observed:—At high water, about 4^h 30′ P.M., the wind then blowing from the SW., with moderate weather, the tide flowed at the dock-gates 18 feet 6 inches. After the tide had fallen from 1 to 2 inches, it began to flow again to the height of 4 or 5 inches. A tempestuous night ensued. The wind blew a heavy gale from the SW., and at high water (5 A.M.) next morning, the tide flowed only 14 feet 1 inch, although, the spring tides having put in, the water ought to have flowed to a higher level than on the preceding evening.

From the accounts given by Mr HOWARD of the weather on the day in question, it appears that there was a very severe storm in every part of England. In the Isle of Wight, it seems to have continued from 4^h 30′ P.M. to 11^h 30′ P.M. In London, it continued from 8 P.M. till past midnight. At Yarmouth, it commenced about 8 P.M.

Mr HOWARD observes, in explanation of the tidal phenomena at Portsmouth and at Hull, that, " if we suppose this gale arriving suddenly from the southward (which appears to be the fact), a *swell* produced by the compression of the water in the Channel and Straits of Dover, may have been propagated on the surface of the sea northward, with greater swiftness than the storm could make its way across the land to Hull. The arrival of this swell at the critical time of the tide's turning, accounts for the first fact. With regard to the second, it is matter of historical record, that an *off-shore* wind (as this was at Hull), if it blow long enough, and with sufficient force, may so remove the sea from the coast as to suspend a whole tide. Portsmouth and Hull were therefore placed, on this occasion, by the operation of the same cause, in opposite circumstances. The one had the highest tide ever remembered, and the other, a tide later, nearly 5 feet less water than was expected."

The only difficulty in the way of this explanation, arises from the fact, that the effects produced by this supposed *swell* manifested themselves at Hull sooner than at Portsmouth. If, however, the gale and the swell both came from the southward, they should have been first felt at Portsmouth.

(16.) In July 1832, I understand from Mr EDMONDSTONE, the sea was at the Shetland Islands in a state of oscillation similar to what occurred in July last. At this time, it appears, there was a furious gale of wind, which continued for several days, and was fatal to many fishing boats.

(17.) On the 12th September 1841, at Kilmore, on the south coast of Ireland, the inhabitants, about noon, were attracted by a number of short, loud, but rather smothered reports out at sea, which they supposed to be cannons fired by a ship

[1] Gentleman's Magazine.

bewildered in a thick fog then prevailing. The tide had flowed pretty well at the time, and the fishing boats in the harbour were all afloat, when, in the space of two or three minutes, the water receded from the pier, and some walked *dry-shod*, where, that short space before, the boats had been floating in 5 or 6 feet of water. In the course of a few minutes, the waters began to return, much in the same way as they had receded, and the tide continued to rise for the usual time. After repeated rolls of thunder, and some heavy showers, the sky cleared up. The wind had in the forenoon been from the SSW.; but immediately before the recess of the tide, the wind lulled, and the low growl of distant thunder was heard.[1]

(18.) On the 11th March 1842, it was observed by Mr CAMPBELL, the keeper of the Island Glass Lighthouse (on the Isle of Lewis), that the tide did not rise or fall more than 3 feet during the whole day; whereas, according to the state of the tides (then neap) it should have risen and fallen about 9 feet perpendicular.

He states that, on the same day, there was a very heavy gale, accompanied by thunder and rain, of which accounts were received shortly after from various parts of the neighbourhood. The thunder was not heard at the lighthouse, owing to "the violence of the storm." The wind was, at 9 A.M., about SW., and by 6 P.M. it had veered to the WNW.

This gale, so severe in the West Hebrides, was probably the same referred to in the following paragraph, extracted from the Annual Register:—"On 10th March 1842, about 10 P.M., at Brighton, the wind began to blow with great violence, accompanied by pelting showers of rain. In the course of a few hours, it increased to a hurricane, which continued the whole of the night. The gusts of wind shook the houses to their foundations."

Now, of these eighteen cases, in which the same or a very similar phenomenon occurred with that of last July, it will be observed, that fully one-half of them were accompanied by remarkable atmospheric disturbance. Gales of wind, or thunder and lightning, and a depressed barometer, in all these cases characterised the weather when they occurred. Nor would it be fair to infer that no such disturbance existed, in those cases where no proof of it is expressly given; indeed, it is probable that a person drawing up an account of any anomalous tidal phenomenon would confine his remarks to it, and take little or no account of the weather at the time. Farther, on examining the seven or eight cases of these phenomena before noticed, which are not expressly stated to have been accompanied by atmospheric disturbances, five of them occurred in July and August, the months in which, of all others, thunder-storms are the most abundant.

Looking, therefore, to the accounts given of the same or similar phenomena at former periods, I think that a strong presumption arises, that they are produced by, or are at least in some way connected with, violent atmospheric disturbances.

[1] Extracted from a Wexford paper.

Storm of 5th July 1843.

This inference is greatly strengthened by the circumstance that, during the first week of July 1843, a succession of thunder-storms passed over different parts of the British Islands; and that, on the 5th July, in particular, one occurred, which, for severity and extent, has been rarely equalled.

Independently of the bearing which it has on the oscillation of the sea at that period, it is interesting to trace the progress of this last mentioned storm, and to shew the nature of it.

Severity of Storm.

The severity of the storm may be judged of from the damage which it did at various places.

Near *Gloucester*, the lightning killed a man, and struck two other persons to the ground. The hailstones which fell, measured $3\frac{1}{2}$ inches in circumference.

At *Huddersfield*, a man was killed by the lightning.

At *Milling*, in Lancashire, a boy was killed by the lightning, and two others were for several hours rendered insensible.

At *Worcester*, it killed two horses and six sheep. It also exploded in a house, filling the room with heat and sulphureous vapour.

At *Leicester*, it set fire to a hay-rick, killed three cows, and knocked down three men and a horse.

At *Stafford*, the lightning struck two houses.

At *Cockermouth*, the lightning struck several persons, and set fire to the curtains of a bed.

At *Whitehaven*, the electric fluid was seen to issue from the earth, and to communicate with a nimbus above.

Near *Longtown*, upwards of 100 trees were torn up by the roots in less than two minutes. A man-servant was lifted from his feet, by the force of the wind.

Near *Peebles*, the lightning killed thirty-four sheep, breaking and scattering the stones of the fold in which they had taken shelter.

In *Edinburgh*, two curious circumstances were mentioned in the newspapers. One was, that whilst a servant girl was polishing a steel fork in a house in the Lothian Road, she received a severe shock of electricity, which caused her life to be despaired of. Dr Peddie, of Rutland Street, was called in, and succeeded in resuscitating the patient. At the same moment, the house immediately opposite, next to Laing's Bazaar, was damaged, the electric fluid having struck the chimneys, and partly rent the gable.

The other circumstance was, that in Great Stuart Street, immediately before the violent discharge, about $7^h\ 20'$ P.M., a maid-servant was ironing some articles of dress, when she perceived a circle of fire vibrating round the irons she held in her hand. The phenomenon was repeated three times with extraordinary rapidity, and shed such a glare of light on the article she was ironing, that she thought it was on fire. The violent claps of thunder accompanying the lightning, together with the vividness of the fire-circle streaming from the irons,

threw the girl into a swoon, in which she lay for about two minutes. When she opened her eyes, she could not see for some minutes.

At *Blairgowrie*, the lightning struck several people, some of whom had at the time metal spoons in their hands, which were driven out of them. It also demolished a byre.

At *Aberdeen*, the lightning killed a man, a girl, and a cow. Several houses were struck and damaged.

The following data shew that the progress of the storm was in a direction from S. or S. by W. to N. or N. by E. As, however, it moved northwards, it seems to have enlarged or spread over a wider space than it occupied when it first reached England. TRACK OF STORM.

Penzance.—During the occurrence of the oscillation of the sea (which, as already mentioned, continued from noon till 3 P.M.), "the sky was overcast with thunder-clouds towards the SE., and early in the morning of that day (the 5th July) a distant thunder-storm was heard in that direction. In the afternoon, before the agitation had entirely subsided, a sudden storm of wind came on from the south, and, almost simultaneously, a heavy sea, so that the large fishing boats which were out at the time had a narrow escape; and the sudden cessation of the wind and sea was as remarkable as their sudden rise."

Plymouth.—I have been favoured by Mr SNOW HARRIS with the following extract from his Meteorological Register. The force of the wind is indicated by its pressure on a square foot expressed in lbs.:—

Days of Month.	Hours.	Direction and Force of Wind.					Temp. Ext.	Pressure of Atmosphere.
		E.	SE.	S.	SW.	W.		
5th July	7 A.M.	.3	...	...	...	...	61	29.681
...	8 ...	.3	...	...	...	...	63.5	.660
...	9 ...	...	.4	...	...	...	65	.632
...	10 ...	...	.4	...	...	...	66	.624
...	11 ...	...	.3	...	...	...	66	.637
...	12 ...	...	.5	...	...	...	66	.640
...	1 P.M.	...	...	.6	...	...	63	.652
...	2 ...	...	...	.7	...	...	64	.622
...	3 ...	...	...	.9	...	...	65	.645
...	4 ...	...	...	1.0	...	...	62	.645
...	5 ...	...	...	1.5	...	...	62	.654
...	6 ...	...	...	1.5	...	...	60	.653
...	7 ...	...	...	2.0	...	...	59	.664
...	8 ...	...	...	2.0	...	...	58	.660
...	9 ...	...	...	...	1.5	...	57	.666
...	10 ...	...	...	...	1.5	...	57	.670
...	11 ...	...	...	...	1.0	...	56	.678
...	12 ...	...	...	...	.7	...	56	.682
6th July	1 A.M.	...	...	...	...	.4	55	.686
...	2 ...	...	...	...	...	.3	55	.686
...	3 ...	...	...	...	...	.3	54	.697
...	4 ...	...	...	...	...	.3	54	.714
...	5 ...	...	...	...	...	.3	54	.722

From this extract it appears that the storm was at Plymouth not very severe, as it never exceeded a pressure of 2 lbs. on the square foot. It will be remarked that the gale commenced at 8 A.M., with the wind at E., and that it ended in fifteen or sixteen hours, with the wind at W., having the same intensity at the beginning as at the end. The barometer reached its lowest point about 10 A.M.

Bristol.—Mr BUNT had the goodness to send an extract from a Meteorological Register kept there, and which contains the following information :—

Days of Month.	Barometer.				Wind for Day.	Weather.
	$9^h\ 30'$ A.M.	No	6 P.M.	$9^h\ 30'$ P.M.		
4th July	29.970	29.942	29.842	29.850	WSW.	Cloudy.
5th ...	.644	.600	.630	.640	North.	Stormy, with thunder and lightning.
6th ...	.764	.798	.840	.864	WSW.	Fine.

From this Table it appears that, just as at Plymouth, there was, on the 5th July, a fall and a rise of the barometer, corresponding with the approach and the retirement of the storm; and that the barometer reached its lowest point, probably about 2 P.M. It is important to observe, that the wind was here from the *north* on the day in question.

Greenwich.—The very accurate register, kept at the Royal Observatory (from which Mr AIREY has sent to me full extracts), supplies the following information :—

Days of Month.	Hours.	Barometer corrected.	Therm. dry.	Wind, direction.	Wind, force.	Amount of clouds.	Remarks.
5th July	8 A.M.	29.607	70.0	Calm.	...	1	A few cirri and small cumuli are scattered over the sky.
...	10 ...	.637	82.1	South.	$\frac{1}{4}$	1	A few light clouds.
...	12 P.M.	.592	84.8	SSE.	$\frac{1}{4}$	3	Do.
...	2 ...	.574	81.7	South.	$\frac{1}{4}$	8	Fleecy clouds. The air is extremely close.
...	4 ...	.507	87.3	S. by E.	$\frac{3}{4}$	3	Cirri and clouds.
...	6 ...	.475	81.7	S.	$\frac{1}{2}$	7	Cirrostratus, scud, and cirri. Sky east of meridian clear.
...	8 ...	.514	72.8	SSW.	$\frac{1}{2}$	8	Undefined clouds cover greater portion of sky.
...	10 ...	.550	64.8	SSW.	$\frac{1}{2}$	$9\frac{3}{4}$	Nearly overcast. Cirrostratus and scud.
...	12 ...	.584	61.1	SW.	$\frac{1}{2}$	$9\frac{3}{4}$	Do.
6th July	10 A.M.	.649	64.0	WSW.	$\frac{1}{2}$	6	Clear in zenith. Rest of sky nearly covered.
...	12 P.M.	.671	66.8	WSW.	$\frac{1}{2}$	10	Cirrostratus and scud. Rain falling.

From this register it appears that the storm was little felt at Greenwich, and that the barometer reached its lowest point about 6 P.M., some hours later than at Plymouth and Bristol. Farther, it is important to observe that the gale (if such it can be called) began there with the wind at SSE., and ended in about twenty-four hours after, with the wind at WSW.

Gloucester.—The storm is said to have been at its height here, between 3 and 5 P.M.

Doncaster.—Almost immediately after *noon*, the atmosphere exhibited signs of great disturbance. At 3 P.M. dark clouds, mass rolling over mass, approached from the SW., bringing with them comparative darkness. After a short pause, a sudden rush of wind indicated, that a more violent storm was at hand. It speedily approached with increasing gloom, and blew a complete hurricane, but it was of short continuance.

Derby.—The storm of thunder and lightning commenced about 4 P.M. A house was struck.

Brimington, Derbyshire (about ten or fifteen miles south of Sheffield).—The storm is described as the most terrific remembered. At 5 P.M. it thundered incessantly, and continued till 6 P.M. without ceasing for an instant. At 6 P.M. the storm came on in all its violence, accompanied by wind and hailstones.

Sheffield.—The following account is extracted, in a letter addressed to me by Mr LUCAS of The Mills, near that town :—" The storm, as you will have learnt, was most disastrous in its effects, considering that it only lasted in its utmost fury for about five minutes, in which time something approaching to L.10,000 damage was done, and the town afterwards presented the appearance of having sustained a siege, or of having been in the hands of a mob for some hours; for not only were the windows broken, but in some instances even the *frames* were partially destroyed.

" There had been evident indications of an approaching storm all the afternoon; and having occasion to pay a visit to a friend, resident across the Derbyshire moors, about twelve miles in a direction about due west of the town, I observed several heavy storms pass off on each side of me as I rode along, and fully expected to have been caught in one myself, but I luckily escaped. I also observed that there was an accumulation of nimbi in the zenith, that appeared perfectly stationary, at the same time the atmosphere was densely close and oppressive; and although we had a heavy thunder-storm where I was, at about 7 P.M., there was little or no hail, and no damage done. At the town of Sheffield, however, it came on about the same hour very suddenly, with a fall of hailstones, some of which were the size of marbles, or rather perhaps of large hazel nuts, as they were of a very irregular shape, and somewhat oblong. As far as I could learn, the storm approached from a point rather S. of W., and passed off in a direction somewhat N. of E. The fall of hailstones, however, appears to have been very capricious, and to have been confined in some instances to a limited space; for all that

portion of the town lying towards the W. suffered most severely, whilst that portion lying towards the N. and NE. suffered in a much less proportion; and at this place only, about one and a quarter miles from the centre of the town, no damage was sustained, but about one and a half miles farther N. some damage was done; and again, at seven or eight miles NE., or between Barnsley and Rotherham, there was considerable damage done; again, in a southerly direction from the town, within a mile or two, no damage of any moment was sustained; but at the villages of Norton and Dronfield, about four and six miles distant, considerable damage was done.

"To shew you, however, the capricious nature and limits of the storm, I happened, about a week after, to be walking in the neighbourhood of our Botanical Gardens with a friend, and I observed that a field of standing corn, that was within 100 yards of the Gardens, did not appear to have sustained any damage, whilst at the Gardens themselves, above 3000 squares of glass were broken; and I have heard of a similar instance of all the glass in one gentleman's vinery being demolished, whilst his adjoining neighbour's, that was within a few hundred yards, sustained no damage whatever.

"Perhaps it may not be amiss here to state, that for two evenings preceding this storm, I had observed there was something peculiar in the atmosphere, as the odour from the new made hay, as well as from many of the odoriferous plants, was particularly strong and overpowering."

Mr Jackson, surgeon in Sheffield, who read an account of the storm to the Literary and Philosophical Society of that town, has also favoured me with some information. He mentions, that "the temperature, for several days previous to the 5th, had been high, ranging as high as 85° in the shade; and about noon of that day, the atmosphere was exceedingly sultry and oppressive." "The wind, about noon, passed round from the W. by N., and at 2 P.M. stood to the E. From 4 to 5 P.M. it veered round to the S., and, as the storm approached, it moved a little to the W. of S., from which quarter the hurricane came. Its continuance was not more than half an hour, after which the wind settled in the W. The temperature was extremely high during the whole of the 5th, till about 4 P.M., when it fell till the occurrence of the storm, the variation being in some places as much as 30°; in a contiguous town, it was mentioned by a gentleman, as much as 50°. The barometer was but little disturbed during the day; one correspondent observed the barometer to fall an inch, during the passage of the storm over his house."

I wrote to Mr Jackson in regard to this last observation, and, in reply, he stated that "the individual, who furnished me with the information respecting the barometer having varied so much as an inch, is a gentleman (W. Butcher, Esq.), merchant, residing about a mile from the town. His residence is rather high, and the storm raged there with great violence. Since you wrote last, I have communicated with that gentleman, and he states that his observation of the

barometer was somewhat casual, but affirms, that there could be no doubt as to the great variation."

Dr HARWOOD of Sheffield wrote to me that the extreme severity of the storm lasted only four minutes, in which short interval a noble range of conservatories was destroyed; but a field of corn 25 or 30 yards from them was uninjured.

From the Sheffield newspapers I gleaned the following additional particulars. The barometer was sinking during all the afternoon. During the day, the wind had been generally from the NE. Immediately previous to the hurricane, there was a dead and sultry calm. From $5^h\ 30'$ P.M. to $6^h\ 30'$ P.M. the thunder continued without intermission. About 6 P.M. the sky was overcast by clouds from the SW., and the blackness continued to increase for a quarter of an hour. The storm commenced with rain, which was quickly followed by a most impetuous wind, driving before it volleys of bullets of hard clear ice—the size of small marbles. In Botanic Garden 5700 panes of glass, and in another garden 10,000 panes, were broken by hailstones.

Wentworth, West Riding of Yorkshire (eight miles north of Sheffield).—During the afternoon the day had been intensely hot, with a gentle southerly breeze. There were a few fleecy clouds floating about, and occasionally some drops of rain fell. About $4^h\ 30'$ P.M. there was distant thunder. From this time till $6^h\ 30'$ P.M. it continued to thunder, when the storm suddenly commenced, and lasted more than half an hour. The lightning was one continued flame, accompanied with gusts of wind. From 9 to 10 P.M., the lightning from N. to S., above a dense cloud on the visible horizon, was seen darting to and fro with amazing rapidity, and highly illuminating the atmosphere with every shade of orange colour.

Norton.—About 3 P.M. thunder was heard in the distance. It gradually came nearer till about 6 P.M. Dark clouds approached from the SW. Shortly after, rain and hail fell in torrents, and the wind blew violently for a few minutes. The hailstones were as large as marbles.

Birmingham.—The height of the barometer and thermometer, the force and direction of the wind, are shewn in the following Table, furnished to me by Mr OSLER. The greatest force of the wind was at $5^h\ 15'$ P.M.

Days of Month.	Hours.	Barometer.	Thermometer.	Wind.	
				Force.	Direction.
5th July	9 A.M.	29.370	62°	...	E. by S.
...	12 ...	29.30	...	1 lb.	ESE.
...	2 ...	...	78	2 ...	S. by E.
...	3 ...	29.238	76	2 ...	South.
...	4 ...	...	...	3 ...	S. by W.
...	5 ...	...	...	4 ...	SSW.
...	7 ...	...	...	1 ...	...
...	8 ...	...	...	1 ...	...
...	12 ...	29.30	52	3 ...	West.

Liverpool.—The thunder-storm occurred about 5^h 30′ P.M. It passed off in about half-an-hour, and went towards the N., which was remarked to be the ordinary direction of severe thunder-storms. During the height of the storm, the wind was very violent, stripping houses of their plaster. At Hilbre Island, hailstones fell, some of which were 4 inches in circumference.

Howden, in Yorkshire (about ten miles W. of Hull).—The storm began about 5 P.M.

York.—A gentleman travelling on the railway, writes that, "about 7 P.M., when ten miles south of York, a most unusually violent thunder-storm, with very vivid lightning, came on from the south."

Scarborough.—The storm began at 7^h 30′ P.M. The thunder and lightning continued till near midnight.

Newcastle.—The following extracts from a Meteorological Register, shew the time at which the storm arrived there, and the manner in which the wind veered :—

Days of Month.	Hours.	Barometer.	Wind.	Weather.
5th July	9 A.M.	29.696	WSW.	Dull.
...	3 P.M.	.640	SE. by S.	Fine. Thunder shower between 4 and 5 P.M.
...	9 ...	.524	S. by W.	Rain. Violent thunder-storm between 8 and 10 P.M.
6th ——	9 A.M.	.524	WSW.	Cloudy.
...	3 P.M.	.628	NW.	Cloudy.
...	9 ...	.760	NW by W.	Clear.

It is stated in the newspapers that at 8 P.M. the storm commenced, and at this time, the lightning was very vivid. It was over in an hour.

Sunderland.—It is stated that the storm came on here about 5 P.M.; but it did not reach its greatest violence till about 8 P.M. The surrounding atmosphere appeared then to be one mass of flame. At 9 P.M. it moderated, and soon after became fair.

Carlisle.—The storm began shortly after 6 P.M., and continued till near 9 P.M.

Dumfries.—The storm occurred there between 6 and 7 P.M. It was the most awful which had occurred, within the memory of the present generation. There were from twenty to twenty-five flashes in a minute.

Glasgow.—Here the thunder-storm commenced a few minutes before 7 P.M. But no thunder was heard twenty or thirty miles west of Glasgow;—and it is still more remarkable, that there was no gale of wind at Glasgow at all. The Anemometer Register at the Observatory, of which an extract was sent to me by Professor NICHOLL, shews not only that the wind during the course of the day

and night was moderate, but that it blew steadily from the east. Previously, the atmosphere and ground had been much parched by cold NE. winds.

Mackerston.—The following extracts were furnished to me by our President, Sir THOMAS BRISBANE, from the very accurate Meteorological Register, kept there by Mr BROWN, his principal assistant.

Days of Month.	Hours.	Barom.	Therm. dry.	Force of Max.	Force of Pres.	Direction of Wind.	Remarks on Weather.
5th July	$1^h\ 10'$ P.M.	29.587	66	.1	.1	NE.	Scud from SSE. Thick cirrhous haze above.
...	$3^h\ 10'$ ...	.548	62.7	.6	.5	NE. by E.	Do. Do.
...	$5^h\ 10'$ ...	.504	61.1	.9	.2	NE. by E.	Scud from S. Thick mass of clouds above. Heavy thunder-showers.
	$7^h\ 10'$ ...	.416	58.5	.7	.4	...	A good deal of thunder, with vivid flashes of lightning, within the last hour, from dark mass of scud to SW. and W. The scud was from the SSW. Cirrhous haze above, loose smoky scud *low* from ENE. At $7^h\ 13'$ P.M. a vivid flash, with a peal of thunder. At $7^h\ 15'$ a general Scotch mist with light rain; $8^h\ 20'$ frequent peals of thunder since $7^h\ 10'$, with vivid flashes. It has now passed off. There is now loose scud, acted on by various currents, moving principally from W.
	$9^h\ 10'$ ...	.430	57.	.4	0		

Here, it will be particularly observed, that the wind, previously prevailing, was from the N.E.; and, even during the passage of the storm, it appears to have retained this direction *on the earth's surface.* The upper regions of the atmosphere indicated, however, a different set of currents, which commenced with SSE., and ended with W.

It so happened, that on this day, Sir THOMAS was endeavouring to deduce the height of his observatory, above the sea at Berwick pier, by barometrical admeasurement. He had for that purpose a very correct barometer placed at Berwick pier, the indications of which were frequently observed. Sir THOMAS has handed to me a note of these, from which it appears that the greatest depression of the mercury was reached about $7^h\ 40'$ P.M.,—in which respect it agrees with the barometer at Mackerston Observatory. The following were the heights at Berwick pier, and they shew the almost constant variation of pressure taking place in the atmosphere.[1]

[1] Sir THOMAS BRISBANE informs me that his attempts on the 5th July to deduce the height of Mackerston above the sea, were completely frustrated, the three excellent barometers which he employed

Hours.	Barometer.	Ext. Temp.
$2^h\ 50'$ P.M.	29.778	58.8
$3^h\ 10'$ ...	.775	58.8
$3^h\ 45'$ ...	.768	56.6
$4^h\ 10'$ ...	.760	56.7
$4^h\ 40'$ ...	.746	55.6
$4^h\ 55'$ ...	.768	56.1
$5^h\ 10'$ ...	.765	56.6
$7^h\ 15'$ ...	.644	54.8
$7^h\ 40'$ ...	.635	55.1
$8^h\ 10'$ ...	.652	55.1

Sir THOMAS BRISBANE'S observer mentions, that at Berwick there was a very thick mist between 1 and 2 P.M., which was generally stationary, and which, when in motion, came from the eastward.

Edinburgh.—The storm commenced about 7 P.M., and, as already mentioned, was most severe about $7^h\ 20'$ P.M.

Strathearn.—The weather in this district, in the last week of June, was hard, dry, and cold. On the night of the 3d July there was a decided storm of dry wind, which continued on the 4th July. By the 5th this storm had subsided. Between 5 and 6 P.M. distant peals of thunder were heard. Towards 7 P.M. it commenced a regular thunder-storm, on a scale of great magnificence, which lasted two hours.

Arbroath.—At 7 P.M. a violent thunder-storm came on, and lasted for two hours, during which, the fall of rain amounted to $1\frac{1}{2}$ inches—as Mr BROWN informs me. The rain and hail were most severe between 7 and 8 P.M.

There had been previously a long ground swell in the sea from the NE., owing to the long prevalence of easterly winds.

Aberdeen.—The storm began at 8 P.M., and continued till 11 P.M.

Kinnaird's Head, about 70 miles north of Aberdeen.—Mr STEVENSON, the engineer of the Northern Lights, has favoured me with the following report from the lighthouse-keeper there:—" On the 5th, a severe thunder storm, which began at 9 P.M. At $9^h\ 45'$, three close flashes of lightning, followed by three loud reports of thunder, so quick that it resembled the close firing of three great guns. It filled the light-room like a blaze; after this it became more distant, but the rain fell in heavy drops till 11 P.M., and the thunder ceased shortly after."

having given very inconsistent results. He adds, " I have learnt an useful lesson—not to depend on Barometrical determinations when the atmosphere is disturbed by a thunder-storm."

Burghead, in Elginshire.—The inspecting lieutenant of the Revenue Station there reports, that on the 5th July the wind was throughout the day variable, and at 8 P.M. from the east;—that at 10 P.M. there were "heavy clouds," and afterwards "thunder, lightning, and heavy rain." Next morning, at 9 A.M., the weather was "fine,"—the wind almost calm.

Banff, in Morayshire.—The storm began at 9 P.M., and lasted three hours. The rain descended in torrents. The lightning was vivid from 9^h $30'$ till 11 P.M.

Inverness.—The entry in the Meteorological Register kept here by Mr MACKENZIE of Raining School, is as follows:—"At 10 P.M., a most vivid flash of lightning, which lasted several seconds, and followed by a rattling peal of thunder."

(1). The first inference which I draw from the foregoing details, is, that the storm in question moved over the surface of Great Britain, in a direction from S. or S. by W., to N. or N. by E., increasing in breadth as it proceeded, though diminishing in intensity.

(2.) Estimating the distance between Cornwall and Kinnaird Head at 500 miles, it would appear that this storm travelled at a rate of from 70 to 80 miles per hour.

(3.) It is most probable that it was of a whirling character; in proof of which the following facts may be referred to:—

a. "At Armley, near *Leeds*, several trees were rent to pieces by the electric fluid and accompanying *whirlwind*. One large tree was taken out of its place by the whirlwind, and was carried a considerable distance in the air. It fell on a cow, and killed her. Another large tree was *twisted* in two in the centre of the trunk, and the fibres of the wood, at the point of separation, appeared *twisted* together like the strands of a rope."[1]

b. The manner in which the wind blew and veered at different places, cannot otherwise be accounted for.

At Plymouth, the storm began with the wind at east, and in about twenty hours afterwards, it ended with the wind at west, having veered round by the south. This can only be explained on the principle that currents of air were whirling round a centre, in a direction contrary to the hands of a watch, as explained by Mr REDFIELD. The current which blew most strongly was from the S. and SW.; because the current in that direction, in addition to its own motion, was aided by the general progressive movement of the storm in the same direction.

At Greenwich, which was evidently farther from the centre of the storm, and to the eastward of it, the gale, as already stated, began with the wind at SSE., and in about twelve or fourteen hours afterwards, ended with the wind at WSW. This place was, therefore, in the east segment or limb of the stormy circle.

At Sheffield, which was evidently in, or very near, the centre of the storm,

[1] Bristol Mirror Newspaper, of 15th July 1843.

it commenced about 2 P.M., with the wind at east, and in about six hours ended with the wind at west.

At Newcastle, which was to the eastward of the centre, though not so much as Greenwich, the storm began with the wind at SE. by S., and ended with the wind at NW. by W., in about eighteen hours.

So, also, at Mackerston, the scud aloft was seen successively to be moving from the SSE., S., SSW., SW., and W.

These indications are all reconcileable, on the supposition that the gale in question consisted of currents revolving from right to left.

This inference is confirmed by the direction of the wind at Bristol, which, on the 5th July, is stated to have been from the north. This is easily explained, on the supposition that the centre of the storm pursued a track to the east of Bristol, which, from other circumstances, it is probable was the case.

c. There are facts of a different description, which lead to the same conclusion.

The sudden and prodigious reduction of temperature in those parts traversed by the centre of the storm, shews that, by some means, cold air was drawn down to the surface of the earth; and in no way would this be so naturally effected as by a whirling storm, producing a rarefaction of the air in its central parts. Hence, in those parts, the quantity and size of the hailstones which fell, and the sudden depression of the barometer (in one place to the extent of a whole inch), during the passage of the storm.

The condition of the atmosphere in different places immediately before the storm, suggests some interesting considerations, as to the mode in which the electrical equilibrium was so violently disturbed. But it would be irrelevant, in this paper, to enter on that path. It may be simply remarked, that in Scotland, for some days before, there had prevailed a cold and parching wind from the NE., whilst, in the south[1] of England, the temperature ranged about 80°. When these two aërial currents, so different in all respects, came into contact, a violent disturbance was the necessary consequence. In the first place, an eddy or vortex would be formed, which would move to the northward, by the greater force of the SW. wind; and if this current, as is probable, flowed on the right hand of the other (looking to the Pole), the whirl would be from right to left. Then, the sudden condensation of the warm vapour into rain and hail, would sufficiently explain the various electrical phenomena which accompanied the storm in its progress northward.

Without dwelling on these topics, I now resume the primary subject of inquiry, viz., the cause of the oscillation of the sea on the 5th July. This phe-

[1] At Brighton, the 5th July was the warmest day of the summer, the thermometer having stood at 78° in the shade. But at night it sunk down to 55½°.

nomenon, I consider, was connected with the storm of which I have just traced the extent and progress; and to this conclusion I am led, by the following considerations:—

(1.) In the first place, the phenomenon has, as already shewn, been generally accompanied by violent storms, or other proofs of atmospheric disturbance.

(2.) The oscillation on the 5th July began in the English Channel, and did not, till some hours after, manifest itself on the east coast of Scotland. So, also, the storm began in the south, and moved northward, the parts both of the English and the Scotch coasts most affected by it, being those where the oscillation was observed.

(3.) In so far as it is possible to judge of the part of the ocean in which the oscillation was generated, that part coincides with the direction from which the storm came.

It has been seen, that the oscillation began at Plymouth about 11 A.M., and at Penzance about half an hour later.[1] It probably originated, therefore, in the English Channel, considerably to the SSE. of Mountsbay.

Now, it is stated that, early in the morning, at Mountsbay, there was heard a distant thunder-storm in that direction, and that, before 3 P.M., a sudden storm of wind came on from the south, and, almost simultaneously, a heavy sea, so as to endanger even large fishing-boats, and that the sudden cessation of the wind and sea was as remarkable as their sudden rise.

It is very manifest, that this was the same storm of which the progress has above been traced; and it is probable, therefore, that they both came from the same part of the ocean.

(4.) The circumstance that the oscillation of the sea on the Cornish and Devonshire coasts preceded the arrival of the storm by some hours, so far from being an objection to the view above suggested, is rather a confirmation of it; as it is well known, from the researches of Mr Scott Russell, that a wave, when generated by a moving force, will acquire a velocity greater than that of the force producing it, if the depth of water be sufficient. I have elsewhere shewn, that the waves produced by the Lisbon earthquakes came to the English and Irish coasts, with a velocity of from 120 to 160 miles an hour. It is therefore probable, that if a wave were generated by the storm in question, it would move forward with about double the rapidity of the storm itself, which, I have shewn, travelled at a rate of only 70 or 80 miles an hour.

Whilst the considerations now submitted appear to me sufficient to connect together the storm and the oscillation, I am aware that something else is awanting to prove the relationship of cause and effect. In what way, then, is it pos-

[1] There is some doubt as to this. In the Athenæum of 1843, p. 849, it is stated that the tide had ebbed about half an hour when the oscillation occurred. Now, it was high water in Mountsbay at $9^h\ 54'$ A.M., and, according to this account, the oscillation occurred about $10^h\ 24'$.

sible that such a disturbance in the atmosphere could produce such an effect on the ocean?

In answer to this question, I would submit the two following considerations, either of which would be sufficient to accomplish the effect observed.

(1.) That wind is capable, by its mere mechanical pressure, to alter the level of the ocean is well known. Mr LUKE HOWARD alludes to this, in his explanation, above quoted, of what occurred at Portsmouth and Hull on the 4th March 1818. This effect is greatly increased when the wind, after blowing in one direction, suddenly shifts, and blows in an opposite direction; an example of which has already been given, under date 13th February 1756.

Now, the storm of 5th July was of this character, and must have acted on the surface of the ocean, so as to impel its waters first in one direction, and soon after in an opposite direction, thereby causing an alternate flux and reflux on the adjoining continents.

It is true that the gale at Plymouth does not appear to have been so severe as at Mountsbay, and especially in the midland counties. This may be explained by supposing that the storm, whilst moving through the atmosphere, descended, in some parts of its track, close to the earth's surface, and in other parts affected only the upper regions. This seems to have been the case at Mackerston, where a low scud from the NE. was generally prevailing, as the whirling currents passed along in the upper regions. Before reaching Plymouth, the storm may have been acting on the surface of the ocean with the same fury which it displayed at Mountsbay, Sheffield, Liverpool, and Longtown, and thereafter risen, by rebounding, as it were, into the higher regions.

By such gusts, then, it is quite possible to conceive how the sea, which was affected by them, may have been thrown into a state of oscillation. That is to say, the superficial waters may have been pushed forward by pressure, and which, on reaching the shore and falling back from it, would produce an alternate flux and reflux.

(2.) The second consideration which I wish to submit, is founded on the well-known fact, that the level of the ocean rises with the fall of the barometer.

Mr WALKER, the Queen's Harbour-Master at Plymouth, to whom I am indebted for an account of the oscillation observed there on the 5th July last, was, I believe, the first person to ascertain, by a long series of observations, that depressions of the barometer are generally accompanied by elevations of the ocean's surface; and he has been able even to calculate the amount of such elevation, with reference to the amount of barometrical depression. These observations of Mr WALKER, are thus referred to and explained by Sir HENRY DE LA BECHE, in his Report on the Geology of Cornwall.[1]

[1] Report on the Geology of Cornwall and Devon, p. 11.

"Mr WALKER has observed, with respect to the influence of the pressure of the atmosphere upon the tidal waters on the shores of Cornwall and Devon, that a fall of 1 inch of the mercury in the barometer, corresponds with a rise of 16 inches in the level of the sea, more than would otherwise happen at the same time, under the other general conditions; a rise in the barometer of 1 inch marking a corresponding fall in the sea-level of 16 inches. This he found to be the usual rate of such alterations in level; but *very sudden* changes in the pressure of the atmosphere are accompanied by elevations and depressions equal to 20 inches of sea-water for 1 inch of mercury in the barometer. Regarding the whole pressure of the atmosphere over the globe as a constant quantity, all local changes in its weight, merely transfer a part of the whole pressure from one place to another; and hence he concludes, that the subjacent water only flows into, or is displaced from, those areas, where, for the time, the atmospheric pressure is either less or greater than its mean state, in accordance with the laws which would govern the condition of two fluids situated in the manner of the atmosphere and sea. We might account for the difference observed by Mr WALKER, in the amount of depression or elevation of sea-level produced by sudden changes in atmospheric pressure, by considering that a sudden impulse given to the particles of water, either by suddenly increased or diminished weight in the atmosphere, would cause a perpendicular rise or fall in the manner of a wave, beyond the height or depth strictly due to the mere change of weight itself."[1]

These inferences of Mr WALKER's have been confirmed by observations made by Mr LUBBOCK at Liverpool, and by Mr BUNT at Bristol. Mr LUBBOCK calculated the amount of rise in the sea level, for a depression of one inch of the barometer, to be 11.2 inches;[2] Mr BUNT calculated it to be 15 inches.[3] This last is pro-

[1] "A circumstance connected with this subject, of considerable practical value, has been noticed by Mr WALKER during his long-continued observations. He has found that changes in the height of the water's surface, resulting from changes in the pressure of the atmosphere, are often noticed on a good tide-gauge *before* the barometer gives notice of any change. Perhaps something may be due in these cases observed by Mr WALKER to the friction of the mercury in the barometer-tube, as it is well known that in taking careful barometrical observations it is necessary to tap the instrument frequently and carefully, to obtain the measure of the true weight of the atmosphere at a given time and place. The practical value of the observation is, however, not the less, be the cause of the phenomenon what it may; for if tide-gauges at important dock-yards shew that a sudden change of sea-level has taken place, indicative of suddenly-decreased atmospheric weight, before the barometer has given notice of the same change, all that time which elapses between the notices given by the tide-gauge and barometer is so much gained; and those engaged with shipping know the value of even a few minutes before the burst of an approaching hurricane." (*Note* by Sir HENRY DE LA BECHE on the page above referred to.) This passage affords a confirmation of the view above suggested, that a diminution of atmospheric weight will, in certain cases, be indicated by an oscillation of the sea before it is indicated by a fall of the barometer, or a change in the state of the weather.

[2] Transactions of Royal Society of London for 1837, p. 103.

[3] British Association Reports for 1840, p. 30.

bably the most correct, seeing that it agrees best with the Plymouth observations, and that it corresponds nearly with the ratio of the relative densities of mercury and sea water.

If, therefore, the storm, as it approached Great Britain, was accompanied with a diminution of atmospheric weight, to the extent of an inch and a half, it would have the effect of raising the sea comprehended within its limits *at least* 2 feet. It might be more; for, as Sir Henry De La Beche remarks, a sudden impulse given to the particles of water, would cause them to rise beyond the height strictly due to the amount of the atmospheric change.

In order to prove that the view now suggested was really applicable to the circumstances of the case, it would have been desirable to have shewn, that the storm in question *was* accompanied by a considerable barometrical depression. If, as appears probable, the storm was one of a whirling character, it may almost be assumed, that, in its central parts, there must have been a considerable diminution of atmospheric pressure; and, acccordingly, it appears that at Sheffield, over or near which the centre of the storm passed, the barometer was seen to sink a whole inch. Such would be the effect produced by the mere whirling of the storm. But the centrifugal action which produced a depression of the barometer at and near the central parts of the storm, would probably produce an opposite effect near its limits. There, a condensation of the air would to some extent take place. It may be reasonably expected, therefore, that, except at places over or near which the very centre of the storm passed, the barometrical returns would shew no great depression.

From the circumstance here alluded to, it may be inferred, that the rise of the sea under the central parts of a whirling storm, would be greatly aided by the depression of the sea towards the limits of the storm, caused by the atmosphere being there in a condensed state. But, independently of these effects, due to the whirling character of the storm, it should be remembered that, during sudden changes in the electrical state of the atmosphere, great changes take place in its density. Beccaria mentions, that he once saw the mercurial column descend during a flash of lightning.[1]

Now, considering the extensive and extraordinary exhibition of electrical phenomena which accompanied the storm in question, it is not unreasonable to suppose, that, from this separate cause also, a sudden and considerable diminution of atmospheric pressure accompanied the storm as it moved over the waters, thus heightening the effects produced by the whirling character of the storm. This separate cause is the more deserving of attention, seeing that, in so many of the instances recorded of similar oceanic oscillations, thunder and lightning occurred abundantly about the same time.

By these combined causes, the pressure of the atmosphere on those parts of

[1] Robertson on the Atmosphere, vol. i. p. 105.

the sea traversed by the storm, must have been greatly and suddenly diminished; and thus, beneath the whirling aërial current, the waters of the ocean would be heaped up, and form a sort of wave, which would advance in the direction of the storm. But, for the reasons already given, it would move forward more rapidly than the storm itself, and make way for the formation of another and another wave, which would continue to advance towards the land, until the storm itself arrived there. Accordingly, it is mentioned in the Penzance account, that the storm reached the Cornish coast, shortly before the oscillation of the sea subsided.

Thus, I conceive it possible at least to explain how, partly by the mechanical pressure of the wind in the storm,—blowing first in one direction, and thereafter in an opposite direction,—and partly by the sudden diminution of atmospheric pressure accompanying its progress, the sea on the SW. coasts of England was thrown into a state of oscillation.

I confess, however, that it is somewhat more difficult to explain the oscillation on the Scotch coast on the afternoon of the same day.

There are only two ways in which it seems possible to account for it. The first is, by supposing that the agitation on the Scotch coasts was propagated like the tidal wave, from those parts of the ocean where the English oscillation originated. The other is, by supposing that the storm itself, on reaching the Scottish seas, produced the same effects there as elsewhere.

There is some difficulty, however, in adopting either explanation.

In regard to the last, it will be observed that, whilst the oscillation commenced in the Firth of Forth between 1 and 2 P.M., the thunder-storm did not, in its course northward, reach Scotland till some hours after; and therefore it could not have affected the German Ocean early enough in the day, to have produced an oscillation.

In regard to the other explanation, the admissibility of it depends on the place where the waves producing the flux and reflux were generated. Two places may be found, either of which, as the supposed birth-place of these waves, would suit the time of their arrival in England and in Scotland. The one is in the Atlantic Ocean, to the W. or NW. of Ireland; for a tidal wave produced at this place would reach the Cornish coasts and the Firth of Forth much about the same time. The other place is about the Straits of Dover. But neither of these points suit well the other conditions of the question,—for this reason among others, that the storm did not come from either the NW. or the NE. to Plymouth and Penzance, but is distinctly proved, both by the observations there and by the general progress of the storm, to have come from the southward.

It is very probable, that the oscillation of the sea on the east of Scotland was produced by another storm altogether, which raged over the German Ocean. It appears that the whole atmosphere in this part of the globe, was about this time violently disturbed. There is good evidence of there having been several very

severe storms on the 6th, 7th, and 8th July,—not only in Great Britain, but also on the Continent. On the 6th July, there was early in the morning a severe thunder-storm at Brussels, which extended over a considerable part of Belgium. On the 7th July, another thunder-storm passed over part of England, which, near Flint, in Wales, killed a number of cattle, and is described as the heaviest which had ever been experienced. It passed over Manchester; and discharged torrents of rain. At Birmingham, as appears from Mr OSLER's Register, it was fair and sunshine in the forenoon, whilst, in the afternoon, there were thunder-storms. The pressure of the wind, at 5^h 30′ P.M., was 4 lb. on the square foot. On the 8th July, another thunder-storm, equally severe and more extensive, passed over the NW. parts of England, and the SW. parts of Scotland. In Lancashire, the lightning struck and injured a Methodist chapel near Rochdale, and a house at Stalybridge. In and near Glasgow, it killed two men, and set fire to a cotton-mill, and it is generally described as having been much more severe than the storm felt there on the 5th. On the 9th July, a highly electric state of the atmosphere was again manifested, particularly in Lancashire. On the 10th July, there was in the West Highlands a hurricane, accompanied by much thunder and lightning, which, though it pursued a narrow track, blew down immense numbers of trees, and did other serious damage.

Thus, for six successive days, the atmosphere over and around Great Britain was in a state of extreme disturbance,—and of which, probably, all the proofs have by no means been collected by me. Enough, however, have been obtained to create a strong presumption that, early on the 5th July, there was, besides the thunder-storm, the progress of which over England has been traced in this paper another between England and the Continent, which affected the sea of the German Ocean in the same way that the sea of the English Channel was affected by the former. At all events—the oscillations of the sea on the 6th, 7th, and 8th July, which were observed along the east coast of Scotland, may fairly enough be ascribed to the disturbed state of the atmosphere on these days,—if I have succeeded in connecting the oscillation on the Cornish and Devonshire coasts with the storm which on the same day passed over these counties in its progress northward, or in shewing how, by such a cause, these oscillations may be produced. The German Ocean being, in one or more places, subjected, during each of these days, to the influence of the several storms above referred to, and probably of others also, which are unknown to us, must have been thrown into a state of oscillation, sufficient to have produced the effects described.

XL.—*Notice concerning the Indian-Grass Oil, or Oil of Andropogon Calamus-aromaticus.* *By* Thomas George Tilley, *Esq., Phil. D. Communicated by* Dr Christison.

(Read 6th May 1844.)

The volatile oil commonly known as Indian-Grass Oil, is supposed by Professor Royle to be produced from a peculiar species of *Andropogon*. This species he considers identical with the Καλαμος αρωματικος of the ancient Greeks, and he has therefore called it *A. calamus aromaticus*.

For the specimen of the oil which I have examined I am indebted to Dr Christison, by whom it was received from Mr Key, of the Madras Medical Service. This gentleman obtained it while resident in the Nizam's territories, not far from Ellichpoor, where the plant grows freely on hilly ground. The oil is there called Roossah-oil.

Dr Christison informs me, that he has frequently received from his East Indian friends, under the name of Grass Oil, specimens of a volatile oil, which, if not the same, bears the greatest resemblance to the one which I have examined, apparently differing to a slight degree in odour alone.

I extract from Dr Christison's letter the account of the medical virtues of this volatile oil. "The oil has been repeatedly sent to me," writes Dr Christison, "as a remedy highly esteemed in India, when used in the way of friction, for rheumatism. I have tried it occasionally with decided advantage. A gentleman, a patient of mine, who was long harassed by severe pains along the course of the nerves of the neck and back, in connection with chronic organic disease of the spinal cord, told me he experienced more relief from friction with this oil than from any other external application he had tried; and a medical friend, much subject to lumbago, assured me that no other substance he had made use of, produced so penetrating a glow of heat. On the whole, the Grass Oil seems to be an article well deserving investigation in many respects."

The Oil of Andropogon has an agreeable smell, which is not, however, the same in all specimens. It has sometimes a greenish, more frequently a yellowish colour. The specimen whose analysis will be given immediately, had a green colour, resembling that of Cajeput. It had a smell similar to that of malt,* and

* The finest qualities of it, including that examined by the Author, have a grateful, penetrating, diffusive odour, more or less approaching the fragrance of oil of roses; so that I suspect this to be one of the oils used for adulterating that costly essence. *Note by* Dr Christison.

suggesting the idea of great volatility. When heated it becomes yellow, and deposits brown flakes; it begins to boil at 255° Fah.; the temperature quickly rises to 400° and, the fluid begins to boil very rapidly at about 430°. At 440° a pure colourless oil distils over, and the boiling fluid does not gain any heat for some time. At this period the receiver was changed, and the oil coming over between 440° and 442° obtained.

To purify this portion of the oil, it was distilled again; and, after having stood some time in contact with melted chloride of calcium, it was once more rectified.

To ascertain the composition of this oil, it was burned with peroxide of copper, when

.2960 of substance gave .3082 water,
= .0334 Hydrogen, and 1.228 Carbonic Acid,
= .28281 Carbon. Carbon being 76.437 a. w.

which numbers give per cent.,

Carbon,	88.10
Hydrogen,	11.29

and these per-centage numbers lead to the formula $C^{10} H^{16}$, the numbers calculated from this formula being, per cent.,

Carbon,	88.46
Hydrogen,	11.54

The numbers found agree so nearly with those deduced by calculation from the formula given, that there can be no doubt but that the Oil of Grass contains, and chiefly consists of, a carbo-hydrogen, in which the proportion of the carbon is to that of the hydrogen as 10 to 16, as in the case of oil of turpentine, and other volatile oils of the same class.

The oil appears to be rendered impure by a certain quantity of a resin, probably formed by the absorption of atmospheric oxygen.

Subjected to the action of powerful oxidizing agents, it is converted into resinous, adhesive bodies, having acid properties, and dissolving in alkalies, giving red solutions. Perzoz' oxidizing fluid, formed by dissolving bichromate of potash in dilute sulphuric acid in equivalent proportions, converts the oil into a glutinous dirty resin. Nitric acid acts with great violence on the oil, and produces, even after the continued application for several days of the strongest acid, only a yellow resin.

The Oil of Grass combines with hydro-chloric acid, forming a yellow oil, heavier than water, and not yielding crystals by the application of intense cold,

produced by snow and salt. Much heat is evolved, and the oil is turned pink and brown in colour, if the hydro-chloric acid is transmitted rapidly.

The binoxide of nitrogen also combines with this oil, giving a resinous mass, but no crystalline body.

Iodine exhibits a violent action on coming into contact with the oil. Sulphuric acid turns it brown.

A portion of the oil contains oxygen; indeed, it is not improbable that it is a mixture of several oils. .3540 of another specimen obtained from Dr CHRISTISON, when purified by distillation and dried, gave 1.116 carbonic acid, and .3655 water, which, when brought to hundred parts, is

Carbon,	87.19
Hydrogen,	11.43

The Oil of Andropogon may be considered to belong to the numerous family of carbo-hydrogens; beyond this it possesses no interest in a chemical point of view. It is, doubtless, a mixture of several of these troublesome bodies, and contains, besides, an oxygenated oil.

XLI.—*On the Existence of an Osseous Structure in the Vertebral Column of Cartilaginous Fishes.* *By* James Stark, M.D., F.R.S.E.

(Read 18th March 1844.)

In the course of an investigation into the structure and composition of the solid frame-work of vertebrated animals, I remarked, in the vertebral column of cartilaginous fishes, an important peculiarity of structure, which seems to have almost entirely escaped the notice of writers on ichthyology and on comparative anatomy.

Fishes have been divided into two great sub-classes, according as their skeleton consisted of bone or of cartilage;—the first sub-class being denominated Osseous, the latter Cartilaginous or Chondropterygious fishes. The writers on ichthyology and on comparative anatomy, up to the year 1817, appear to have remarked nothing more regarding the skeletons of the chondropterygious fishes, than that their hard parts were composed of cartilage. But in the year 1817, the "Regne Animal" of the celebrated Baron Cuvier was published, and in it occurred the following particulars, relative to the cartilaginous skeleton of those animals—particulars not noticed in the first edition of the "Leçons d'Anatomie Comparée." "Their skeletons," says Cuvier, "remain essentially cartilaginous; and, in general, no osseous fibres are formed in them. The calcareous matter is deposited in little grains, and not in threads or filaments.* . . . It even happens that articulations which are moveable in other fishes are not at all present in them. For instance, a part of the vertebræ of certain Rays, and all the vertebral column of the Lampreys, are united into a single mass, and are only distinguished by annular markings." In speaking of the family of the Cyclostomes, he adds: "The bodies of the vertebræ are united into a single tendinous cord, filled internally with a mucilaginous substance, and clothed exteriorly with cartilaginous rings, scarcely distinguishable from one another. The annular part, which is a little more solid than the rest, is not, however, cartilaginous in all its thickness."

Blumenbach, whose work on comparative anatomy was translated into the English language, the first edition by Lawrence, and the second by Coulson in 1827, and Carus, in his Introduction to the Comparative Anatomy of Animals, translated into English, and published in 1827, take no notice of these peculiari-

* Cuvier, Regne Animal, 1st Edition. Paris, 1817. vol. ii., p. 114.

ties of the cartilaginous skeletons of chondropterygious fishes—simply styling it cartilage. Nor is any farther information given by MECKEL. He, however, notices the consolidation of the cartilaginous vertebræ of the Rays, at that portion corresponding to the cervical vertebræ.

In 1828, Baron CUVIER commenced the publication of his great work on Fishes. The first volume contains the general remarks on the structure of these animals; and the peculiarities of the skeleton of cartilaginous fishes is summed up in these words: "Their hard parts consist internally of a homogeneous and semitransparent cartilage, which is only covered over at its surface in the Ray and Shark tribes, with a layer of small opake calcareous grains, closely applied to each other. But in the Lampreys, even this envelope is wanting; and, finally, in the Ammocetes this part remains absolutely membranous."* Baron CUVIER, however, shewed that there occur a few exceptions to this general rule; as, for instance, in the Sturgeon, about whose head and shoulder true bones occur.

In 1834, MÜLLER,† in a paper on the Comparative Anatomy of the Myoxinoideæ—a family of chondropterygious fishes, endeavoured to shew that four kinds of cartilage occur in cartilaginous fishes. The *first*, or hyaline cartilage, is nearly transparent, and abounds in the Sturgeon and Chimeræ. The *second*, or tubercular cartilage, occurs in the Ray and Shark tribes. The *third*, or cellular cartilage, is met with in the Bdellostoma. And the *fourth*, or ossified cartilage, forms the harder parts of the Sharks and Rays. Subsequent writers do not seem to have been aware of MÜLLER's researches, as I have not seen them alluded to by any one.

In the first volume of the second edition of CUVIER's "Leçons d'Anatomie Comparée," published by Dumeril in 1835, the following interesting remarks are made relative to the skeletons of chondropterygious fishes:

"The earthy molecules of these (cartilaginous) fishes are deposited in many different ways, but never form threads, nor take the stony hardness of some of the bones of the mammalia. In most of the bones of the Rays and Sharks there is found at the surface a layer of closely applied grains, while the centre remains pure cartilage. These grains are seen uniformly everywhere. There are no radiations nor centres of ossification, consequently, no sutures on the cranium or jaws. In the thick bones, as the bodies of the vertebræ and certain jaws, there is also a granular layer on the surface; but the interior of the cartilage is often penetrated by phosphates, either in lamellæ forming cellulosities, or in plates differently but regularly disposed. In certain larger Squali (the maximus, for example), there are cylindrical plates, quite concentric, all separated by layers of a tender carti-

* CUVIER, Hist. Nat. des Poissons. Paris, 1828, vol. i., p. 294.

† Abhandlungen der Königlichen Academie der Wissenschaften zu Berlin. Aus dem Jahr, 1834. Berlin, 1836.

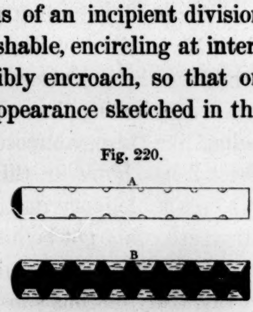
lage, all pierced by holes, as if riddled; in a word, with admirable regularity. Sometimes, the total appearance seems homogeneous, though less hard in the interior than at the surface."*

Dr Grant, in his "Outlines of Comparative Anatomy," published first in the Lancet in 1833 and 1834, and afterwards as a separate treatise, adds but little to Cuvier's above description. In pointing out the progress of ossification, as we advance higher in the scale of animated beings, he remarks, "you can easily perceive this consolidation of the laminæ in this Shark, and in several other specimens of this animal that are before you, and still better in the Sturgeon; indeed, in the Sturgeon you have a beautiful illustration of this fact, that while the bodies of the vertebræ remain perfectly united, transparent, soft, and cartilaginous, the laminæ and spinous processes, and the transverse processes themselves, have become white, opaque, and ossified, and have received a larger quantity of the earthy matters we have enumerated, than the bodies themselves."†

The only other work on comparative anatomy, which has of late years appeared in this country, is the very excellent one of Jones, entitled, "A General Outline of the Animal Kingdom," and published in 1841. Relative to the vertebræ of fishes he has the following remarks:—

"Even in tracing the modifications observable in the construction of the vertebral column, we have a beautiful illustration of the progressive advances of ossification, in this central portion of the osseous system. The spine of the Lamprey, although at first sight apparently entirely soft and cartilaginous, presents already, in the arches which compose the spinal canal, and in the soft cord that represents the bodies of the vertebræ, slight indications of an incipient division into distinct pieces; rings of ossific matter are distinguishable, encircling at intervals the soft spinal cartilage upon which they perceptibly encroach, so that on making a longitudinal section of the cord, it offers an appearance sketched in the adjoining figure (Fig. 220, A). In a more advanced form of a fish's skeleton, as, for example, in the Sturgeon, these ossified rings are found to have enlarged considerably, and penetrate still more deeply into the cartilaginous mass (Fig. 220, B). As the bony rings thus developed approximate the centre, it becomes more and more evident that they represent the bodies of so many vertebræ."‡

Fig. 220.



Further on he says: "The skeletons of the cartilaginous fishes (*Chondropterygii*) will require a distinct notice, inasmuch as they present very remarkable

* Cuvier, Leçons d'Anatomie Comparée, by Dumeril. Paris, 1835, vol. i., pp. 127-28.

† Grant, in Lancet for December 1833, p. 540.

‡ Jones, General Outline of the Animal Kingdom. London, 1841, p. 490.

peculiarities of no inconsiderable interest. In the Sharks, Skates, and other genera belonging to this important division of the great class we are now considering, the interior of the bones remains permanently cartilaginous; but the skeleton is in some regions encrusted, as it were, with osseous granules. No centres of ossification, from which radiating fibres of bony matter progressively extend themselves, as is the case in the osseous fishes, are ever developed."*

Such, then, is the amount of information relative to the vertebral column of the cartilaginous fishes, contained in works known to me. The beautiful descriptions of Sir EVERARD HOME, of the intervertebral apparatus of the Sharks and rays, have been omitted to be noticed, as it is of the bones alone I purpose to make any mention in this communication.

In the course of investigations into the structure of the skeletons of the vertebrated animals, I procured the vertebral column, and some of the other solid parts of the Common Skate (*Raia batis*), Thornback Ray (*R. clavata*), Starry Ray (*R. radiata*), and Sharp-nosed Ray (*R. oxyrhynchus*), for the purpose of submitting them to a chemical examination. The bones or cartilages were carefully cleaned of all fleshy and other matters, dried till they ceased to lose weight, and were then burned. As I had been prepared to expect, the calcareous granular particles which were seen very distinctly on the surface of the cartilages, were procured in the state of a loose granular powder; but I was not a little surprised to find that each vertebra yielded, at the same time, a solid osseous nucleus, very analogous in form, and somewhat resembling in structure, the bony vertebræ of the osseous fishes.

A careful examination of the vertebral column of these animals satisfied me, that this internal nucleus consisted not of agglutinated calcareous particles, resembling those scattered over the surface of the cartilaginous skeleton, but of true bone, having the same general structure as the vertebræ of osseous fishes, being, like them, composed of concentric rings, or laminæ of osseous matter. In these fishes, however, this osseous nucleus was highly condensed, so as to cause it to resemble ivory itself; and in consequence of this condensation, the fibrous structure could not be distinctly seen in it.

It further appeared, that the essential part of each vertebra was composed of this truly osseous substance, and that the whole of the external cartilaginous covering, with its interspersed calcareous grains, could be removed without interfering with the integrity of the vertebral column, in so far as the union of the one vertebra to the other was concerned. In fact, it was by means of this osseous portion, that solidity and strength were given to the whole vertebral column; and it was to this osseous portion, and to it alone, that the intervertebral ligamentary apparatus, so well described by Sir EVERARD HOME, was attached.

* JONES, p. 505.

In the Rays, then, the vertebral column, far from being solely formed of cartilage, consists essentially of true bone, surrounded, it is true, by cartilaginous matter, and having all its processes formed of cartilage, with its attendant covering of calcareous granules, but still consisting essentially of the very same structure as the vertebræ of osseous fishes.

Each vertebra consists internally of two cup-shaped osseous pieces united by their narrow bases, or of two truncated cones united by their truncated apices, so that each vertebral bone resembles a wooden egg-cup, or an hour-glass. Towards the extremity of the tail of the Rays, these hour-glass-shaped vertebræ are nearly simple; or the point of union between the two cups—that part which forms the body of the vertebra—is swollen out into a bulging ring, as at fig. 1, *a*. In some Rays this form of vertebra is often alternated with the simple double cup-shaped one. The bodies of all these vertebræ are perforated by an aperture so minute as scarcely to admit the point of the finest needle. (Fig. 1.)

Fig. 1.

Fig. 2.

Fig. 3.

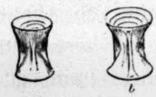


Fig. 4.

Fig. 5.

As the body of the animal is approached, these double cups receive strengthening pillars or plates of osseous matter, first on two opposite sides (Fig. 2), and soon on four sides (Fig. 3), so as perfectly to support the enlarged size of the vertebræ in all violent motions of the animal. These solid strengthening pillars or plates are placed perpendicularly to the outer surface of the vertebræ, and are always strongest and broadest on the upper or dorsal surface, and on the lower or abdominal surface, and fill up more or less of the angular space between the two cups of each vertebra (Fig. 3). The upper or dorsal plate is generally single, and, in all the larger vertebræ, extends forward to the edge of the cup of the articular surface of the vertebra (Fig. 3, *b*). The plate on the anterior or abdominal surface of the vertebra is either double in the larger bones (Fig. 4, *a*), or consists of a broad, somewhat grooved, plate in the centre, opening out into two plates or supporting bases, as it reaches the edge of the vertebral cup (Fig. 5, *a*). In all the larger vertebræ, the anterior plate or plates extend forwards to the outer margin of the cup-shaped articular surfaces of the vertebræ, and thus fill up the whole angular space. It is otherwise, however, with the lateral columns. They rarely fill up the whole angular space left by the union of the articular cups of each vertebra, but only about a third or half of that space (Figs. 4 and 5, *b b b*). In the upper dorsal vertebræ, however, these lateral pillars are often double, and advance forwards to the edge of the articulating cups. The central aperture which pierces the body of the largest vertebra, is not larger than that of the smallest vertebra—scarcely admitting the point of the finest needle.

Now, it is worthy of remark that these strengthening pillars represent exactly the analogous supporting columns of the vertebral cups of the osseous fishes. Indeed, deprive the vertebræ of many of the osseous fishes of their spinous processes (those of the Haddock for example), and it will appear evident to every one that these supporting columns are not only of equal number, but subserve the same useful purposes, in both classes of fishes.

In the Rays, every space between the supporting columns is filled up by cartilaginous matter, so as to cause the body of each vertebra to assume the form of a short cylinder; and it is this cartilaginous envelope which has hitherto prevented the above described osseous structure from being recognised.

In almost all the vertebræ, however, there is an additional means used to strengthen the osseous cups, and prevent them from being fractured in violent motions of the animal. It is well known that cartilaginous, like other fishes, possess only a limited power of flexing the spinal column in a perpendicular direction. The generally square or triangular form of the cartilaginous envelopes of the vertebræ, but especially their large interlocked processes, effectually prevent much power of flexion in a perpendicular direction. Lateral motion, however, is freely allowed; and as we have seen that the lateral osseous supporting columns do not strengthen above half the diameter of the cups of the vertebræ, had no other means been used, there might have been some chance of the vertebræ giving way in this direction, during violent motions of the animal. This, however, is guarded against by a most beautiful arrangement.

From the lateral edges of the cup-shaped bodies arise from two to six or more processes of true bone, which, however, after extending a very little way to meet those from the opposite cup of the same vertebra, are continued as a chain of calcareous granular particles, resembling those forming the calcareous encrustations of the skeleton (Fig. 6, *a*). Between these are frequently remarked lines of square calcareous granules running in the same direction—viz., from the outer margin of the upper cup to that of the lower cup of the same vertebra. This arrangement, consequently, forms a series of supporting pillars, partly osseous and unyielding, partly composed of yielding jointed particles. As these jointed lateral supports are attached to the margin of the cups, while the lateral solid columns above described do not extend over more than from a third to a half of the outer surface of the cup, a free space intervenes between these two kinds of support,—a space filled up in the recent vertebra with cartilaginous matter. (Fig. 7.)

Fig. 6.

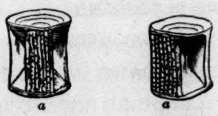


Fig. 7.

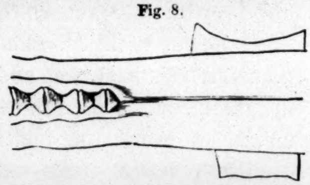
This apparatus seems beautifully adapted to the purpose of allowing a great amount of lateral motion without endangering the rupture of the vertebræ. The shortness of the internal solid osseous

column permits a small amount of flexibility in the body of each vertebra; and the jointed external lateral supports, while they prevent that flexion being carried too far, in consequence of the square particles of calcareous matter supporting each other, tend, by their partly cartilaginous nature, to restraighten the vertebræ.

As already remarked, the above described osseous structure was found present in the vertebral column of the Common Skate, Thornback Ray, Long-nosed Ray, and Starry Ray; so that I have no doubt it is present in all this order of cartilaginous fishes. As might be expected, the minute disposition of these parts varies a little in each species, yet not so much as to deviate in any material point from the above description.

Before leaving the Rays it may be as well to point out the mode in which these osseous portions of the vertebræ are united to the cartilaginous tube which occupies the place of the cervical vertebræ (if we may so speak) in these animals. All writers, as above noticed, simply describe this portion of the spine as a cartilaginous tube. But it is instructive to remark, that this cartilaginous tube, at its lowest part, contains the solid osseous bodies of several vertebræ, united to each other by the same regular intervertebral ligamentary apparatus, as the free vertebræ. These osseous portions, however, as they approach the head, gradually diminish in size, and at last terminate in a minute cup, which is regularly articulated to the lower vertebra, but superiorly gives place to the cartilaginous mass which supplies the place of vertebræ in the upper part of the spine. (Fig. 8.)

Fig. 8.



Being anxious to ascertain whether another great family of cartilaginous fishes contained osseous nuclei in their vertebral column, I procured a few species of Dog-fish, animals belonging to the family of Squalidæ or Sharks, as this family had been characterised by Baron Cuvier as one of those whose skeletons contained no true bone, but consisted of cartilage, soft within, and with an interspersion of calcareous granules on the surface only.

The internal nucleus of the vertebræ of these animals was found to consist of a truly osseous basis, of much the same general form as that of the Rays. Each vertebra consisted essentially of two cup-shaped bodies, or hollow truncated cones, united by their truncated apices, and resembled wooden egg-cups still more closely than the vertebræ of the Rays. (Fig. 9, *a*). When perfectly cleaned, it was seen that the cup on the one extremity of the vertebra communicated by a large rounded aperture with that on the opposite extremity, as had been remarked in 1809 by Sir Everard Home, when describing the intervertebral structure of the *Squalus maximus*, or Basking Shark. The aperture is smaller in the more minute vertebræ, and of larger diameter in the larger vertebræ.

The two cup-shaped bodies of which each vertebra is composed, are, on their outer margin, not perfectly circular, but of an irregular, somewhat hexagonal, form, and consist of a truly osseous structure, differing little, excepting in increased density, from the vertebral bones of osseous fishes. The osseous matter is, as in osseous fishes, deposited in concentric rings. The mode in which these cups are strengthened differs, however, as might be expected, from that observed in the Rays. The motions of this family of fishes are more quick and varied than those of the rays; and no class, perhaps (Eels alone excepted), possesses a greater amount of flexibility in their spinal column than the Squalidæ or Sharks.

The double cup-shaped vertebræ, then, are not supported by pillars or plates which fill up the angle formed by the sloping sides of each cup, as it is in the Rays, but the supporting plates are wholly external, and consist of six flat plates of osseous matter, stretching from the outer edge of the one cup to the outer edge of the other of the same vertebra. (Fig. 9, *b*). The breadth of each plate corresponds to that of the hexagonal side of the cup which it supports. The anterior or abdominal side being much the narrowest, possesses the narrowest supporting plate; but this plate is in general grooved. The other plates are of nearly equal breadth, and almost touch each other by their margins. In the recent animal, however, the margins are prolonged into cartilaginous processes, covered with calcareous particles, and form the various vertebral laminæ, processeses and canals, remarked on the spinal column. The free space between the osseous cups and the supporting plates is filled up with tough cartilage—a cartilage removeable by boiling. (To exhibit this, a section is represented at Fig. 9, *c*.)

Fig. 9.

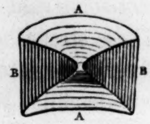
Notwithstanding the somewhat hexagonal form of the external margin of the cups of each vertebra, the articular surfaces, or rather the internal cavity, of the cup is nearly circular.

The same general structure was found present in the Spinax Acanthias, the Picked Dog-fish, in the *Scyllium Canicula*, the Spotted Dog-fish, the *Scyllium Catulus*, the Common Dog-fish, and the *Galeus vulgaris*, the Common Tope. It is reasonable, therefore, to infer, that the same structure will be found present in all the allied species.

In the other genera of the great family of Squalidæ or Sharks, the osseous portion of the vertebral column presents various modifications of structure. In the *Selache maximus*, or Basking Shark, the vertebræ are of much larger dimensions, in proportion to the size of the animal, than in the genera above alluded to. This is easily accounted for when the structure of these vertebræ is examined. The osseous matter is not, as in the dog-fish, deposited as a central nucleus of stony hardness, but is deposited in concentric plates, each of which is separated from the adjoining one by a layer of cartilage. From the centre to the circumference of the vertebræ, therefore, there is presented alternate layers

of bone and cartilage. These osseous plates are all beautifully cribriform, and, by their numerous apertures, allow a free communication between the adjoining layers of cartilage. The plates of bone, however, do not appear to send processes from one osseous plate to the other: each forms an independent plate or layer encircling the layers within it. The adjoining figure represents a section of this vertebra,—A A representing the section of the two cup-shaped articular surfaces, B B the concentric layers of osseous matter. The internal layers are more condensed than the external ones. It ought to be mentioned, that the osseous plates are partially interrupted on four sides.

Fig. 10.

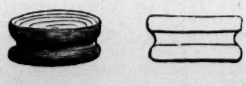


With this structure no additional means are required for giving support to the vertebræ in violent motions of the animal, and none else are found present.

In the *Carcharias vulgaris*, or White Shark, each vertebra is composed of two very flat slightly hollowed discs, formed of concentric layers of osseous matter. These discs are supported on four sides by very broad supports, which extend from the outer margin to the centre of the discs. The lateral supports are by much the broadest; in fact, each supports about a third of the circumference of the vertebral cups. These supporting columns are formed of plates of spongy osseous matter, which extend from the circumference to the centre of the vertebræ, and, in this respect, differ essentially from those of the *Selache maximus*, which have a concentric arrangement. A central aperture perforating the body of the vertebra is barely perceptible.

In a young specimen of the *Pristis antiquorum*, or Saw-fish, an animal belonging to the same family as the Sharks, I found the internal portion of each vertebra composed of a solid osseous portion, which, however, differed in its specific characters from that of the other cartilaginous fishes. Each vertebra was composed of two flat, but strong, somewhat rounded, cup-shaped bodies, so flat, however, as to resemble two discs united by a narrow neck. In fact, the vertebral column almost exactly resembled a continued series of the joints of some of the Crinoideæ. I could not detect any aperture piercing the body of the vertebra, so as to allow the flat cup-shaped articular surface of the one side to communicate with that of the other. As the specimen was a young one, and had but few of the vertebræ remaining, the rest having been removed for the purpose of stuffing it, I am unable to give further particulars regarding them, or of the mode by which they are strengthened; but, from their form, great strength, and solidity, I do not think it likely that they receive any strengthening pillars. Figure 11 represents the general form of the osseous portion of the vertebræ of the Saw-fish—viz., two rings or discs separated by a neck, and hollowed into shallow cups on each articular surface.

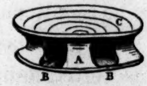
Fig. 11.



In the *Chimæra*, which belongs to a family closely allied to the Sharks, the

vertebræ are extremely solid, and bear a very close resemblance to those of the saw-fish. Each vertebra is formed of two flat shallow saucer-shaped discs of bone, united to each other by four very broad osseous pillars which extend from the margins of the cups to the centre of the vertebra. The margins of the cups project somewhat beyond those broad supports which form the body of the vertebra. These strengthened portions are on the abdominal and dorsal, and two lateral surfaces: the intermediate portions, however, are hollowed out, and form square cavities filled with cartilaginous matter, which extend nearly to the centre of the vertebra. (Fig. 12 represents one of the dorsal vertebræ. A the anterior or abdominal supporting plate; B the lateral hollow; C the flat articular surface.) The examination of the articular surface shews that, in these vertebræ, the osseous matter of the cups is deposited in concentric layers, as in all the other cartilaginous fishes; but not having had an opportunity of making a section of the bone, I am unable to speak of the peculiar disposition of the supporting plates which form the body of the vertebra.

Fig. 12.



Two other orders of fishes are arranged under the chondropterygious subclass, viz., the *Cyclostomi* and *Sturiones*. These two orders, however, differ in many essential points from those cartilaginous fishes the structure of whose vertebræ we have been considering: For, while the *Plagiastomi*, which include all the Sharks and Rays, possess, generally speaking, an organization of a higher order than that of the osseous fishes, the *Sturiones*, but especially the *Cyclostomi*, possess an organization of an inferior order,—an organization which renders them in some measure the connecting link between the more highly organized Mollusca and the Fishes.

This circumstance is accordingly not only seen in their digestive, generative, and nervous development, but extends, in a remarkable degree, to what ought to constitute their internal skeleton. In the *Sturiones*, or Sturgeons, the spinal column is formed of a continuous purely cartilaginous tube, divided at intervals into regular pieces, but with no osseous matter whatever in that part which ought to constitute the bodies of the vertebræ. Mr JONES, therefore, must have made some mistake in the passage above quoted, in instancing the spinal column of the Sturgeon as one in which osseous matter is encroaching on the cartilaginous matter of the bodies of the vertebræ; while Dr GRANT and CUVIER are strictly correct in describing the vertebral column of the Sturgeon as consisting of soft transparent cartilage, having earthy salts only deposited on the laminæ.

It is to be recollected that the Sturgeon is one of those animals in which the external skeleton is very strongly developed; and the different rows of osseous plates or shields are so placed as mutually to support one another, and thus keep the body extended, even although no osseous structure is developed in the spinal column.

I have not had an opportunity, as yet, of examining minutely the vertebral column of the Lamprey, Pride, or Hake, the fishes which belong to the order *Cyclostomi;* but I have every reason to believe, that Cuvier and Dr Grant are correct in describing that column as consisting of a simple cartilaginous tube.

As doubts may arise in the minds of some as to whether the osseous structure, which is above described, be really bone, or is simply condensed cartilage, impregnated with earthy matter, I submitted it to a chemical examination, and found it to yield in every case the same amount of earthy and of animal matter as the most solid bones of osseous fishes.

The osseous portion of the vertebræ of the Skate and Thornback Ray yielded 69.1 per cent. of earthy, and 30.9 of animal matters; while the common cartilaginous skeleton of the same animals yielded only 35.0 per cent. of earthy, and 65.0 per cent. of animal matters. In the various species of Dog-fish or *Squalidæ* examined, the proportions of earthy and of animal matters were found to be within a fraction of those of the Rays, being, in the perfectly cleaned osseous portion of the vertebræ, 68.9 per cent. of earthy, and 31.1 per cent. of animal matters. In the osseous laminæ of the vertebræ of the *Squalis maximus* or Basking Shark, I found 71.5 per cent. of earthy matters, and 28.5 per cent. of animal matters. These proportions are almost the very same as those which I have found to be present in the perfectly cleaned bones of osseous fishes. The earthy matters were chiefly composed of phosphate of lime; carbonate of lime was also present, but in very small quantity.

Chevreul and Müller are the only writers known to me who have published analyses of the hard parts of cartilaginous fishes. Chevreul appears to have undertaken the analyses for Baron Cuvier, and in 100 parts of the vertebra of the Basking Shark (*Squalus maximus*), he found of azotized matter and of oil 64.85, sulphate of soda 18.59, chloride of sodium 13.62, subcarbonate of soda 2.00, phosphate of lime, &c., only 0.94. M. Chevreul thought that all these soluble salts did not exist in a solid state in the cartilage, but were held in solution by the large quantity of water which recent cartilage contains,—a quantity amounting to no less than 90 per cent. of the weight of recent cartilage.

It is quite apparent from this analysis, that M. Chevreul had only analysed a portion of the enveloping cartilage of the vertebræ, and none of the truly osseous structure which constitutes the essential basis of these bodies. The circumstance of meeting with no phosphates proves this point:—it does more; it proves he had not even included in his analysis any appreciable amount of the concentric osseous plates of the vertebræ.

Müller, on the other hand, analysed each of the varieties of cartilage which he found present in cartilaginous fishes. In what he calls tubercular cartilage, he found a small proportion of earthy matter, which chiefly consisted of the phosphate of lime. In what he terms the ossified cartilage, he found in one case

41.55 per cent. of earthy matter, and in another 42.068 per cent. of earthy matter. These earthy matters consisted chiefly of phosphate of lime, with a small proportion of carbonate of lime, sulphate of lime, muriate of soda, &c.

It is evident from this analysis, that MÜLLER had not discovered the existence of the osseous structure which I describe, but had analysed the whole vertebræ, laminæ and all, considering it all to be what he terms ossified cartilage. He had, in fact, made no distinction between the osseous nucleus and its enveloping cartilage, with its covering of calcareous granules. When I analysed the vertebra in the same way, that is to say, with its cartilaginous laminæ and enveloping cartilages, I got, in one instance, in the Skate 53.3 per cent. of earthy matter, and 46.7 of animal matter, and in the Thornback Ray 54.2 of earthy, and 45.8 of animal matters. But this gives no information as to the real amount of earthy or animal matters in any part of the structure, seeing that the osseous base of the vertebra contains a very different amount from the enveloping cartilage; and seeing, also, that the proportions will vary according to the relatively greater or lesser size of the cartilaginous laminæ and enveloping cartilage.

The essential portion, then, of the vertebræ of cartilaginous fishes is true bone, which has the same composition, and the same concentric laminar arrangement, as the bones of osseous fishes. This fact appears to be of no small importance in enabling us to arrive at more just conclusions regarding the position which cartilaginous fishes ought to occupy in the scale of animated beings. Possessing, as all the *Plagiostomi* do, in so far as their nervous, generative, and digestive, systems are concerned, an organization superior to that of most fishes, it always appeared an anomaly that they should, by their imperfect skeletons, approach so much nearer the mollusca than other fishes. The discovery, however, of a perfect osseous structure in their vertebral column,—that column which is the distinguishing mark of their belonging to the higher classes of animated beings,—at once serves to explain the supposed anomaly, by shewing it resulted from an imperfect knowledge of their true anatomical structure.

In fact, the *Plagiostomi* ought to constitute a separate sub-class of fishes; and, in a descending scale of organization, be placed at the head of the fishes, as they manifestly form the connecting link between the Fishes and Reptiles.* While the *Sturiones* and *Cyclostomi* ought to constitute another and distinct sub-class, to which the term *Cartilaginous* might still be retained, and be placed after the Osseous fishes in the descending scale of natural classification, forming, as they undoubtedly do, the connecting link between the higher organised mollusca, and the lower organised fishes.

* Even the intervertebral ligamentary apparatus in the *Plagiostomi* makes a very close approach to the same structure in the *Amphibia*. In many of the species, especially in those whose vertebral cups are flat, it consists of concentric rings of fibro-cartilaginous matter, with softer albuminous or albumino-cartilaginous matter between them.

But, though these facts be interesting to the student of zoology, it is, perhaps, to the geologist to whom they will prove of most value. It is a known fact in geology, that though the dorsal spines, teeth, and palates, of Sharks and other cartilaginous fishes, have been met with in great abundance in various strata, few or no remains of their skeletons have been discovered; and Dr GRANT,* and other writers on comparative anatomy, state, that there is no likelihood of such remains being ever found, on account of the destructible nature of the cartilage, of which they suppose the skeleton is alone formed. It is true that M. AGASSIZ, in his great work on Fossil Fishes, has given five figures of three nearly complete impressions of cartilaginous fishes,—viz., of one allied to the Dog-fish—one allied to the Saw-fish,—and one allied to the Skate. In these impressions, the forms of the vertebral column, as well as of the fins and scales, have been preserved; but still, with these exceptions, it is from the scales, teeth, palates, or dorsal spines alone, that all the species of cartilaginous fishes are known. The vertebræ, in fact, have not been recognised when met with in a separate state. It is interesting, however, to remark, that in those impressions of entire fossil cartilaginous fishes, figured by M. AGASSIZ, the form of the osseous vertebræ may at once be recognised, so as, from their character alone, to determine to what order the fish belonged. Thus, in the two plates representing the *Spinacorhynus polyspondylus* (Plates 42 and 43 of Vol. III.), any one who previously examines the vertebræ of the Saw-fish, laid on the table, could at once say that the animal figured by M. AGASSIZ must belong to that division of cartilaginous fishes. The same is true of the other impressions of cartilaginous fishes figured in that work.

One circumstance may be noticed as shewing the probable importance of recognising in the fossil state these osseous portions of the vertebral column. When the teeth or dorsal spines of cartilaginous fishes are found imbedded in the strata, the size of the animal to which they belonged can only be judged of by comparing these remains with the analogous structures of recent species. In this way, however, wrong inferences may occasionally be drawn. Thus, in contact with one of the dorsal spines of a Shark figured by M. AGASSIZ, which, from its size, he thinks had belonged to a very large species (the Spinax major), a few vertebral remains occur. From their character, they evidently belong to the same animal to which the dorsal spine belonged; but they prove that, far from the animal having been of gigantic dimensions, it could not have exceeded two feet in length—so much have the weapons of defence of the antediluvian races exceeded those of our day.

Not being sufficiently conversant with this branch of geology, I applied to one or two known geologists, and through them to the great Oxford authority,

* GRANT, Lectures in Lancet, Jan. 1834, p. 576.

to ascertain whether the vertebræ of cartilaginous fishes were known to occur in a fossil state,—whether they had themselves seen any,—or, if recognised, what they were taken for. In answer, it was stated, that the vertebræ of cartilaginous fishes were unknown in a fossil state, nor were they ever expected to be met with on account of the destructible nature of the cartilaginous matter of which they were supposed to be alone composed. Dr HIBBERT WARE added, that M. AGASSIZ had several times expressed to him the same sentiment. Both Dr HIBBERT WARE and Mr TREVELYAN, however, think they have seen in the chalk, and in the new tertiary strata, bodies like what I have shewn constitute the essential portion of the spinal column of cartilaginous fishes. This, along with the circumstance of the exact figures of the osseous portions of the vertebræ being preserved in the impressions of the cartilaginous fishes figured by M. AGASSIZ, shews that, when the attention of geologists is drawn to the subject, they will probably meet with them in the same strata as those in which the teeth and spines occur.

Specimens of the structures above described were laid on the table.

21 HERIOT ROW.

XLII.—*On the Conversion of Relief by Inverted Vision.* *By* Sir David Brewster, K.H., D.C.L., F.R.S., and V.P.R.S. Edin.

(Read 6th May 1844.)

Under the name *Conversion of Relief*, an expression first used by Mr Wheatstone, I include all those optical illusions which take place in the vision of cameos and intaglios, of elevations and depressions, whether they are produced with opaque or transparent bodies,—on surfaces with or without shadows,—in reflected or transmitted light,—while using one or both eyes,—or by erect or inverted vision. In these various forms of the phenomenon, the illusion is modified by certain secondary causes, which were regarded both by Mr Wheatstone* and myself† as primary causes; so that we were led away, each in a different direction, from the right path of inquiry.

The phenomenon occurs in its most general and simple form, when it is produced by viewing a shadowless depression, or elevation, made in an extended surface, through an inverting microscope, or the inverting eye-piece of a telescope, and at an angle intermediate between 0° and 90°. In so far as I know, the phenomenon has never been thus limited, and, consequently, no explanation of it has ever been given. That which I shall now submit to the Society is capable of the most rigorous demonstration; and when it is once in our possession, we can have no difficulty in recognising the secondary causes which increase or diminish the influence of the primary one, and which, in its absence, are sometimes the immediate cause of the illusion.

Let A, Fig. 1, be a deep spherical concavity, and A′, Fig. 2, a high spherical

Fig. 1.

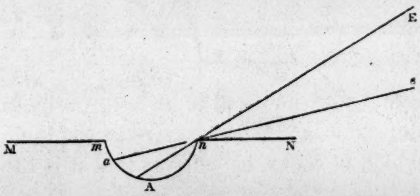
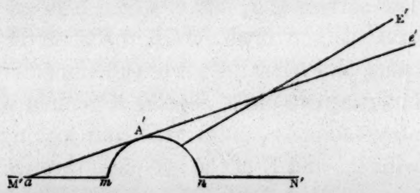


Fig. 2.



convexity in an extended horizontal table M N, M′ N′, and let them be shadowless, or illuminated by a *quaquaversus* light, like that of the sky. If the observer, placed at a moderate distance, view these objects in the directions E A, E′ A′, either with

* *Phil. Trans.*, 1838, pp. 383, 384.

† *Edin. Trans.*, Vol. xv. p. 365; *Edin. Journal of Science*, Vol. iv. p. 97; and *Letters on Natural Magic*, p. 98.

one or with both eyes, his accurate appreciation of the distances E A, E′ A′, will prove to him that A is a *concavity*, and A′ a *convexity ;* but if E A, E′ A′ approach to equality, either from the distance of the observer, or from the shallowness of A, or the slight elevation of A′, he will cease to recognise any difference in the distances E A, E′ A′, and will be unable to tell which is the convexity, and which the concavity. So great, indeed, is this uncertainty, that, from causes which he cannot discover, they will sometimes appear convex and sometimes concave. In this indetermination of the judgment, a touch of A, A′ by the finger, or the introduction of a shadow, will remove or confirm the illusion, whatever it may be. The same result will be obtained, if we view A and A′ vertically, with an erect or inverting eye-piece. In all these cases, we suppose that the circular, or rather the elliptical, base of the convexity or concavity is distinctly seen.

Let us now look at A, A′, at obliquities varying from 0° to 90°. In Fig. 1 the *concavity* A will have an elliptical section at all obliquities, till, at 90°, it appears a straight line; but in the *convexity* the effect is very different. In passing from 0° to the position E′, Fig. 2, the circular section of A′ will appear an ellipse; but in passing from E′ to 90°, the appearance of A′ will lose all resemblance to A. When the eye is at *e*′, for example, the summit A′ of the convexity will cover the point *a* of the table, and *a m* will be invisible; and near 90°, the convexity A will eclipse the whole surface of the table *m* M, however extended it may be, and will rise above it.

Let us now suppose that the eye at E, Fig. 3, views the concavity A through the inverting eye-piece E G H, the horizontal table M N must obviously be inverted as well as the hollow A; but the *apparent* change, produced by inversion, is very different from the real change. The surface M N, out of which A is excavated, and upon which the observer leans, and rests the lower end H of his inverting eye-piece, appears to remain where it was, and still to look upwards, in place of appearing inverted, and looking downwards. When he strikes the table with the end H of the eye-piece through which he looks, he believes that it is the lower end of the field of view that strikes the table, and rests upon it. With these convictions, he sees what is represented in Fig. 4. The concavity *m* A *n*, Fig. 3, appears inverted; and as the visible part of the concavity A *m*, Fig. 3, is nearest the eye in Fig. 4, and the invisible part A *n*, Fig. 3, farthest from the eye in Fig. 4, *m* A *n* must ap-

Fig. 3.

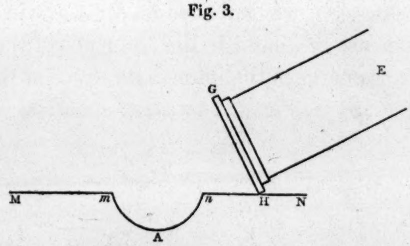
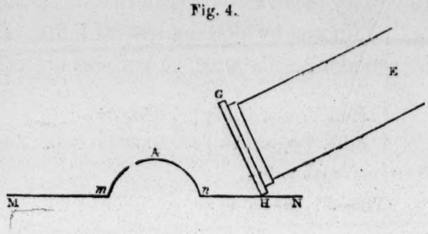


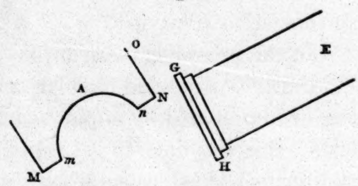
Fig. 4.



pear a concavity in Fig. 4, solely because it seems to rise out of the surface M N, which looks upward, as if it had not been inverted by the eye-piece.

Now, in this experiment, the conversion of the concavity into a convexity depends on two separate illusions, one of which springs from the other. The *first* illusion is the belief that the surface M N is looking upwards, whereas it is really inverted, as shewn in Fig. 5; and the *second* illusion, which arises from the first, is, that the point n appears *farthest* from the eye, whereas it is *nearest* to it, as shewn in Fig. 5. All these observations are equally applicable *mutatis mutandis* to the vision of convexities; and hence it follows, that the conversion of relief, occasioned by the use of an inverting eye-piece, is not produced directly by the inversion, but by an illusion, in virtue of which we conceive the remotest side of the convexity or concavity to be nearest our eye when it is not.

Fig. 5.



In order to demonstrate the correctness of this explanation, let the concavity m A n be made in a *narrow stripe* of wood, as in Fig. 5, and let it be viewed, as formerly, through the inverting eye-piece. It will now appear, as in Fig. 5, really inverted, and free from both the illusions which formerly took place. The narrow surface M N being now wholly included in the field of view, and the thickness N O of the stripe of wood distinctly seen, the inversion of the surface M N, which now looks downward, will be at once recognised. The edge n of the concavity will appear *nearest* the eye,* as it really is, and *the concavity, though inverted, will still appear a concavity.* The very same reasoning is applicable to a convexity on a narrow stripe of wood.

When, as in Fig. 4, the concavity is seen as a convexity, let it be viewed more and more obliquely. The *elliptical margin of the convexity will always be visible*, which is impossible in a real convexity; and the elevated apex will gradually sink till the elliptical margin becomes a straight line, and *the imaginary convexity completely levelled.* The struggle between truth and error is here so singular, that while one part of the Figure m A n has become concave, the other part retains its convexity!

In like manner, when a convexity is seen as a concavity, the concavity loses its true shape, as it is viewed more and more obliquely, till its remote elliptical margin is encroached upon by the apex of the convexity; and, towards an inclination of 90°, the concavity disappears altogether, under circumstances analogous to those already described.

If, in place of using an inverting eye-piece, we invert the concavity m A n, by

* The inversion of an object never makes the *nearer* part of an object *more remote*, nor the *remote* part *nearer*.

looking at its image in the focus of a convex lens, it will sometimes appear a convexity, and sometimes not. In this form of the experiment the image of the concavity, and consequently its apparent depth, is greatly diminished. Hence any trivial cause, such as a preconception of the mind, or an approximation to a shadow, or a touch of the hollow by the point of the finger, will either produce a conversion, or prevent it.

In the preceding experiments we have supposed the convexity to be high and the concavity deep and circular, and we have supposed them also to be shadowless, or illuminated by a quaquaversus light, such as that of the sky in the open fields. This was done to get rid of all secondary causes, which interfere with and modify the normal cause when the concavities and convexities are shallow, and have distinct shadows, or when the concavity has the shape of an animal, or any body which we are accustomed to see convex.

Let us now suppose that a strong shadow is thrown upon the concavity. In this case the normal experiment, already explained and shewn in Fig. 5, is much more perfect and satisfactory. The illusion is complete, and invariable when the concavity is in an extended surface; and it as invariably disappears when it is in a narrow stripe.

In the secondary forms of the experiment, the inversion of the shadow becomes the principal cause of the illusion; but, in order that the result may be invariable, or nearly so, the concavities must be shallow, and the convexities a little raised. At great obliquities, however, this cause of the conversion of Form ceases to produce the illusion, and in varying the inclination from 0° to 90°, the cessation takes place sooner with deep than with shallow cavities. The reason of this is, that the shadow of a concavity is very different at great obliquities from the shadow of a similar convexity. The shadow never can emerge out of a cavity so as to darken the surface in which the cavity is made; whereas the shadow of a convexity soon extends beyond the outline of its base, and, finally, throws a long stripe of darkness over the surface on which it rests. Hence it is impossible to mistake a convexity for a concavity, whenever its shadow extends beyond its base.

When the concavity is a horse or a dog upon a seal, it will often rise into a convexity when seen through a single lens which does not invert it; but the illusion disappears at great obliquities. In this case the illusion is favoured, or produced, by two causes: the first is, that the convex form of the horse or dog is the one which the mind is most disposed to seize; and the second is, that we use only one eye, with which we cannot measure depths as well as with two. The illusion, however, still takes place when we employ a lens *three* or more inches wide, so as to admit the use of both eyes, but it is less certain, as the binocular vision enables us to keep in check, to a certain degree, the other causes of illusion.

The influence of these secondary causes is strikingly displayed in the follow-

ing experiment. In the armorial bearings upon a seal, the shield is often more deeply cut than the surrounding parts. With binocular vision the shallow parts rise into a convexity sooner than the shield, or continue so while the shield remains concave; but if we shut one eye, the shield then becomes convex like the rest. In these experiments with a single lens, a slight variation in the position of the seal, or a slight change in the direction or intensity of the illumination, or particular reflections from the interior of the stone, will favour or oppose the illusion. In viewing the shield, or the deepest portion, with a single lens, a slight rotation of the seal round the wrist, backwards and forwards, will remove the illusion, in consequence of the eye perceiving that the change in the perspective is different from what it should be.

In a paper in the *Edinburgh Journal of Science*, already referred to, I have described several other examples of the conversion of form, in which inverted vision is not employed. As seen by the naked eye, hollows in *mother of pearl*, and other semi-transparent bodies, rise into relief; and the same thing happens on surfaces of agate and woods of various kinds, when transparent circular portions are illuminated by refraction, at those parts of their circumference where they would have been illuminated had they been convexities.* But the most interesting cases of conversion of form are those in which *the mind alone* operates, and receives no aid either from inversion, shadow, or monocular vision. "If we take, as I have elsewhere remarked, one of the Intaglio moulds, used in making the bas-reliefs of that able artist Mr HENNING, and direct the eyes to it steadily, without noticing surrounding objects, we may coax ourselves into the belief that the Intaglio is actually a bas-relief. It is difficult at first to produce the deception, but a little practice never fails to accomplish it. We have succeeded in carrying this deception so far as to be able, by the eye alone, to raise a complete hollow mask of the human face into a projecting head. In order to do this we must exclude the vision of other objects; and also the margin or thickness of the cast. This experiment cannot fail to produce a very great degree of surprise in those who succeed in it; and it will, no doubt, be regarded by the sculptor (who can use it) as a great auxiliary in his art."†

From these observations it will be seen that the conversion of Form, excepting in the normal case, depends upon various causes which are effective only under particular conditions; such as the depth of the hollow or the elevation of the relief—the distance of the object—the sharpness of vision—the use of one or both eyes—the inversion of the shadow—the nature of the object—and the means

* In examining, under the microscope, the shallow fluid cavities *within* the substance of a film of sulphate of lime, described in the *Edinburgh Transactions*, vol. x. p. 35, they frequently appeared as *elevations* on the surface of the plate next the eye.

† *Edinburgh Journal of Science*, No. VIII. p. 109, Jan. 1826.

used by the mind itself to produce the illusion. In the normal case, however, where the cavity or convexity is shadowless, and upon an extended surface, and where inverted vision is used, the conversion of Form depends solely on the illusion, which it is impossible to resist, that the side of the cavity or elevation next the eye is actually farthest from it—an illusion not produced by inversion, but by a false judgment respecting the position of the surface on which the form is placed.

ST LEONARD'S COLLEGE, ST ANDREWS,
May 4. 1844.

XLIII.—*On the Knowledge of Distance given by Binocular Vision.* By Sir DAVID BREWSTER, K.H., D.C.L., F.R.S., and V.P.R.S. Edinburgh.

(Read 15th April 1844.)

IN analysing Mr WHEATSTONE'S beautiful discovery, that in binocular vision we see all objects of three dimensions by means of two dissimilar pictures on the retina, I trust I have satisfied the Society that the dissimilarity of these two pictures is in no respect the cause of our vivid perception of such objects, but, on the contrary, an unavoidable accompaniment of binocular vision, which renders it less perfect than vision with one eye. On the other hand, it is quite true that, in Mr WHEATSTONE'S experiment of producing the perception of objects of three dimensions by the apparent coalescence of two dissimilar representations of such objects *in plano*, the dissimilarity of the pictures is necessary in the exhibition of that beautiful phenomenon.

In performing, with the eye alone, the various experiments detailed in a former paper, I was very much struck with the fact, that the apparent solid figure, produced by the union of its dissimilar pictures, never took its right position in absolute space: that is, in place of appearing suspended between the eye and the plane upon which the dissimilar figures were drawn, the base of the solid seemed to rest on that plane, whether its apex was nearer the eye or more remote than its component plane figures.

With the view of finding the cause of this, I placed the component figures on a plate of glass suspended in the air, so as to have no vision of the surface on which they rested, and after uniting these figures by binocular vision, and concealing the two outstanding single figures, I obtained results which, though not entirely satisfactory, proved that there existed some disturbing cause which prevented the united image from placing itself in the *binocular centre*, or the intersection of the optical axes. This disturbing cause was simply the influence of other objects in the same field of view, whose distance was known to the observer.

In order to avoid all such influences, and to study the subject under a more general aspect, it occurred to me that these objects would be gained by using a numerous series of plane figures, such as those of flowers or geometrical patterns upon carpets or paper-hangings. These figures being always at equal distances from each other, and almost perfectly equal and similar, the coalescence of any pair of them, by directing the optic axes to a point between the paper-hangings and the eye, is accompanied with the coalescence of every

other pair. When the observer, therefore, places himself in front of that side of a papered room in which there are neither doors nor windows, and conceals from his eye the floor, the roof, and the right and left hand sides of the room, the whole of his retina will be covered with the images of the united plane figures, and there will be no interposing objects to prevent him from judging of the distance of the picture that may be presented to him.

Let the observer, therefore, now place himself *three* feet in front of the papered wall, and unite two of the figures, suppose two flowers, at the distance of *twelve* inches. The whole wall will now be presented to his view, consisting of flowers as before, but each flower will be composed of two flowers superimposed at the binocular centre, or the point of convergence of the optical axes. If we call D the distance of the eyes from the wall or *three* feet, C the distance between the eyes or two-and-half inches, and d the distance between the similar parts of the two flowers, we shall have x the distance of the binocular centre from the wall, $x = \frac{D\,d}{C+d} = 30$ inches nearly, and $D - x = 6$ inches, the distance of the binocular centre from the middle point between the two eyes.

Hence the whole papered wall, with all its flowers, in place of being seen, as in ordinary vision, at the distance of *three* feet, *is now suspended in the air, at the distance of six inches from the observer.* In maintaining this view of the wall, the eye will, at first, experience a disagreeable sensation; but after a few experiments the sensation will disappear, and the observer will contemplate the new picture with the same satisfaction and absence of all strain as if he were looking directly at the wall itself: for there is a natural tendency in the eyes to unite two similar pictures, and to keep them united, provided they are not too distant.

When this picture is at first seized by the observer, he does not, for a while, decide upon its distance from himself. It sometimes appears to advance from the wall to its true position in the binocular centre, and, when it has taken its place, it has a very extraordinary character:—the surface seems slightly convex towards the eye; it has a sort of silvery transparent aspect, and looks more beautiful than the real paper; it moves, with the slightest motion of the head, either laterally or to or from the wall. If the observer, who is now *three* feet from the wall, retires from it, the suspended wall of flowers will follow him, moving farther and farther from the real wall, and also, but very slightly, farther and farther from the observer: that is, the distance of the observer from the real wall increases faster than the distance of the suspended wall from it, according to the law expressed by the preceding formula. The binocular centre, therefore, recedes from the eye as the observer retires, and the strain consequently diminishes.

in order to observe these phenomena in the most perfect manner, the paper

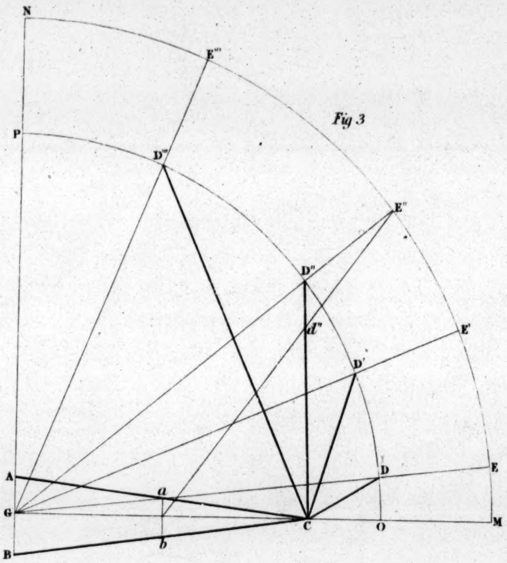


Fig 3

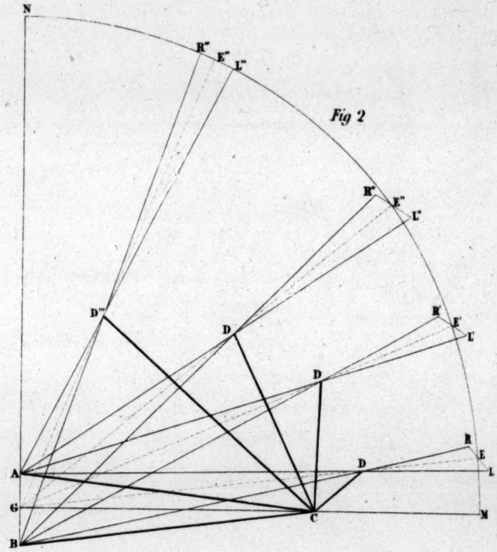


Fig 2

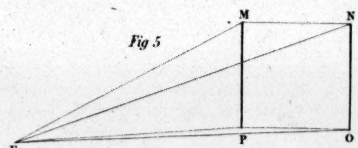


Fig 5

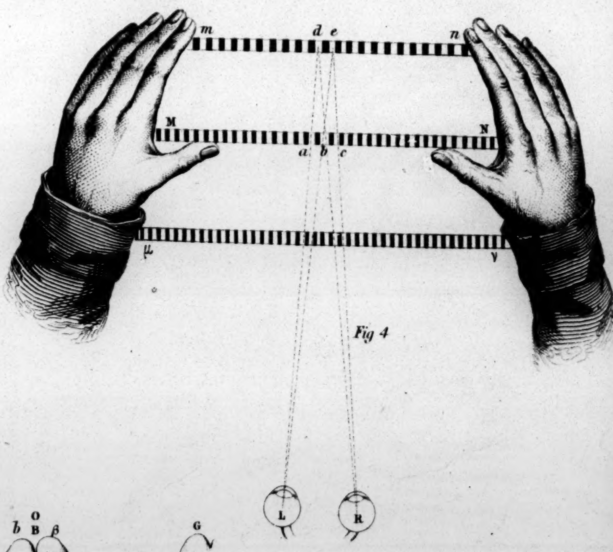
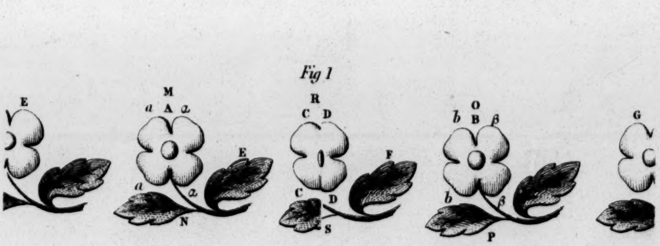


Fig 4

Fig 1



should be pasted upon a large screen, previously unseen by the observer, unconnected with the roof or the floor, and placed in a large apartment. The deception will then be complete; and when the picture stands suspended before the observer, and within a few inches of himself, he may stretch out his hand and place it on the other side of the picture, and even hold a candle on the other side of it, so as to satisfy himself that in both cases the picture is between his hand and himself.

When we survey this picture with attention, several very curious phenomena present themselves. Some of the flowers, when narrowly examined, appear somewhat like real flowers. In some the stalk gradually retires from the general plane of the picture; in others, it rises above it: one leaf will come farther out than another, or the flower will appear thicker and more solid, deviating considerably from the plane representation of it seen by each eye separately. All this arises from slight and accidental irregularities in the two figures which are united, thus producing an approximation to three dimensions in the picture. If the distance, for example, of the ends of two stalks in two coalescing flowers is greater than the distance of corresponding points in other parts of the stalk, the end of the stalk will rise from the general surface of the figure, and *vice versa*. In like manner, if the distance between two corresponding leaves is greater than the distance between other two corresponding leaves, then the two first, when united, will appear nearer the eye than the other two, and hence the appearance of a solid flower is partially given to the combination. These effects are better seen in old and imperfectly made paper-hangings than in those which are more carefully executed.

In continuing our survey of the suspended image, another curious phenomenon presents itself: a part of one of the pieces of paper, and sometimes a whole stripe from the roof to the floor, will retire behind the general plane of the image, or rise above it; thus displaying, on a large scale, an imperfection in the workmanship which it would have required a very narrow inspection to discover. This defect arises from the paper-hanger having cut off too much of the white margin of one or more of the adjoining pieces, so that when the two halves of a flower are united, part of the middle of the flower is left out; and hence when this defective flower is united with the one on the right hand of it, and the one on the left hand united with the defective one, the united or corresponding portion, being at a less distance, will appear farther from the eye than those parts of the suspended image composed of complete flowers. In like manner, if the two portions of the flowers are not brought together, but separated by a small space, the opposite effect will be produced. This will be understood from Fig. 1 (Plate 17), where M N, O P represent portions of two separate pieces of paper, each twenty-one inches wide. In this specimen, there are only two flowers in each piece, namely one white flower, A or B, and two halves. If the two halves

C, D, are united as in the figure, it is obvious that the flower is incomplete, a part of the central circle of the corolla having been cut off from each half. If we now, by straining the eye, unite C D with B, and also with A, then, at the same time, E will be united with the second or left hand image of A, and G with the second or right hand image of B. But since a piece has been cut out of C D, the half $\alpha\,\alpha$ of A is nearer the half D D than the other half $a\,a$ is to the other half C C; and, in like manner, the half $b\,b$ of B is nearer the half C C than the other half $\beta\,\beta$ is to the other half D D. Hence, when the strained eyes unite $\alpha\,\alpha$ to D D, the binocular centre is more remote than when $a\,a$ is united to C, and the same is true of the other halves; consequently, the halves D D and $b\,b$ must appear, as it were, sunk in the wall, or as farther removed from the observer; and if the defective cutting exists along the line R S from the floor to the ceiling, the whole stripe of paper between R S and O P, from the floor to the ceiling, will appear sunk in the papered wall. But if the defect is confined to a portion only of the flowers, then a rectangular space of the breadth R O, and of a height equal to the defective portion, will appear sunk in the paper. If every junction has the same defect as that at R S, then the whole will appear to consist of equal stripes, every alternate one being raised and the other depressed.

In the preceding example, there are only *two* flowers in a breadth, and their distance is $10\frac{1}{2}$ inches, which is also the breadth of the sunk stripes. But if the flowers are three or four in number, and their distance $\frac{21}{3}$, $\frac{21}{4}$ inches, the sunk stripes will vary according as we unite two flowers whose distances are in the one case 7 or 14 inches, and $5\frac{1}{4}$ or $10\frac{1}{2}$ or $16\frac{3}{4}$ or 21 in the other. Calling B the breadth of the paper, n the number of flowers or figures in that breadth, and W the width of the sunk stripe, then we have $W=\frac{B}{n}$ or $\frac{2\,B}{n}$ or $\frac{3\,B}{n}$ according as we unite the two nearest, or the first and second flower, the first and third, or the first and fourth. When W=B, the sunk stripes will cover the whole paper, and all the flowers will lie in the same plane.

These results afford an accurate method of examining and discovering defects in the workmanship of paper-hangers, carpet-makers, painters, and other artists whose profession it is to combine a series of similar patterns in order to form an uniform and ornamental surface. The smallest defect in the similarity and equality in the figures or lines which compose a pattern, and any difference in the distance of the single figures, is instantly detected; and, what is remarkable, a small inequality of distance in a line perpendicular to the axis of vision, or in one dimension of space, is exhibited in a magnified form as a distance coincident with the axis of vision, and in an opposite dimension of space!

At the commencement of this class of experiments, it is difficult to realize, and very easy to dissolve, the singular binocular picture which we have been

describing; but after the eyes have been drilled for a while to this species of exercise, the pictures become very persistent. Although the air-suspended image might be expected to disappear after closing one eye, and still more after having closed and re-opened both, yet I have found it in its original position in this latter case, and even after rubbing my eyes and shaking my head; and I have sometimes experienced a difficulty in ascertaining, after these operations, whether it was the real or the air-suspended wall that was before me. On some occasions a singular effect was produced. When the flowers on the paper are distant six inches, we may either unite two six inches distant, or two twelve inches distant. In the latter case, when the eyes have been accustomed to survey the suspended picture, I have found that, after shutting and opening them, I neither saw the picture formed by the two flowers twelve inches distant, nor the papered wall itself, but a picture formed by uniting the flowers six inches distant! The binocular centre had shifted its place, and instead of advancing to the wall, as is generally the case, and giving us ordinary vision of it, it advanced exactly as much as to unite the nearest flowers, just as on a ratchet wheel the detent slips over one tooth at a time; or, to speak more correctly, the binocular centre advanced in order to relieve the eyes from their strain, and when the eyes were opened, it had just reached that point which corresponded with the union of the flowers six inches distant.

In the construction of complex geometrical diagrams consisting only of fine lines, and in which similar figures are repeated at equal distances, it is very difficult to attain minute accuracy. The points of the compasses sink to different depths in the paper, and the lines which join such points seldom pass through their centres. Hence arises a general inaccuracy which the eye cannot detect; but if we examine such diagrams by strained binocular vision, their imperfections will be instantly displayed. Some parts will rise higher than others above the general level, and the whole will appear like several cobwebs placed at the distance of a tenth or a twelfth of an inch behind each other.*

In all the experiments made by Mr WHEATSTONE by the stereoscope, and in those described in my former paper, the dissimilar figures are viewed in a direction perpendicular to the plane on which they are drawn. A series of very interesting results, however, are obtained by uniting the images of lines meeting at an angular point, when the eye is placed at different heights above the plane of the paper, and at different distances from the angular point.

Let A C, B C be two lines meeting at C, the plane passing through them being the plane of the paper, and let them be viewed by the eyes at E‴, E″, E′, E at different heights in a plane G M N perpendicular to the plane of the paper.

* This effect is finely seen in the diagram of the Homogeneous Curve, which forms Plate IX. of Mr HAY's work "On the Harmony of Form."

Let R be the right eye and L the left eye, and when at E'' let them be strained so as to unite the points A, B. The united image of these points will be seen at the binocular centre D''', and the united lines A C B C will have the position D''' C. In like manner, when the eye descends to E''', E', E, the united image D''' C will rise and diminish, taking the positions D'' C, D' C, D C till it disappears on the line C M, when the eyes reach M. If the eye deviates from the vertical plane G M N the united image will also deviate from it, and is always in a plane passing through the eye and the line G M.

If at any altitude E M the eye advances towards A C B in the line E G, the binocular centre D will also advance towards A C B in the line E G, and the image D C will rise and become shorter as its extremity D moves along D G, and after passing the perpendicular to G E it will increase in length. If the eye, on the other hand, recedes from A C B in the line G E, the binocular centre D will also recede, and the image D C will descend to the plane C M and increase in length.

The preceding diagram is, for the purpose of illustration, drawn in a sort of perspective, and therefore does not give the true positions and lengths of the united images. This defect, however, is remedied in Fig. 3, where E, E', E'', E''' is the middle point between the two eyes, the plane G M N being, as before, perpendicular to the plane passing through A C B. Now, as the distance of the eye from G is supposed to be the same, and as A B is invariable as well as the distance between the eyes, the distance of the binocular centres O, D, D', D'', D''', P, from G will also be invariable, and lie in a circle O D P whose centre is G, and whose radius is G O, the point O being determined by the formula $G\,O = G\,D = \frac{G\,M \times A\,B}{A\,B + R\,L}$. Hence, in order to find the binocular centres D, D', D'', D''', &c., at any altitude E, E' &c., we have only to join E G, E' G, &c., and the points of intersection D, D', &c., will be the binocular centres, and the lines D C, D' C, &c., drawn to C, will be the real lengths and inclinations of the united images of the lines A C, B C.

When G O is greater than G C there is obviously some angle A, or E'' G M at which D'' C is perpendicular to G C. This takes place when cos. $A = \frac{G\,C}{G\,O}$. When O coincides with C, the images C D, C D', &c., will have the same positions and magnitudes as the chords of the altitudes A of the eyes above the plane G C. In this case, the raised or united images will just reach the perpendicular when the eye is in the plane G C M, for since G C = G O, cos. A = 1, and A = 0°.

When the eye at any position, E'' for example, sees the points A and B united at D'', it sees also the whole lines A C B C forming the image D'' C. The binocular centre must, therefore, run rapidly along the line D'' C: that is, the inclination of the optic axis must gradually diminish till the binocular centre

reaches C, when all strain is removed. The vision of the image D″ C, however, is carried on so rapidly, that the binocular centre returns to D″ without the eye being sensible of the removal and resumption of the strain which is required in maintaining a view of the united image D″ C.

If we now suppose A B to diminish, the binocular centre will advance towards G, and the length and inclination of the united images D C, D′ C, &c., will diminish also, and *vice versa*. If the distance R L (Fig. 2) between the eyes diminishes, the binocular centre will retire towards E, and the length and inclination of the images will increase. Hence persons with eyes more or less distant will see the united images in different places and of different sizes, though the quantities A and A B be invariable.

While the eyes at E′ are running along the lines A C, B C, let us suppose them to rest upon the points *a*, *b* equidistant from C. Join *a b*, and from the point *g*, where *a b* intersects G C, draw the line *g* E″, and find the point *d″* from the formula $g\,d'' = \frac{g\,E'' \times a\,b}{a\,b + R\,L}$. Hence the two points *a*, *b* will be united at *d″*, and when the angle E″ G C is such that the line joining D and C is perpendicular to G C, the line joining *d″* C will also be perpendicular to G C, the loci of the points D″ *d″ d′ d* will be in that perpendicular, and the image D C, seen by successive movements of the binocular centre from D″ to C, will be a straight line.

In the preceding observations we have supposed that the binocular centre D″, &c., is between the eye and the lines A C B C; but the points A, C, and all the other points of these lines, may be united by fixing the binocular centre beyond A B. Let the eyes, for example, be at E″; then if we unite A B when the eyes converge to a point, Δ″ (not seen in the figure), beyond G, we shall have $G\,\Delta'' = \frac{G\,E \times A\,B}{R\,L - A\,B'}$ and if we join the point Δ″ thus found and C, the line Δ′ C will be the united image of A C and B C, the binocular centre ranging from Δ″ to C, in order to see it as one line. In like manner, we may find the position and length of the image Δ‴ C, Δ′ C, and Δ C corresponding to the position of the eyes at E‴ E and E. Hence all the united images of A C, B C: viz. C Δ‴, C Δ″, &c., will lie below the plane of A B C, and extend beyond a vertical line N B continued; and they will grow larger and larger, and approximate in direction to C G as the eyes descend from E‴ to M. When the eyes are near to M, and a little above the plane of A B C, the line, when not carefully observed, will have the appearance of coinciding with C G, but stretching a great way beyond G. This extreme case represents the celebrated experiment with the compasses described by Dr Smith, and referred to by Professor Wheatstone. He took a pair of compasses, which may be represented by A C B, A B being their points, A C B C their legs, and C their joint; and having placed his eyes about E above their plane, he made the following experiment:—“ Having opened the points of a pair of compasses somewhat wider

than the interval of your eyes, with your arm extended, hold the head or joint in the ball of your hand with the points outwards, and equidistant from your eyes, and somewhat higher than the joint. Then, *fixing your eyes upon any remote object* lying in the plane that bisects the interval of the points, you will first perceive two pair of compasses (each by being doubled with their inner legs crossing each other, not unlike the old shape of the letter W.) But by compressing the legs with your hand, the two inner points will come nearer to each other; and when they unite (having stopt the compression), the two inner legs will also entirely coincide and bisect the angle under the outward ones, and will appear more vivid, thicker and larger, than they do, so as to reach from your hand to the remotest object in view even in the horizon itself, if the points be exactly coincident."* Owing to his imperfect apprehension of the nature of this phenomenon, Dr SMITH has omitted to notice that the united legs of the compasses lie below the plane of A B C, and that they never can extend farther than the binocular centre at which their points A and B are united.

There is another variation of these experiments which possesses some interest, in consequence of its extreme case having been made the basis of a new theory of visible direction by the late Dr WELLS.† Let us suppose the eyes of the observer to advance from E to N, and to descend along the opposite quadrant on the left hand of N G, but not drawn in fig. 3 (plate 17), then the united image of A C, B C, will gradually descend towards C G, and become larger and larger. When the eyes are a very little above the plane of A B C, and so far to the left hand of A B, that C A points nearly to the left eye, and C B to the right eye, then we have the circumstances under which Dr WELLS made the following experiment:—"If we hold two thin rules in such a manner that their sharp edges (A C, B C in Fig. 3) shall be in the optic axes, one in each, or rather a little below them, *the two edges will be seen united in the common axis* (G C in Fig. 3); and this apparent edge will seem of the same length with that of either of the real edges, when seen alone by the eye in the axis of which it is placed." This experiment, it will be seen, is the same with that of Dr SMITH, with this difference only, that the points of the compasses are directed towards the eyes. Like Dr SMITH, he has omitted to notice that the united image rises above G H, and he commits the opposite error of Dr SMITH, in making the length of the united image too short.

If in this form of the experiment we fix the binocular centre beyond C, then the united images of A C, B C descend below G C, and vary in their length, and in their inclination to G C, according to the height of the eye above the plane of A B C, and its distance from A B.

It is a remarkable circumstance, that no examples have been recorded of false estimates of the distance of near objects, in consequence of the *accidental*

* SMITH's Optics, vol. ii. p. 388, § 977. † Essay on Single Vision, &c., p. 44.

binocular union of similar images. This has, no doubt, arisen from the rare occurrence of these circumstances or conditions, under which alone such illusions can be produced. In a room where the paper hangings have a small pattern, or similar figures recurring at the distance of 1, $1\frac{1}{2}$, or 2 inches, a short-sighted person might very readily turn his eyes on the wall, when their axes converged to some point between him and the wall, which would unite one pair of the similar images; and, in this case, he would see the wall nearer him than the real wall, and moving with the motion of his head like something aerial. In like manner, a long-sighted person, with his optical axes converged to a point beyond the wall, might see an image of the wall more distant, and of an aerial character;—or a person who has taken too much wine, which often fixes the optical axes in opposition to the will, might, according to the nature of his sight, witness either of the illusions above mentioned.

In the preceding observations, we have confined ourselves to the binocular union of figures upon an opaque ground. This limitation almost necessarily precluded us from observing the results when the binocular centre is beyond the plane where these figures are situated, because it is not easy to adjust the eyes to a distant object, unless we look through the surfaces containing the figures. Now, this is by far the most interesting form of the experiment, and it has the advantage of putting scarcely any strain upon the eyes, not only because the binocular centre is more distant, but because we cannot, in this way, unite figures whose distance exceeds $2\frac{1}{2}$ inches, the interval between the eyes. Transparent patterns for these experiments may be cut out of stiff card paper, or thin plates of metal, or they may be made of paper pasted upon large panes of glass. Experiments may be made with trellis work, or with windows composed of small squares or lozenges; but the readiest pattern is the cane bottom of a chair, and I have performed my experiments by simply placing such a chair upon a high table, with its cane bottom in a vertical position. The distance of the centres of the eight-sided open figures in the direction of the width or depth of the chair, varies in different patterns from 0.54 to 0·76 of an inch. In order to simplify the calculations, we shall take the distance at 0·5, or half an inch. Then let

$D = 12$ inches be the distance of the pattern from the eyes.

$d = 0{\cdot}5$ the distance of the centres of the similar figures.

$+\Delta =$ distance of suspended image from, and in front of, the pattern.

$-\Delta' =$ distance of suspended image from, and behind, the pattern.

$C = 2{\cdot}5$ the distance between the eyes.

Then we shall have

$$+\Delta = \frac{D\,d}{C + d} \text{ and } -\Delta' = \frac{D\,d}{C - d}. \quad \text{Hence}$$

$D - \Delta =$ distance of suspended image from the eye, and in front of the pattern, and

$D + \Delta' =$ its distance from the eye, and behind the pattern.

From these formulæ we have computed the following table, adapted to similar figures, whose centres are distant $\frac{1}{2}$ an inch, 1, $1\frac{1}{2}$, 2, and $2\frac{1}{2}$ inches; but in reference to the positive values of Δ and D, we may consider them as feet, 0·5 being in that case = 6 inches.

D Inches.	$d = 0{\cdot}5$		$d = 1{\cdot}0$		$d = 1{\cdot}5$		$d = 2{\cdot}0$		$d = 2{\cdot}4$		$d = 2{\cdot}5$	
	$+\Delta$	$-\Delta$	$+\Delta$	$-\Delta$	$+\Delta$	$-\Delta$	$+\Delta$	$-\Delta$	$+\Delta$	$-\Delta$	$+\Delta$	$-\Delta$
6	1	1·5	1·72	4	2·25	9	2·66	24	2·94	144	3	Infin.
12	2	3	3·43		4·50	18	5·33	48	5·88	288	6	Infin.
24	4	6	6·86	16	9	36	10·66	96	11·76	576	12	Infin.
48	8	12	13·7	32	18	72	21·33	192	23·52	1152	24	Infin.

Taking the case where D is 12 inches, and uniting the two nearest openings where d is 0·5, let M N (Fig. 4, Pl. 17) be a section of the transparent pattern, L, R the left and right eyes, L a d, L b e lines drawn through the centres of two of the open figures a b, and R b d, R c e lines drawn through the centre of b and c, and meeting L a d, L b e at d and e, d being the binocular centre when we look at it through a and b, and e the binocular centre when we look at it through b and c. Now, the right eye R sees the opening b at d, and the left eye L sees the opening a at d, hence the image at d consists of the similar images of a and b united. In like manner e consists of b and c united, and so on with all the rest, so that the observer at L R no longer sees the real pattern M N, but a suspended image of it at m n, *three* inches behind M N. If the observer now approaches M N, the image m n will approach to him, and if he recedes, m n will recede, being $1\frac{1}{2}$ inches distant from M N when the observer is 6 inches from M N, and 12 inches from M N when he is 48 inches from M N, the image m n moving from M N with a velocity $\frac{1}{4}$th of that with which the observer recedes. These two velocities are in the ratio of D to $\frac{\text{D}\,d}{\text{C} - d}$.

Resuming the position in the figure where the observer is 12 inches distant from M N, let us consider the important results to which this experiment cannot fail to lead us. If the observer, with his eyes at L R, grasp the cane bottom or pattern at M N, as shewn in Fig. 4, pl. 17, his thumbs pressing upon M N, and his fingers trying to grasp m n, he will then *feel what he does not see*, and *see what he does not feel!* The real pattern is absolutely invisible at M N, and stands fixed at m n. The fingers may be passed through and through—now seen on this side of it—now in the middle of it, and now on the other side of it. If we next place the palms of each hand upon M N, feeling it all over, the result will be the same.

No knowledge derived from touch—no measurement of real distances—no actual demonstration from previous or subsequent vision, that there is a real solid body at M N, and nothing at all at *m n*, will remove or shake the infallible conviction of the sense of sight that the object is at *m n*, and that *d* L or *d* R is its real distance from the observer. If the binocular centre be now drawn back to M N, the image *seen* will disappear, and the real object be seen at M N. If it be brought still farther back to *f*, the object M N will again disappear, and will be seen at $\mu\,\nu$, as described in a former part of this paper.

In making these experiments, the observer cannot fail to be struck with the remarkable fact, that though the openings at M N, *m n*, and $\mu\,\nu$, have all the same angular magnitude, that is, subtend the same angle at the eye, viz., *d* L *e*, *d* R *e*, yet those at *m n* appear larger than those at M N, and those at $\mu\,\nu$ smaller. If we cause the image *m n* to recede, and $\mu\,\nu$ to approach, the figures in *m n* will invariably *increase* as they *recede*, and those in $\mu\,\nu$ will *diminish* as they *approach* the eye, and their *visual magnitudes*, as we shall call them, will depend on the respective distances at which the observer, whether right or wrong in his estimate, conceives them to be placed.

Now, this is an universal fact, which the preceding experiments demonstrate; and though the estimate of magnitude thus formed is an erroneous one, yet it is one which neither reason nor experience is able to correct.

When we look at two equal lines, whose difference of distance is distinctly appreciable by the eye, either directly, or by inference, but whose difference of angular magnitude is not appreciable, the most remote must necessarily appear the smallest. For the same reason, if the remoter of two lines is really smaller than the nearer, and, therefore, its angular magnitude also smaller from both these causes, yet, even in this case, if the eye does not perceive distinctly the difference, the smaller and more remote line will appear the larger.*

The law of visual magnitude, which regulates this class of phenomena, may be thus expressed.

If we call A the angular magnitude of the *nearest* of two lines or magnitudes

* MALEBRANCHE seems to have been the first who introduced the *apparent* distance of objects as an element in our estimate of *apparent* magnitude. *De la Recherche de la Verité*, tom. i. liv. i.; tom. iii. p. 354. See also Bouguer, *Mem. Acad. Par.* 1755, p. 99. These views, however, have been abandoned by several subsequent writers, and the *real distance* of objects has been substituted for their *apparent* distance. VARIGNON, *Mem. Acad. Par.* 1717, p. 88. M. LEHOT, for example, says, "L'expression de la grandeur visuelle d'un corps est egale à la grandeur reelle, multipliée par le logarithme de la *distance reelle* divisée par cette distance." *Nouvelle Théorie de la Vision*, 1[er] *Mem.* Suppl. p. 7, 8. Paris, 1823. This estimate of distance is incompatible with experiment and observation.

whose apparent distance is d, a the angular magnitude of the remoter line, whose apparent distance is D, and V, v the visual magnitudes of the two lines, then

$$V : v = A \times d : a \times D.$$

Now, let the two lines M O, N P, be the two sides of a quadrilateral figure seen obliquely by an eye at E, then, if the apparent distances of M O, N P, are such, that

$$A \times d > a \times D, \text{ then } V > v,$$

and the lines M N, O P, will converge to a vanishing point beyond N P. But if

$$A \times d = a \times D, \text{ then } V = v,$$

and the line M N, O P, will appear to be parallel. And if

$$A \times d < a \times D, \text{ then } V < v,$$

and the lines M N, O P, will converge to a vanishing point between M O and the observer.

These results may be considered as laying the foundation of a new art, to which we may give the name of VISUAL PERSPECTIVE, in contradistinction to GEOMETRICAL PERSPECTIVE. This art furnishes us with an immediate explanation of a great variety of optical illusions which have never yet been explained; and there is reason to believe that some of its principles were known to ancient architects, and even employed in modifying the nature and position of the lines and forms which enter into the construction of their finest edifices.

ST LEONARD'S COLLEGE, ST ANDREWS,
April 10. 1844.

APPENDIX.

APPENDIX.

When I wrote the paragraph in page 647, I had no expectation of learning that any example of such an illusion had ever occurred. A friend, however, to whom I had occasion to shew the experiments, and who is short-sighted, mentioned to me that he had been on two occasions greatly perplexed by the vision of these suspended images. Having taken too much wine, and being in a papered room, he saw the wall suspended near him in the air; and on another occasion, when kneeling and resting his arms on a cane-bottomed chair, he had fixed his eyes on the carpet, which accidentally united the two images of the open-work, and threw the suspended image of the chair bottom to a distance, and beyond the plane on which his arms rested.

The following case, communicated to me by Professor Christison, is still more interesting. "Some years ago, when I resided in a house where several rooms are papered with rather formally recurring patterns, and one, in particular, with stars only, I used occasionally to be much plagued with the wall suddenly standing out upon me, and waving, as you describe, with the movements of the head. I was sensible that the cause was an error as to the point of union of the visual axes of the two eyes; but I remember it sometimes cost me a considerable effort to rectify the error; and I found that the best way was to increase still more the deviation in the first instance. As this accident occurred most frequently while I was recovering from a severe attack of fever, I thought my near-sighted eyes were threatened with some new mischief; and this opinion was justified in finding that, after removal to my present house—where, however, the papers have no very formal pattern—no such occurrence has ever taken place. The reason is now easily understood from your researches."

PROCEEDINGS

OF THE

EXTRAORDINARY GENERAL MEETINGS,

AND

LIST OF MEMBERS ELECTED AT THE ORDINARY MEETINGS,

SINCE NOVEMBER 23. 1840.

PROCEEDINGS, &c.

Monday, November 23. 1840.

At a Statutory General Meeting, Sir T. M. BRISBANE in the Chair, the following Council was elected :—

Sir THOMAS M. BRISBANE, Bart., K.C.B., President.

Sir WILLIAM MILLER, Bart.,
Dr HOPE,
Sir D. BREWSTER, K.H.,
Rev. Dr CHALMERS,
Dr ABERCROMBIE,
} Vice-Presidents.

Professor FORBES, General Secretary.

Dr CHRISTISON,
Mr DAVID MILNE,
} Secretaries to the Ordinary Meetings.

Mr RUSSELL, Treasurer.

Dr TRAILL, Curator of Library and Instruments.

Mr STARK, Curator of Museum.

COUNSELLORS.

Mr THOMAS THOMSON.	Professor HENDERSON.
Mr J. T. GIBSON-CRAIG.	Professor KELLAND.
Dr GRAHAM.	Sir GEORGE WARRENDER, Bart.
Dr ALISON.	Sir JOHN ROBISON, K.H.
Sir H. JARDINE.	Sir JOHN M'NEILL, G.C.B.
JOHN SHANK MORE.	Professor SYME.

The following Committee was named to audit the Treasurer's Accounts :—

Sir HENRY JARDINE. GILBERT FINLAY, Esq.

DAVID SMITH, Esq.

On the motion of Lord GREENOCK, seconded by Dr HOPE, the thanks of the Society were conveyed by the Chair to Mr RUSSELL for his valuable services as Treasurer.

It was moved by the President, and carried by acclamation, That the Society view with extreme regret the Resignation by Sir JOHN ROBISON of the office of General Secretary to the Society, which he has now held for the period of thirteen years:

That they can never too highly appreciate the obligation under which they lie to him, for the zeal with which he has constantly discharged its duties, and for the great share he has had in promoting the prosperity of the Society during that long period; and that the cordial thanks and the regrets of the Society be communicated to him on this occasion.

The Treasurer then said, that, while he heartily concurred in this Resolution, he thought that some more substantial expression of the Society's sentiments than a vote of thanks should also be offered to Sir JOHN ROBISON, as a mark of the sense which the Society entertains of his valuable services; and he therefore moved, That the Council be requested to consider, and report to a Special General Meeting, the nature of a proper Testimonial to be presented to Sir JOHN, as a mark of their entire approbation of his conduct in the discharge of his duties as Secretary. Which motion, being seconded by Mr NAIRNE, was carried unanimously.

Lord GREENOCK moved that an Address should be presented to Her Majesty on the occasion of Her Majesty having recently given birth to a Princess. Which motion, being seconded by Mr JOHN SHANK MORE, was carried unanimously; and the Council were directed to prepare a suitable Address, and bring it before the next Ordinary Meeting of the Society, on the first Monday of December.

(Signed) GREENOCK, V. P.

Memorandum.—December 7. 1840.—At the Ordinary Meeting of this date, it was agreed, that the following Address to the QUEEN on the birth of the Princess Royal be transmitted through the President to the DUKE of SUSSEX, for presentation to her Majesty:—

TO THE QUEEN.

WE, the President and Fellows of the Royal Society of Edinburgh, beg leave to offer to your Majesty our unfeigned congratulations on an event which has filled the hearts of your Majesty's faithful and loving subjects with universal joy, and with a deep-felt sense of that gracious and overruling Providence under which your Majesty's safety, and the security and happiness of your realms, have hitherto been so signally protected. Impressed as we are with a conviction of the vast importance of whatever may tend to the stability of the hereditary throne of these kingdoms, and with a lively interest in whatever may be calculated to promote and secure your Majesty's happiness, and that of your Majesty's illustrious Consort, our earnest desires are for the permanence of the blessings thus auspiciously conceded to the prayers and hopes of a loyal and grateful people.

We are,

May it please your Majesty,

Your Majesty's most faithful and dutiful Subjects,

THE PRESIDENT AND FELLOWS OF THE ROYAL SOCIETY OF EDINBURGH.

THOMAS MAKDOUGAL BRISBANE, *P.*

JAMES D. FORBES, *Sec.*

December 7. 1840.

MEMBERS ELECTED.

ORDINARY.

James Anstruther, Esq.

January 4. 1841.

James Hunter, M.D.

Col. Morrison, C.B., Madras Artillery.

J. P. Muirhead, Esq.

January 18. 1841.

Honorary.

Professor Encke, Esq., Berlin.

Ordinary.

John Miller, Esq., Civil-Engineer.

February 1. 1841.

George Smyttan, M.D.

James Hamilton, Esq.

February 18. 1841.

Graham Speirs, Esq.

April 5. 1841.

Robert Spittal, M.D.

April 19. 1841.

James Dalmahoy, Esq.

December 6. 1841.

James Kinnear, Esq., W.S.

Memorandum.—December 21. 1841.—On the motion of the Council, referring to the Remit by the Society on the 23d November, it was resolved, "That the Society do vote the sum of L.300 to Sir John Robison, in acknowledgment of his long services as General Secretary,—that being the form adopted in the case of each of his predecessors."

November 22. 1841.

At a Statutory General Meeting, Lord Greenock in the Chair, the following Council was elected :—

Sir T. M. Brisbane, Bart., President.

Sir William Miller, Bart.,
Dr Hope,
Sir D. Brewster, K.H.,
Lord Greenock,
Dr Chalmers,
Dr Abercrombie,
Vice-Presidents.

Professor Forbes, General Secretary.
Dr Christison, } Secretaries to Ordinary Meetings.
D. Milne, Esq., }
John Russell, Esq., Treasurer.
Dr Traill, Curator of Library.
Mr Stark, Curator of Museum.

COUNSELLORS.

Sir H. Jardine.	Professor Syme.
J. S. More, Esq.	Mr A. Bell.
Professor Henderson.	Mr W. A. Cadell.
Professor Kelland.	Dr Maclagan.
Sir G. Warrender, Bart.	Mr James Wilson.
Sir J. Robison, K.H.	Bishop Terrot.

The Treasurer laid his Accounts on the Table. The following Committee was appointed to audit them :—

Sir H. Jardine. W. Paul, Esq. James Walker, Esq.

On the motion of Lord Greenock, the following Address to Her Majesty, prepared by the Council, was unanimously adopted by the Meeting, and desired to be transmitted to the Secretary of State through the President of the Society :—

TO THE QUEEN.

May it Please Your Majesty,

We, your Majesty's loyal and devoted subjects, the President and Fellows of the Royal Society of Edinburgh, beg leave to approach your Majesty with our heartfelt congratulations upon the auspicious event by which a merciful Providence has at once granted a happy completion to the tenderest desire of our Sovereign's maternal heart, and to the wishes and prayers of all her faithful and affectionate subjects, in the birth of an Heir-Apparent to the Throne on which your Majesty and your Royal Ancestors have so long ruled this great Empire. We acknowledge a happy omen for the perpetuity of that union of liberty and social order under which the power and happiness of these realms have grown up and flourished; and an additional reason to hope that, under the parental rule of your Majesty and your Royal Descendants, we and our children, and our children's children, shall continue to enjoy those national blessings which we gratefully acknowledge that we have received under your Majesty's mild and beneficent administration of the high powers committed to your hands.

That your Majesty may, through a long and prosperous reign, be supported by that Divine aid which has hitherto been so conspicuous in the happy events both of your public and private life; that you may long enjoy all the domestic happiness which can crown the wishes of a wife and a mother, together with the gratifying conviction of being the instrument of happiness, and the object of grateful and respectful affection, to a great and united nation;

that the Royal Infant, on whose birth we now congratulate your Majesty, may grow up to be worthy of the race from which he is sprung, and the destiny to which he is called; and that he may gratify your Majesty's most sanguine hopes, whether springing from the feelings of the parent or of the Queen, is the sincere prayer of

Your Majesty's faithful and devoted subjects,

THE PRESIDENT AND FELLOWS OF THE ROYAL SOCIETY OF EDINBURGH.

Signed in the name, and by appointment, of the Society,

THOMAS M. BRISBANE, *Pres.*
JAMES D. FORBES, *Secy.*

Sir GEORGE MACKENZIE gave notice, that, at a future Meeting of the Society, he should move that the office of Vice-President be no longer permanent, but that two Vice-Presidents shall retire annually.

MEMBER ELECTED.

ORDINARY.

January 3. 1842.

JAMES THOMSON, Esq., Civil Engineer.

Memorandum.—On the 7th February 1842, at an Extraordinary Meeting of the Society, after special notice of more than four weeks' standing, Sir G. S. MACKENZIE moved the following Resolution:—

That the first part of Law XVI. shall remain under that number; the remainder, together with Law XVII., shall be altered, and stand thus, under Law XVII.:—

"The Council shall consist of a President, Six Vice-Presidents, Twelve Counsellors, a General Secretary, Two Secretaries to the Ordinary Meetings, a Treasurer, a Curator or Curators of the Museum and Library. The President shall be elected annually, and may be re-elected. Of the Six Vice-Presidents, the three seniors may be re-elected annually, and their number filled up on vacancies occurring, by the choice of Members who have distinguished themselves for a lengthened period as eminent and active Fellows of the Society. One of the remaining three who shall be resident in Edinburgh shall go out of office annually by rotation, and not be eligible for one year. The other office-bearers to be elected annually. The Council shall conduct the Publications, and regulate the Private Business of the Society."

Sir HENRY JARDINE seconded the motion.

Dr TRAILL moved, as an amendment, That the office of Vice-President shall remain as at present; which was seconded by Mr W. A. CADELL.

The President having put the motion and amendment, the amendment was carried.

MEMBERS ELECTED.

ORDINARY.

February 22. 1842.

Dr JOHN DAVY, Insp. Gen. of Hospitals.
ROBERT NASMYTH, Esq., F.R.C.S.E.

March 7. 1842.

Sir JAMES FORREST, Bart.
JAMES STARK, M.D., F.R.C.Ph.E.
JAMES MILLER, Esq., Prof. of Surgery.
JOHN ADIE, Esq.

August 29. 1842.

At an Extraordinary General Meeting of the Society, called by advertisement in the Newspapers and by Circulars, in consequence of orders from the Council, Lord GREENOCK, as the Senior Vice-President present, took the Chair; and stated that the object of the Meeting was to consider the propriety of voting Addresses of Congratulation to Her MAJESTY and to PRINCE ALBERT, on the occasion of their expected arrival in Scotland, and also of electing PRINCE ALBERT an Honorary Fellow of the Society.

The Lord JUSTICE-GENERAL (BOYLE) then moved, and JOHN RUSSELL, Esq., seconded, the following Address to Her Majesty:—

UNTO HER MOST EXCELLENT MAJESTY THE QUEEN.

MAY IT PLEASE YOUR MAJESTY,

The Royal Society of Edinburgh, instituted for the promotion of Letters and Science, beg leave to join in the universal expression of joy, on your Majesty's happy arrival on the scene of their humble exertions.

They are aware that when all their fellow-subjects are eager to be permitted to offer their loyal congratulations, those of individual bodies should be briefly expressed. They now, therefore, beg leave to approach your Majesty, in order to offer this humble tribute of their devoted loyalty; and that it may please Almighty God to grant your Majesty many and happy years, and to repay to your Majesty the happiness which your Majesty's arrival has so widely diffused in Scotland, is the prayer of,

Your Majesty's faithful and devoted subjects,

THE PRESIDENT AND FELLOWS OF THE ROYAL SOCIETY OF EDINBURGH.

Signed at Edinburgh, on the 29th August 1842, in the name, and by the appointment, of the Society.

This Address was unanimously approved of by the Society, and the Vice-President in the Chair was appointed to sign it.

Thereafter, Dr ABERCROMBIE moved, and Sir CHARLES MENTEITH of Closeburn seconded, the following Address to PRINCE ALBERT:—

UNTO FIELD-MARSHAL HIS ROYAL HIGHNESS PRINCE ALBERT, K. G., &c. &c.

MAY IT PLEASE YOUR ROYAL HIGHNESS,

The Royal Society of Edinburgh beg leave to offer their respectful congratulations to your Royal Highness, on your arrival in this portion of the dominions of your Illustrious Consort, our beloved Queen; and to cherish a hope, that this visit, which has afforded so much joy to Her Majesty's Loyal Scottish Subjects, may tend to interest your Royal Highness in a country and people with whose welfare your Royal Highness is now connected by the most endearing ties.

That Almighty God may long preserve to your Royal Highness the enjoyment of your present domestic felicity, with many other blessings, is our fervent and united prayer.

Signed at Edinburgh, on the 29th August 1842, in the name, and by appointment, of the Society.

This Address was unanimously approved of, and the President in the Chair was appointed to sign it.

Thereafter, Dr Cook of St Andrews moved, That a Deputation be appointed to present the Addresses, and to take the proper steps for ascertaining at what time and in what manner they should be presented;—the Deputation to consist of such of the Office-bearers of the Society as may be selected by the President, after it has been ascertained whether there is any rule as to the number of persons who will be allowed to compose a Deputation for the presentation of Addresses.

This Motion was seconded by Mr Stark, and unanimously agreed to.

Thereafter, Sir John Robison stated, that it was the opinion of the Council, that Prince Albert, who was already a Fellow of the Royal Society of London, and who was in all respects worthy of being an Honorary Fellow of this Society, should have this honour now conferred on him. The Resolution of the Council to propose his Royal Highness to be elected an Honorary Fellow, had been notified to the Members.

This Motion, having been seconded by Dr Maclagan, was unanimously agreed to.

A Diploma in favour of Prince Albert was signed by the President in the Chair and by the Secretary.

The Deputation above named for the Addresses, was appointed also to present the Diploma to Prince Albert.

(Signed) John Abercrombie, *V. P.*

November 28. 1842.

At a Statutory General Meeting, Minutes of last Meeting read and approved; after which Mr Milne read the following Report:—

REPORT by Mr Milne, as to the Proceedings connected with the Presentation of the Addresses to the Queen and to Prince Albert, and of the Diploma to Prince Albert.

Mr Milne, with the view of carrying into execution the Resolutions of the General Meeting of the Society, held on the 29th August, addressed a Letter to Lord Aberdeen, as one of the Secretaries of State attending Her Majesty, and then staying at Dalkeith Palace, of which Letter the following is a Copy:—

My Lord, *Edinburgh*, 10 *York Place*, *August* 30. 1842.

As one of the Secretaries of the Royal Society of Edinburgh, I take the liberty of acquainting your Lordship, that, at a General Meeting of that body, held yesterday, an Address to the Queen was resolved on, and signed by the President in the Chair, congratulating Her Majesty on her arrival in Scotland.

I have farther to mention, that a Deputation of the Office-Bearers of the Society was appointed to present this Address to Her Majesty; and I will feel obliged by your Lordship informing me, when and where this Address may be received by Her Majesty.

I may add, that I have been informed that when KING GEORGE IV. visited Scotland, a similar Address from the Royal Society was received by His Majesty in the Royal Closet.

I have the honour to remain,

MY LORD,

Your very obedient servant,

(Signed) DAVID MILNE.

The Right Honourable
The EARL of ABERDEEN, &c. &c.

To the foregoing Letter the following answer was received by Mr MILNE.

SIR, *Dalkeith, September* 2. 1842.

Under the circumstances connected with the Queen's visit to Scotland, Her Majesty will not be enabled to grant any audiences in the Royal Closet, on the present occasion.

With respect to the presentation of the Address of the Royal Society of Edinburgh, I beg to acquaint you, that it may be presented at the General Reception; or if you will transmit it to me, I shall have much pleasure in laying it before Her MAJESTY.

I have the honour to be, Sir, your obedient servant,

(Signed) ABERDEEN.

DAVID MILNE, Esq.

Mr MILNE also addressed a Letter to the Hon. Colonel ANSON, Private Secretary to His Royal Highness PRINCE ALBERT, then staying at Dalkeith Palace.

SIR, *Edinburgh*, 10 *York Place*, *August* 30. 1842.

As one of the Secretaries of the Royal Society of Edinburgh, I have to acquaint you, that, at an Extraordinary General Meeting of that body, held in their hall yesterday, a loyal and dutiful Address, congratulating his Royal Highness PRINCE ALBERT on his arrival in Scotland, was resolved on. I now beg to enclose a copy of this Address.

A Deputation was appointed, of some of the Office-Bearers of the Society, to present this Address to His Royal Highness.

I have also to acquaint you, that the Royal Society, at the same Meeting, unanimously resolved on electing His Royal Highness an Honorary Fellow of the Society; and his Diploma was thereupon signed by the President in the Chair.

May I be permitted to inquire, whether it will be agreeable to His Royal Highness to receive the Deputation, for the purpose of presenting the Address, as well as the Diploma?

It has been usual for all Fellows of the Royal Society of Edinburgh, as of the Royal Society of London to enrol their names when any opportunity of doing so offers, in a book kept for that purpose; and, if agreeable to His Royal Highness, this book will be presented by the Deputation, in order that the Society may have the honour of possessing His Royal Highness' signature, in their book of Members. If His Royal Highness intends to visit the Institution in Princes

Street, where the Royal Society Apartments are, and where also various other Public Bodies, instituted for the study of the Arts, Sciences, and Antiquities, hold their Meetings, it may be more convenient for His Royal Highness to receive the deputation on that occasion. But wherever, and at whatever time, it may be most agreeable to His Royal Highness, to have the Address and the Diploma presented to him, the Deputation will attend, on your giving me a few hours' previous notice.

I have honour to remain, Sir, your most obedient servant,

The Honourable GEORGE ANSON, (Signed) DAVID MILNE.

Secy. to H. R. H. PRINCE ALBERT.

To this Letter, the following answer was received.

SIR, *Dalkeith House, September* 1. 1842.

I beg to acknowledge the receipt of your letter of the 30th ult., and, in reply, to inform you, that H. R. H. Prince Albert will be happy to receive the address from the Royal Society of Edinburgh, at Her Majesty's reception at Dalkeith House, on Monday next, at two o'clock.

I have it farther in command, to request you will convey to the Members of the Royal Society of Edinburgh, the assurance of H. R. Highness' gratification in finding that they have elected him an Honorary Fellow of their Society. If the Book of the Royal Society were transmitted to Mr ANSON, H. R. Highness would be happy to inscribe his name in the usual manner.

I am, Sir, your most obedient Servant,

DAVID MILNE, Esq. (Signed) G. E. ANSON.

Mr MILNE immediately on receiving the Letters from the Earl of ABERDEEN and Colonel ANSON just referred to, went to Lord GREENOCK, and having, on the 3d September, received from his Lordship a list of the office-bearers proposed by him to form a Deputation to present the Addresses and Diploma voted by the Society, notice was sent to Sir JOHN ROBISON, Dr ABERCROMBIE, Sir DAVID BREWSTER, and Dr HOPE, to request them to accompany Lord GREENOCK to the general reception at Dalkeith, to be held on the 5th September.

Mr MILNE, on the same occasion, delivered to Lord GREENOCK the Addresses and the Diploma.

In consequence of the difficulties and delays which occurred to prevent the possibility of any regularity being insured, with regard to arriving at Dalkeith Palace on the day of the reception in time for the regular assembling of the Deputation, previously to the presentation of the Address and Diploma, Lord GREENOCK, who was unable to reach the palace until near the close of the Drawing-Room, was under the necessity of presenting the Addresses by himself to Her MAJESTY and PRINCE ALBERT on his reaching the presence, which duty he duly performed by delivering the Address to the QUEEN, to Her Majesty's Lord in Waiting, and that with the Diploma for PRINCE ALBERT to Colonel ANSON, His Royal Highness' Secretary.

It had been intended, that the Society's Book of members should have been taken out to

the reception at Dalkeith, on the 5th September, in order that PRINCE ALBERT might there inscribe his name in the Book; but the Prince having, beetween 9 and 10 o'clock A.M. of that day, rode into Edinburgh, and visited the Society's Apartments, the Book was then presented to His Royal Highness by Mr JOHN RUSSELL and Sir JOHN ROBISON, when he inscribed his name in it.

As it may be desirable to preserve some account of the manner in which the Address of of the Society to His Majesty GEORGE IV. was presented when he visited Scotland, reference may here be made to a letter from Sir DAVID BREWSTER, then Secretary of the Society, to Mr RUSSELL, Treasurer, dated 29th August 1842.

St Leonard's College, St Andrews,
August 29. 1842.

DEAR SIR,

Sir WALTER SCOTT had arranged that the Address of the Royal Society should be presented to GEORGE IV. in the Closet by a Deputation; but it was found that this would have been troublesome to the KING, as several other bodies had the same title to this mode of presentation. The Address was therefore presented by the President; but whether along with any of the Vice-Presidents, I do not recollect. I remember, however, that I, as Secretary, did not accompany the President. Sir WALTER SCOTT was much annoyed at the change, and did what he could to prevent it.

I am, dear Sir, ever most truly yours,

JOHN RUSSELL, Esq. (Signed) D. BREWSTER.

Mr MILNE also received from Sir THOMAS D. LAUDER a Letter, dated 30th August, regarding the access to the Society's Apartments during Her Majesty's Procession, which he now produced.

He has only farther to report, that the Institution was, on the night of the 2d September, illuminated by a device which covered the entire front of the Building, and that the share of the expense which was paid by the Royal Society amounted to L.6, 3s.

The above Report having been heard, it was directed to be engrossed in the General Minute-Book.

The Meeting then proceeded to ballot for Office-Bearers for the year 1842–3, the Ballot-box having been examined by the President, the following gentlemen were found to be duly elected:—

COUNCIL FOR 1842–3.

Sir T. MAKDOUGALL BRISBANE, K.C.B., Bart., President.

Sir WILLIAM MILLER, Bart.,
Dr HOPE,
Sir DAVID BREWSTER, K.H.,
Lord GREENOCK,
Dr ABERCROMBIE,
Principal LEE,
} Vice Presidents.

Professor FORBES, General Secretary.

Dr CHRISTISON, and
D. MILNE, Esq., } Secretaries to Ordinary Meetings.

JOHN RUSSELL, Esq., Treasurer.

Dr TRAILL, Curator of Library.

J. STARK, Esq., Curator of Museum.

COUNSELLORS.

Sir GEORGE WARRENDER, Bart.
Sir JOHN ROBISON, K.H.
Professor SYME.
ARCHIBALD BELL, Esq.
W. A. CADELL, Esq.
Dr MACLAGAN.
JAMES WILSON, Esq.
Bishop TERROT.
Dr DAVY.
Dr PARNELL.
Dr CARSON.
Sir JOHN MACNEILL, G.C.B.

The following gentlemen were named to audit the Treasurer's Accounts :—

Sir HENRY JARDINE.
J. T. GIBSON-CRAIG, Esq.
WILLIAM PAUL, Esq.

MEMBERS ELECTED.

ORDINARY.

December 5. 1842.

JOHN GOODSIR, Esq., Edinburgh.

January 9. 1843.

A. D. MACLAGAN, M.D., F.R.C.S.E.

February 6. 1843.

JOHN ROSE CORMACK, M.D., F.R.C.Ph.E.
ALLEN THOMSON, M.D., Professor of the Institutes of Medicine.

February 21, 1843.

JOSEPH MITCHELL, Esq., Civil-Engineer, Inverness.
DUNCAN DAVIDSON, Esq. of Tulloch.

March 6. 1843.

ANDREW COVENTRY, Esq., Advocate.
D. BALFOUR, Esq., Younger of Trenaby.
JOHN HUGHES BENNETT, M.D., F.R.C.Ph.E., Edinburgh.

March 20. 1843.

HENRY STEPHENS, Esq., Edinburgh.

April 17. 1843.

W. H. Norie, Esq.

November 27. 1843.

At a Statutory General Meeting, held for the purpose of appointing Office-Bearers for the ensuing Session,—

Dr Abercrombie, V. P., in the Chair. The Ballot was taken in the usual way, and the Box having been examined, the following gentlemen were found to be duly elected :—

COUNCIL FOR 1843–4.

Sir T. Makdougal Brisbane, Bart., President.

Sir William Miller, Bart.,
Dr Hope,
Sir David Brewster, K.H.,
Right Honourable Earl Cathcart,
Dr Abercrombie,
The Very Rev. Principal Lee,
} Vice Presidents.

Professor Forbes, General Secretary.

David Milne, Esq.,
Professor Kelland,
} Secretaries to Ordinary Meetings.

John Russell, Esq., Treasurer.
Dr Traill, Curator of Library.
Mr Stark, Curator of Museum.

COUNSELLORS.

W. A. Cadell, Esq.
Dr Maclagan.
James Wilson, Esq.
Bishop Terrot,
Dr Parnell.
Dr Carson.
Sir John MacNeill, G.C.B.
Sir G. S. Mackenzie, Bart.
Sir Thomas Dick Lauder, Bart.
Alan Stevenson, Esq.
James T. Gibson-Craig, Esq.
Dr Craigie.

MEMBERS ELECTED.

ORDINARY.

December 18. 1843.

Arthur Forbes, Esq. of Culloden.
J. Burn Murdoch, Esq.

January 2. 1844.

Honourable Lord Murray.

February 5. 1844.

Lieutenant-Colonel John Low.

February 19. 1844.

Archibald Swinton, Esq., Professor of Civil Law.
James Begbie, M.D., F.R.C.S.E., Edinburgh.

March 18. 1844.

Nicholas Grut, Esq., Edinburgh.
Rev. Archibald Bennie, Edinburgh.
J. Y. Simpson, M.D., Professor of Midwifery.
David Stevenson, Esq., Civil-Engineer, Edinburgh.

April 15. 1844.

Thomas R. Colledge, M.D., F.R.C.Ph.E.

LIST OF THE PRESENT ORDINARY MEMBERS, IN THE ORDER OF THEIR ELECTION.

Major-Gen. Sir THOMAS M. BRISBANE, Bart., G.C.B., &c., F.R.S. Lond.,
PRESIDENT.

Date of Election.

Sir William Miller, Bart., Lord Glenlee.

The above Gentleman is the only surviving member of the Edinburgh Philosophical Society.

THE FOLLOWING MEMBERS WERE REGULARLY ELECTED.

1787 James Home, M.D., *Emeritus Professor of the Practice of Physic.*
1788 Right Honourable Charles Hope.
1798 Alexander Monro, M.D., *Professor of Anatomy, &c.*
1799 Sir George Stuart Mackenzie, Baronet, F.R.S. Lond.
Robert Jameson, Esq., *Professor of Natural History.*
1802 Colonel D. Robertson Macdonald.
1805 Thomas Thomson, M.D., F.R.S. Lond., *Professor of Chemistry, Glasgow.*
George Dunbar, Esq., *Professor of Greek.*
1807 John Campbell, Esq. *of Carbrook.*
Thomas Thomson, Esq., *Advocate.*
1808 James Wardrop, Esq.
Sir David Brewster, K.H., LL.D., F.R.S. Lond.
1811 Major-General Sir Thomas Makdougal Brisbane, B.T., G.C.B., G.C.H., F.R.S. Lond
John Thomson, M.D., *Emeritus Professor of General Pathology, Edinburgh.*
James Jardine, Esq., *Civil Engineer.*
Captain Basil Hall, R.N., F.R.S. Lond.
J. G. Children, Esq., F.R.S. Lond.
Alexander Gillespie, Esq., *Surgeon, Edinburgh.*
W. A. Cadell, Esq., F.R.S. Lond.
Macvey Napier, Esq., F.R.S. Lond., *Professor of Conveyancing.*
James Pillans, Esq., *Professor of Humanity.*
1812 Sir George Clerk, Bart., F.R.S. Lond.
1813 William Somerville, M.D., F.R.S. Lond.

Date of Election.

J. Henry Davidson, M.D., *Edinburgh.*

1814 Sir Henry Jardine.

Patrick Neill, LL.D., *Secretary to the Wernerian and Horticultural Societies.*

Right Honourable Lord Viscount Arbuthnot.

John Fleming, D.D., *Professor of Natural Philosophy, King's Coll., Aberdeen.*

Alexander Brunton, D.D., *Professor of Oriental Languages.*

Professor George Glennie, *Marischal College, Aberdeen.*

1815 Robert Stevenson, Esq., *Civil Engineer.*

Sir Thomas Dick Lauder, Bart. *of Fountainhall.*

Henry Home Drummond, Esq. *of Blair-Drummond.*

Sir Charles Granville Stuart Menteath, Bart. *of Closeburn.*

William Thomas Brande, Esq., F.R.S. Lond., and *Professor of Chemistry in the Royal Institution.*

1816 Colonel Thomas Colby, F.R.S. Lond., *Royal Engineers.*

Leonard Horner, Esq., F.R.S. Lond.

Henry Colebrooke, Esq., *Director of the Asiatic Society of Great Britain.*

George Cooke, D.D., *Professor of Moral Philosophy, St Andrews.*

Honourable Lord Fullerton.

Hugh Murray, Esq., *Edinburgh.*

1817 Right Honourable Earl of Wemyss and March.

John Wilson, Esq., *Professor of Moral Philosophy.*

Alexander Maconochie, Esq. *of Meadowbank.*

Sir David James Hamilton Dickson, M.D., *Clifton.*

William P. Alison, M.D., *Professor of the Practice of Physic.*

Robert Bald, Esq., *Civil Engineer.*

1818 Robert Richardson, M.D., *Harrowgate.*

Patrick Miller, M.D., *Exeter.*

John Watson, M.D.

Right Honourable John Hope, *Lord Justice-Clerk.*

Wiliam Ferguson, M.D., *Windsor.*

1819 His Grace the Duke of Argyll.

Patrick Murray, Esq. *of Simprim.*

James Muttlebury, M.D., *Bath.*

Thomas Stewart Traill, M.D., *Professor of Medical Jurisprudence*

Alexander Adie, Esq., *Edinburgh.*

William Couper, M.D, *Glasgow.*

Marshall Hall, M.D., *London.*

John Borthwick, Esq., *Advocate.*

Richard Phillips, Esq., F.R.S. Lond.

Reverend William Scoresby, *Exeter.*

George Forbes, Esq., *Edinburgh.*

1820 James Hunter, Esq. *of Thurston.*

Right Honourable David Boyle, *Lord Justice-General.*

James Keith, Esq., *Surgeon, Edinburgh.*

Date of Election.

1820 James Nairne, W.S., *Edinburgh.*,
Charles Babbage, Esq., F.R.S., Lond.
Thomas Guthrie Wright, Esq., *Auditor of the Court of Session.*
Sir John F. W. Herschel, Bart., F.R.S., Lond.
Adam Anderson, A.M., LL.D., *Prof. Nat. Phil. St Andrews.*
John Shank More, Esq., *Professor of Scots Law.*
Samuel Hibbert Ware, M.D.
Robert Haldane, D.D., *Principal of St Mary's College, St Andrews.*
Sir John Mead, M.D., *Weymouth.*
Dr William Macdonald, *Edinburgh.*
Sir John Hall, Bart. *of Dunglass.*
Sir George Ballingall, M.D., *Professor of Military Surgery.*

1821 Robert Graham, M.D., *Professor of Botany.*
Sir James M. Riddell, Bart. *of Ardnamurchan.*
Archibald Bell, Esq., *Advocate.*
John Clerk Maxwell, Esq., *Advocate.*
John Lizars, Esq., *Surgeon.*
John Cay, Esq., *Advocate.*
Robert Kaye Greville, LL.D., *Edinburgh.*
Robert Hamilton, M.D., *Edinburgh.*
Sir Archibald Campbell, Bart. *of Garscube.*
Sir David Milne, K.C.B.
A. R. Carson, Esq., LL.D., *Rector of the High School.*

1822 James Smith, Esq. *of Jordanhill*, F.R.S. Lond.
William Bonar, Esq., *Edinburgh.*
Captain J. D. Boswall, R.N., *of Wardie.*
George A. Walker-Arnott, Esq., *Advocate.*
Very Reverend John Lee, D.D., *Principal of the University.*
Sir James South, F.R.S., Lond.
Lieutenant-General Martin White, *Edinburgh.*
Walter Frederick Campbell, Esq. *of Shawfield*, M.P.
W. C. Trevelyan, Esq., *Wallington.*
Sir Robert Abercromby, Bart. *of Birkenbog.*
Dr Wallich, *Calcutta.*
The Right Honourable Sir George Warrender, Bart. *of Lochend.*
John Russell, Esq., W.S., *Edinburgh.*
John Dewar, Esq., *Advocate.*

1823 Sir Edward Ffrench Bromhead, Bart., A.M., F.R.S., Lond. *Thurlsby Hall.*
Captain Thomas David Stuart, *of the Hon. East India Company's Service.*
Andrew Fyfe, M.D., *Lecturer on Chemistry, Edinburgh.*
Robert Bell, Esq., *Advocate, Procurator for the Church of Scotland.*
Captain Norwich Duff, R.N.
Warren Hastings Anderson, Esq.
Alexander Thomson, Esq. *of Banchory, Advocate.*

Date of Election.

1823 Liscombe John Curtis, Esq., *Ingsdon House, Devonshire.*
Robert Knox, M.D., *Lecturer on Anatomy, Edinburgh.*
Robert Christison, M.D., *Professor of Materia Medica.*
John Gordon, Esq. *of Cairnbulg.*
1824 Dr Lawson Whalley, *Lancaster.*
William Bell, Esq., W.S., *Edinburgh.*
Alexander Wilson Philip, M.D., *London.*
Sir Charles Adam, R.N.
Robert E. Grant. M.D., *Professor of Comparative Anatomy, Univ. Coll., London.*
Claud Russell, Esq., *Accountant, Edinburgh.*
Rev. Dr William Muir, *one of the Ministers of Edinburgh.*
W. H. Playfair, Esq., *Architect, Edinburgh.*
John Argyle Robertson, Esq., *Surgeon, Edinburgh.*
James Pillans, Esq., *Edinburgh.*
James Walker, Esq., *Civil-Engineer.*
Sir William Newbigging, *Surgeon, Edinburgh.*
William Wood, Esq., *Surgeon, Edinburgh.*
1825 The Venerable Archdeacon John Williams, *Rector of the Edinburgh Academy.*
W. Preston Lauder, M.D., *London.*
Right Honourable Lord Ruthven.
Dr Reid Clanny, *Sunderland.*
Sir William Jardine, Bart. *of Applegarth.*
Hon. Lord Wood.
1826 Sir George Macpherson Grant, Bart. *of Ballindalloch.*
William Renny, Esq., W.S., *Solicitor of Stamps.*
Sir David Hunter Blair, Bart.
John Stark, Esq., *Edinburgh.*
Dr John Macwhirter, *Edinburgh.*
1827 John Gardiner Kinnear, Esq. *Edinburgh.*
William Burn, Esq., *Edinburgh.*
James Russell, M.D., *Edinburgh.*
Henry Thornton Maire Witham, Esq. *of Lartington.*
Rev. Dr Robert Gordon, *one of the Ministers of Edinburgh.*
James Wilson, Esq., *Edinburgh.*
Very Rev. Edward Bannerman Ramsay, A.M. *of St John's College, Cambridge.*
George Swinton, Esq., *Edinburgh.*
1828 Erskine Douglas Sandford, Esq., *Advocate.*
David Maclagan, M.D., *Edinburgh.*
Sir William Maxwell, Bart.
John Forster, Esq., *Architect, Liverpool.*
Thomas Graham, A.M., *Professor of Chemistry, London University.*
David Milne, Esq., *Advocate.*
Dr Manson, *Nottingham.*
William Burn Callender, Esq.

Date of Election.

1829 A. Colyar, Esq.
William Gibson-Craig, Esq., *Advocate.*
James Ewing, LL.D., *Glasgow.*
Sir Charles Ferguson, Bart., *Advocate.*
Right Hon. Duncan MacNeill, *Lord-Advocate.*
Rev. John Sinclair, A.M., *Pembroke College, Oxford.*
Arthur Connell, Esq., *Professor of Chemistry, St Andrews.*
Bindon Blood, Esq., M.R.I.A.
James Walker, Esq., W.S.
William Bald, Esq., M.R.I.A.
1830 J. T. Gibson-Craig, Esq., W.S.
Archibald Alison, Esq., *Advocate, Sheriff-Depute of Lanarkshire.*
Hon. Mountstuart Elphinstone.
James Syme, Esq., *Professor of Clinical Surgery.*
Thomas Brown, Esq., *of Lannfine.*
James L'Amy, Esq., *Advocate, Sheriff-Depute of Forfarshire.*
Thomas Barnes, M.D., *Carlisle.*
1831 James D. Forbes, Esq., F.R.S. Lond., *Professor of Natural Philosophy.*
Right Honourable Lord Dunfermline.
John Abercrombie, M.D., *Edinburgh, First Physician to her Majesty in Scotland.*
Donald Smith, Esq.
Captain Sir Samuel Brown, R.N.
O. Tyndal Bruce, Esq., *of Falkland.*
David Boswell Reid, M.D., *London.*
T. S. Davies, Esq., A.M., *Woolwich.*
1832 John Sligo, Esq. *of Carmyle.*
James Dunlop, Esq. *Astronomer, New South Wales.*
James F. W. Johnston, A.M., *Professor of Chemistry in the University of Durham.*
William Gregory, M.D., *Professor of Chemistry.*
Robert Allan, Esq., *Advocate.*
Robert Morrieson, Esq., *Hon. E.I.C. Civil Service.*
Montgomery Robertson, M.D.
1833 Captain Milne, R.N.
Alexander Earle Monteith, Esq., *Advocate.*
His Grace the Duke of Buccleuch.
A. T. J. Gwynne, Esq.
David Craigie, M.D., *Edinburgh.*
George Buchanan, Esq., *Civil-Engineer.*
Sir John Stuart Forbes, Bart. *of Pitsligo.*
Alexander Hamilton, Esq., LL.B., W.S.
Right Honourable Earl Cathcart.
1834 Mungo Ponton, Esq., W.S.
Isaac Wilson, M.D., F.R.S. Lond.
David Low, Esq. *Professor of Agriculture.*

Date of Election.

1834 Thomas Henderson, Esq., *Professor of Astronomy.*
Rev. Dr Chalmers, *Edinburgh.*
Alexander Kinnear, Esq.
Patrick Boyle Mure Macredie, Esq., *Advocate.*
John Davie Morries Stirling, Esq.
Thomas Jameson Torrie, Esq., *Edinburgh.*
William Copland, Esq. *of Colliston.*
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Date of Election.

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Robert Spittal, M.D., *Edinburgh.*
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Date of Election.

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ELECTED UNDER THE OLD LAWS.

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LIST OF HONORARY FELLOWS.

His Majesty the King of the Belgians.
His Imperial Highness the Archduke John of Austria.
His Royal Highness the Archduke Maximilian.
His Royal Highness Prince Albert.

BRITISH SUBJECTS (LIMITED TO TWENTY, BY LAW X.)

Robert Brown, Esq., F.R.S.
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Dr Faraday, F.R.S.
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Sir W. R. Hamilton, M.A., M.R.I.A., *Astronomer-Royal, Ireland.*

THE FOLLOWING EIGHT NAMES WERE INCLUDED WITH THE ABOVE PRIOR TO THE CHANGE IN THE LAW, 18TH JANUARY 1836.

Baron Humboldt,	*Berlin.*
M. Gay Lussac.	*Paris.*
M. Biot,	*Do.*
M. Arago,	*Do.*
Chevalier Hammer.	
M. Berzelius.	*Stockholm.*

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Le Baron Degerando,	*Paris.*
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M. Oersted,	*Copenhagen.*
M. Schumacher,	*Altona.*
Sir Henry Bernstein,	*Berlin.*

	Bishop Munter,	*Zealand.*
10	Baron Charles Dupin,	*Paris.*
	M. Brongniart,	*Do.*
	Chevalier Bürg,	*Vienna.*
	M. Bessel,	*Konigsberg.*
	M. Thenard,	*Paris.*
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	M. Mitscherlich,	*Berlin.*
	M. G. Rose,	*Berlin.*
	M. Hausmann,	*Göttingen.*
	J. J. Audubon, Esq.,	*United States.*
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	M. Agassiz,	*Neuchatel.*
	Le Baron Cousin,	*Paris.*
	M. Plana,	*Turin.*
25	M. Quetelet,	*Brussels.*
	M. Struve,	*Dorpat.*
	Professor Tiedemann,	*Heidelberg.*
	Professor Encke,	*Berlin.*

LIST OF FELLOWS DECEASED, RESIGNED, AND CANCELLED.

FROM 1840 TO 1844.

HONORARY FELLOWS.

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M. De Candolle, Geneva.
Right Honourable Lord Wallace.
Dr Dalton, F.R.S.
James Ivory, Esq., K.H., F.R.S.
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ORDINARY FELLOWS DECEASED OR RESIGNED.

Thomas Charles Hope, M.D., F.R.S. Lond., *Professor of Chemistry.*
William Wallace, L.L.D., *Emeritus Professor of Mathematics.*
Robert Ferguson, Esq. of Raith, F.R.S. Lond.
Sir Charles Bell, K.H., F.R.S. Lond., *Professor of Surgery.*
David Ritchie, D.D., *Emeritus Professor of Logic.*
Daniel Ellis, Esq., *Edinburgh.*
Sir John Robison, K.H., *Edinburgh.*
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Sir Francis Chantrey, F.R.S. Lond.
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Sir Francis Walker Drummond, Bart. *of Hawthornden.*
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Thomas Edington, Esq., F.G.S.
G. A. Stuart, Esq.
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RESIGNATIONS.

John Craig, Esq., *Edinburgh.*
Rev. Geo. Coventry, *Edinburgh.*
Gilbert Lawrie Finlay, Esq.
Graham Speirs, Esq.

ELECTIONS CANCELLED.

William Dyce, Esq., A.M.
Rev. J. P. Nichol, *Professor of Practical Astronomy, Glasgow.*

LIST OF DONATIONS.

(Continued from Vol. XIV. p. 731.)

December 7. 1840.

DONATIONS.	DONORS.
The American Journal of Science and Arts. Conducted by Benjamin Silliman, LL.D. For January, April, July, and October 1840.	The Editor.
Proceedings of the American Philosophical Society. January, February, May, June, July.	The Society.
On the Heat of Vapours and on Astronomical Refractions. By John William Lubbock, Esq.	The Author.
Researches in Embryology. (Second Series.) By Martin Barry, M.D., F.R.S.E.	The Author.
Transactions of the Cambridge Philosophical Society. Vol. vii. Part 1.	The Society.
Mémoires de la Société de Physique et d'Histoire Naturelle de Génève. Tome viii. Part 2.	Ditto.
Transactions of the Society instituted at London for the Encouragement of Arts, Manufactures, and Commerce. Vol. lii. Part 2.	Ditto.
The Quarterly Journal of Agriculture; and the Prize Essays and Transactions of the Highland and Agricultural Society of Scotland. For June, September, and December.	Ditto.
Journal of the Asiatic Society of Bengal. For June, July, August, September.	Ditto.
Asiatic Researches; or Transactions of the Society instituted in Bengal for inquiring into the History, the Antiquities, the Arts and Sciences, and Literature. Vol. xix. Part 2.	Ditto.
De Graphite Moravico et de phænomenis quibusdam originem Graphitæ illustrantibus Commentatio. E. F. De Glocker.	The Author.
Proceedings of the Geological Society of London. Nos. 67 to 71.	The Society.
Astronomische Nachrichten. Nos. 387 to 399.	Prof. Schumacher.
Memoirs of the Wernerian Natural History Society for the Years 1837–38. Vol. viii. Part 1.	The Society.
Journal of the Royal Asiatic Society. May 1840.	Ditto.
Collecçao de Noticias para a Historia e Geografia das Naçoes Ultramarinas que vivem nos dominios Portuguezes ou lhes sào visinhas; publicada pela Academia Real das Sciencias. Tomo v. No. 2.	The Royal Academy.
Astronomical Observations made at the Royal Observatory, Edinburgh. By Thomas Henderson, F.R.S.E., Professor of Practical Astronomy. Vol. iii.	The Royal Society, Lond.
Report on Education in Europe, to the Trustees of the Girard College for Orphans. By Alexander Dallas Bache, LL.D., President of the College.	The Author.
Tijdschrift voor Natuurlijke Geschiedenis en Physiologie. Uitgegeven door J. Van Der Hoeven, M.D., Prof. te Leiden, en W. H. De Vriese, M.D., Prof. te Amsterdam. Deel vi. St. 4. Deel vii. Stks. 1. 2.	The Editors.
Bulletin de l'Académie Royale des Sciences et des Belles Lettres de Bruxelles, 1840, Nos. 1 to 3.	The Academy.

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Brevi Cenni di Alcuni Resti delle Classi Brachiopodi di G. Michelotti.	The Author.
De Solariis in Supracretaceis Italiæ Stratis repertis. Auctore Joanne Michelotti.	Ditto.
Transactions of the Geological Society of London. (Second Series.) Vol. v. Part 3.	The Society.
The Rod and the Gun. Being Two Treatises on Angling and Shooting, by James Wilson, Esq., F.R.S.E., and by the Author of the "Oakleigh Shooting Code."	The Authors.
Madras Journal of Literature and Science. 1839. July, September, and December.	The Editors.
Oryctographie du Gouvernement de Moscow. Publiée par Gotthelf Fischer De Waldheim.	The Imperial Society of Naturalists of Moscow.
Einiges gegen den Vulkanismus. Von B. M. Keilhau.	The Author.
Notice sur les Gallas de Limmon. Par M. Jomard.	Ditto.
Notation Hypsométrique ou Nouvelle Manière de Noter les Altitudes. Par M. Jomard.	Ditto.
Quelques Recherches sur la Chaleur Spécifique. Par MM. les Professeurs De la Rive et Marcet.	Ditto.
Deuxième Mémoire sur les Variations Annuelles de la Température de la Terre à differentes profondeurs. Par A. Quetelet.	Ditto.
Second Mémoire sur le Magnétisme Terrestre en Italie. Par A. Quetelet.	Ditto.
Résumē des Observations Météorologiques faites en 1839, à l'Observatoire Royal de Bruxelles. Par A Quetelet.	Ditto.
Mémoires Couronnés par l'Académie Royale des Sciences et Belles Lettres de Bruxelles. Tome xiv. 1ere partie.	The Royal Academy.
Fisica de Corpi Ponderabili ossia Trattato della Constituzione Generale de' Corpi del Cavaliere Amedeo Avogadro. 2 vols. 8vo.	The Author.
Memorie della Reale Accademia delle Scienze di Torino. (Serie Seconda.) Tomo i.	The Academy.
Proceedings of the Linnean Society of London. Nov. 6. 1838 to March 17. 1840.	The Society.
Transactions of the Linnean Society of London. Vol. xviii. Part 3.	Ditto.
Ancient Laws and Institutes of England; comprising Laws enacted under the Anglo-Saxon Kings from Athelbirht to Cnut, with an English Translation of the Saxon; the Laws called Edward the Confessor; the Laws of William the Conqueror, and those ascribed to Henry the First; also Monumenta Ecclesiastica Anglicana, from the seventh to the tenth century; and the ancient Latin Version of the Anglo-Saxon Laws.	The Commissioners on the Public Records of the Kingdom.
Ordnance Survey of Ireland—County of Carlow in 28 sheets; and King's County in 49 sheets.	The Lord Lieutenant of Ireland.
Disquisizione delle Dottrine sulla Struttura e sulle Funzioni del cuore e delle Arterie. G. M. Zecchinelli.	The Author.
Footsteps of the Cheirotherium as shewn on Slabs of New Red Sandstone from Storeton Hill Quarry, Cheshire; illustrated in 3 plates.	Dr Buckland.
Elements of Chemistry. By the late Edward Turner, M.D. Enlarged and revised by Justus Leibig, M.D., Wilton G. Turner, and W. Gregory, M.D. Part 3. No. 2.	The Editors.
Transactions of the American Philosophical Society held at Philadelphia for promoting Useful Knowledge. (New Series.) Vol. vii. Part 1.	The Society.
Mémoires de la Société des Sciences Naturelles de Neuchatel. Tome ii.	Ditto.
Mémoires de l'Académie Impériale des Sciences de Saint Petersbourg. (Sciences Mathématiques, &c.) Tome ii. Livns 3, 4.	Ditto.

DONATIONS.	DONORS.
Mémoires de l'Académie Impériale des Sciences de Saint Petersbourg. (Sciences Politiques, &c.) Tome iv. Livns 4, 5.	
Do. Do. (Sciences Naturelles.) Tome iii. Livns 1, 2, 3, 4.	
Recueil des Actes des Séances Publiques de l'Académie Impériale des Sciences de Saint Petersbourgh, tenues le 29 Dec. 1838 et le 29 Dec. 1839.	The Imperial Academy.
Voyage dans la Russie Méridionale et la Crimée. Par M. de Demidoff. Livns 2, 3, des planches.	
Do. Do. (Partie Scientfique) Livns 6, 7, 8, 9, 10, en 8vo, et planches en folia.	The Author.
Comptes Rendus Hebdomadaires des Séances de l'Académie des Sciences de Paris 1840. 1er Sémestre No. 13, 14, 15, 16, 17, 18, 19.	The Academy.
Mémoire sur la Formation de l'Indigo dans les Feuilles du Polygonum Tinctorium, ou Renouée Tinctoriale. Par Ch. Morren.	The Author.
Recherches sur le Mouvement et l'Anatomie du Style du Goldfussia Anisophylla. Par M. Ch. Morven.	Ditto.
Twentieth Report of the Council of the Leeds Philosophical and Literary Society at the close of the Session 1839–40.	The Society.
Essays and Heads of Lectures on Anatomy, Physiology, Pathology, and Surgery. By the late Alexander Monro, Secundus, M.D., F.R.S.E., upwards of fifty years Professor of Anatomy and Surgery in the University of Edinburgh. With a Memoir of his Life by his Son and Successor.	Dr Monro.
Mr Greenough's Geological Map of England.	The Geological Society.
A Collection of Specimens of Fossil Organic Remains.	Chevalier Michelotti of Turin.

December 21.

Elements of Chemistry, including the actual State and prevalent Doctrines of that Science. By the late Edward Turner, M.D., F.R.S.L. and E. Edited by Justus Leibig, M.D., F.R.S.L. and E., and William Gregory, M.D., F.R.S.E.	The Publishers.
A Tabular View of the Yearly Quantity of Rain which falls in different parts of Great Britain. By Joseph Atkinson, Harraby, near Carlisle.	The Author.
Memoirs of the Royal Astronomical Society, Vol. xi.	The Society.
Astronomische Nachrichten. Nos. 400 to 411.	Prof. Schumacher.
A Collection of Fossil Fishes from Orkney.	Dr Traill.

January 4. 1841.

Bericht über die zur Bekanntmachung geeigneten Verhandlungen der Königl. Preuss. Akademie der Wissenschaften zu Berlin. Juli 1839 bis Juni 1840.	The Academy.
Abhandlungen der Königlichen Akademie der Wissenschaften zu Berlin. 1832. Thiels 3 and 4, and 1838.	Ditto.
Flora Batavia. No. 120.	King of Holland.
Annuaire Magnétique et Météorologique du Corps des Ingénieurs des Mines de Russie. Année 1838. Par A. F. Kupffer.	The Author.

January 18.

Journal of the Asiatic Society of Bengal. No. 97. 1840.	The Society.
Madras Journal of Literature and Science. January to March 1840.	Ditto.
Bulletin de la Société d'Encouragement pour l'Industrie Nationale pour les années 1838 et 1839.	Ditto.
Proceedings of the American Philosophical Society. No. 13.	Ditto.

DONATIONS.	DONORS.
Tijdschrift voor Natuurlijke Geschiedenis en Physiologie. Uitgegeven door J. Van der Hoeven, M.D. en W. H. de Vriese, M.D. Deel 7. Stks. 3, 4.	The Editors.
Experimental Researches on the Strength of Pillars of Cast Iron, and other Materials. By Eaton Hodgkinson, Esq.	The Author.
February 1.	
Researches in Embryology. 3d Series. By Martin Barry, M.D.	Ditto.
Quarterly Journal of the Statistical Society of London. Vol. iii. Part 4. January 1841.	The Society.
Flora Batava. No. 121.	King of Holland.
Voyage dans la Russie Méridionale et la Crimée. Par M. de Demidoff. (Partie Scientifique.) Livns 11 et 12, et planches.	The Author.
The American Almanac and Repository of Useful Knowledge for the year 1841.	American Philosophical Society.
February 15.	
Journal of the Asiatic Society of Bengal. 1840. No. 98.	The Society.
On the Study of Natural History as a Branch of General Education in Schools and Colleges. By Robert Patterson, Vice-President of the Natural History Society of Belfast.	Natural History Society of Belfast.
March 1.	
The Quarterly Journal of Agriculture, and the Prize-Essays and Transactions of the Highland and Agricultural Society of Scotland, for March 1841.	The Highland and Agricultural Society.
On the Constitution of the Resins. Parts 4 and 5. By James F. W. Johnston, Esq., A.M., F.R.S.	The Author.
March 15.	
Journal of the Asiatic Society of Bengal. No. 100. 1840.	The Society.
Mittlere Vertheilung der Wärme auf der Erdoberfläche, nebst Bemerkungen über die Bestimmung der mittleren Temperatur. Von Wilhelm Mahlmann.	The Author.
Memorie della Reale Accademia delle Scienze di Torino. (Serie Seconda.) Tomo ii.	The Academy.
Philosophical Transactions of the Royal Society of London for the year 1840. Parts 1, 2.	The Royal Society.
Proceedings of the Royal Society 1840. Nos. 41, 42, 43, 44, and 45.	Ditto.
Report of the Ninth Meeting of the British Association for the Advancement of Science, held at Birmingham in August 1839.	The British Association.
A Supplementary Report on Meteorology, presented to the Meeting of the British Association in 1840. By Professor Forbes.	The Author.
April 5.	
Mémoire de la Société Géologique de France. Tome iv. Ptie 1.	The Society.
The Transactions of the Royal Irish Academy. Vol. xix. Part 1.	The Academy.
Comptes Rendus Hebdomadaires des Séances de l'Académie des Sciences. 1841. Nos. 6, 7, 8, 9, 10.	Ditto.
Proceedings of the Geological Society of London. Nos. 74 and 75.	The Society.
The American Journal of Science and Arts. Conducted by Professor Silliman; for January 1841.	The Editor.
Etudes Géologiques dans les Alpes. Par M. L. A. Necker, Tome i.	The Author.
Maps of the Ordnance Survey of England and Wales. Nos. 75, 76, 79, and 82.	The Board of Ordnance.

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April 19.	
The Transactions of the Linnean Society of London. Vol. xvii. Parts 2, 3, 4; and Vol. xviii. Part 1.	
The Proceedings of the Linnean Society of London. Nos. 8 and 9.	The Society.
Transactions of the Society for the Encouragement of Arts, Manufactures, and Commerce. Vol. liii. Part 1.	Ditto.
Journal of the Asiatic Society of Bengal. Nos. 99, 101, and 102.	Ditto.
Travels in the Himalayan Provinces of Hindustan and the Punjab. By William Moorcroft and George Trebeck. Edited by H. H. Wilson, Esq. 2 vols. 8vo.	The Asiatic Society of Bengal.
Comptes Rendus Hebdomadaires des Séances de l'Académie des Sciences. Tome x. Nos. 19 to 26; Tome xi. and Tome xii. Nos. 1 to 5.	The Academy.
Mémoires de l'Académie Royale des Sciences de l'Institut de France. Tomes xiv. xv. xvi. et xvii.	
Mémoires présentés pars divers Savants à l'Académie Royale de l'Institut de France. Tome 5.	The Academy.
Voyage dans la Russie Méridionale et la Crimée. Par M. de Demidoff. (Partie Scientifique.) Livr^{ns} xiii. et xiv. des planches.	The Author.
Transactions of the Zoological Society of London. Vol. i. Part 3.	
Proceedings of the Zoological Society of London. Nos. 73 to 90.	The Society.
December 6. 1841.	
Astronomical Observations made at the Royal Observatory, Greenwich, in the years 1838 and 1839, under the direction of George Biddle Airy, Esq. 2 Vols.	The Royal Society of London.
Journal of the Royal Geographical Society of London. Vol. x. Part 3.	The Society.
Report of the Tenth Meeting of the British Association for the Advancement of Science, held at Glasgow in August 1840.	The British Association.
Bulletin de la Société Géologique de France. Tome x. Feuilles 24–9. Tome xi. et Tome xii. Feuilles 1–11.	The Society.
Proceedings of the Meteorological Society during the Sessions 1838–39 and 1839–40.	Ditto.
Quarterly Journal of the Statistical Society. Vol. iv. Part 1–2.	Ditto
Proceedings of the American Philosophical Society. Nos. 14, 15, 16, 17, 18.	Ditto.
Transactions of the American Philosophical Society, held at Philadelphia, for Promoting Useful Knowledge. New Series. Vol. vii. Parts 2, 3.	Ditto.
Flora Batava. No. 122.	The King of Holland
Produzioni relative al Programma di tre quistioni Geometriche proposto da un nostro professore.	The Author.
Problema Fondamentale per le polari Coniche Reciproche Geometricamente Risoluto da Nicoli Trudi.	Ditto.
Boston Journal of Natural History, containing Papers and Communications read to the Boston Society of Natural History, and published by their direction. Vols. i. ii., and vol. iii. Parts 1, 2, 3.	The Boston Society of Natural History.
The Quarterly Journal of Agritulture; and the Prize Essays and Transactions of the Highland and Agricultural Society of Scotland. Nos. 53, 54, and 55.	The Highland and Agricultural Society.
The Americal Journal of Science and Arts, conducted by Professor Silliman and Benjamin Silliman, Jun. For April, July, and October 1841.	The Editors.

DONATIONS.	DONORS.
Lectures on Agricultural Chemistry and Geology. By James F. W. Johnston, Nos. 1 to 11.	The Author.
Mémoire sur la Chaleur Solaire, sur les Pouvoirs Rayonnants et Absorbants de l'air atmospherique, et sur la Temperature de l'Espace. Par M. Pouillet.	Ditto
Bulletin de la Société d'Encouragement pour l'Industrie Nationale pour 1840.	The Society.
The Article on the Silurian System, from the Edinburgh Review for April 1841. By W. H. Fitton, Esq.	The Author.
Catalogue de l'Ecole des Vignes de la Pépinière du Luxembourg.	Le Duc Decaze.
Commentatio de usu Experientiarum Metallurgicarum ad disquisitiones Geologicas adjuvandas. Auctore J. F. L. Hausmann.	The Author.
Illustrations of the Affinity of the Latin Language to the Gaelic or Celtic of Scotland. By T. Stratton, Esq.	Ditto.
Madras Journal of Literature and Science. April to Septemher 1840.	The Madras Literary Society.
Mémoires de la Société Géologique de France. Tome iii., and Tome iv. Première Partie.	The Society.
Proceedings of the London Electrical Society. Session 1841–42. Nos. 1, 2.	
The Transactions and the Proceedings of the London Electrical Society. Vol. i.	Ditto.
Tijdschrift voor Natuurlijke Geschiedenis en Physiologie. Uitgegeven door J. Van Der Hoeven, M.D., en W. H. De Vriese, M.D. Deel viii. St. 2, 3.	The Editors.
Comptes Rendus Hebdomadaires des Séances de l'Académie des Sciences. Tome xii. Nos. 25, 26, et Tome xiii. Nos. 1–18.	The Academy.
Traité Elémentaire des Fonctions Elliptiques. Par P. F. Verhulst.	The Author.
Analyse Raisonnée des Travaux de Georges Cuvier, précédée de son Eloge. Par P. Flourens.	Ditto.
Des Moyens de soustraire l'Exploitation des Mines de Houille aux chances d'explosion. Recueil de Mémoires et de Rapports publié par l'Académie Royale des Sciences et Belles Lettres de Bruxelles.	
Annuaire de l'Observatoire Royal de Bruxelles, pour l'an 1841. Par le Directeur, A. Quetelet.	
Annuaire de l'Académie Royale des Sciences et Belles Lettres de Bruxelles. 1841.	
Bulletin de l'Académie Royale de Bruxelles. Tome vii. Nos. 9, 10, 11, 12. Tome viii. Nos. 1–6.	
Nouveaux Mémoires de l'Académie Royale des Sciences et Belles Lettres de Bruxelles. Tome xiii.	
Mémoires Couronnés par l'Académie Royale des Sciences et Belles Lettres de Bruxelles. Tome xiv.	The Academy.
Annuaire Magnétique et Météorologique du Corps des Ingénieurs des Mines de Russie pour l'année 1839. Par A. T. Kupffer.	The Author.
The Eighth Annual Report of the Royal Cornwall Polytechnic Society. 1840.	The Society.
Journal of the Asiatic Society of Bengal. Nos. 105, 106, 107, 108, 109, 110, 111.	Ditto.
Proceedings of the Geological Society of London. No. 76.	
Transactions of the Geological Society of London. New Series. Vol. vi. Part 1.	Ditto.
Bulletin de la Société de Géographie. Deuxieme Serie. Tomes 13, 14, 15.	Ditto.
Recueil de Voyages et de Mémoires publié par la Société de Géographie. Tome vi.	Ditto.
Natuurkundige Verhandelingen van de Hollandsche Maatschappij der Wetenschappen te Haarlem. (Second Series.) Deel 1.	Ditto.
The Oily Acids, forming the first Supplement to the Seventh Edition of Dr Turner's Chemistry. By Justus Liebig, M.D., and William Gregory, M.D.	The Editors.

DONATIONS.	DONORS.
Annuaire du Journal des Mines de Russie, pour les Années 1835, 36, 37, et 38, et Introduction. 5 Tomes.	General Tchefskine.
Transactions of the Botanical Society of Edinburgh. Vol. i. Parts 1, 2.	
Fourth and Fifth Annual Reports and Proceedings of the Botanical Society of Edinburgh.	The Society.
Eighteenth Report of the Whitby Literary and Philosophical Society, presented at the Annual Meeting, November 4. 1840.	Ditto.
Dictionarium Anamitico-Latinum, primitus inceptum ab illustrissimo et Reverendissimo P. J. Pigneaux, Vicario Apostolico Cocincinæ, et dein absolutum et editum a J. L. Taberd, Episcopo Isauropolitano, &c.	The Editor.
Dictionarium Latino-Anamiticum, auctore J. L. Taberd, Episcopo Isauropolitano, &c.	The Author.
Abstract of the Magnetic Observations made at the Trevandrum Observatory, during the month of May 1841. By John Caldecott, Esq., Director.	Ditto.
Museo Numismatico Lavy appartenente alla R. Accademia delle Scienze di Torino. Parts 1, 2.	Chevalier P. Lavy.
Descriptive Account of the Antiquities and Coins of Affghanistan. By. H. H. Wilson.	The Honourable the Directors of the E. I. C.
Archives de l'Electricité. Par N. A. de la Rive. No. 1.	The Author.
Det Kongelige Danske Videnskabernes Selskabs Naturvidenskabelige og Mathematiske Afhandlinger. 8 Vols.	The Academy.
An Abridgement of the Acts of the Parliament of Scotland from 1424 to 1707. By William Alexander, Esq., W.S., F.R.S.E.	The Author.
Transactions of the Philosophical Society of Cambridge. Vol. vii. Part 2.	The Society.
Annals of the Lyceum of Natural History of New York. Vols. i. ii. iii. iv. Parts 1, 2, 3, and 4.	The Directors of the Lyceum.
Voyage dans la Russie Méridionale et la Crimée. Par M. Anatole de Demidoff. Planches. Liv^{ns} 6, 7.	The Author.
Commentationes Societatis Regiæ Scientiarum Gottingensis Recentiores. Vols. 7 and 8.	The Society.
Reports presented to the Legislature of the Commonwealth of Massachusetts on Wheat and Silk, Invertebrate Animals, Herbaceous Plants and Quadrupeds.	The Bowditch Family.
Æsop's Fables, written in Chinese by the learned Mun Mooy Seen-Shang. Translated by Robert Thom, Esq.	The Translator.
Ancient Laws and Institutes of Wales.	The Commissioners on Public Records.
Novorum Actorum Academiæ Cæsareæ Leopoldino-Carolinæ Naturæ Curiosorum, Vol. 18. Supplement.	The Academy.
Monografia de genere Murex ossia enumerazione delle principali specie. Per Giov. Michelotti.	The Author.
List of the Instruments and Apparatus belonging to the Royal Society.	Royal Society.
List of the Portraits in possession of the Royal Society.	Ditto.
Report of the Committee of Physics, including Meteorology, on the objects of Scientific Inquiry in those Sciences.	Ditto.
Catalogue of the Scientific Books in the Library of the Royal Society.	Ditto.
Catalogues of the Miscellaneous Manuscripts, and of the Manuscript Letters in the possession of the Royal Society.	Ditto.
Statutes of the Royal Society. 1840.	Ditto.
Proceedings of the Royal Society of London. Nos. 46, 47, 48.	Ditto.

DONATIONS.	DONORS.
Philosophical Transactions of the Royal Society of London for 1841. Part 1.	
Astronomical Observations made at the Royal Observatory, Edinburgh. Vol. 4. By Thomas Henderson, F.R.SS.L. & E., &c.	The Royal Society of London.
Rara Mathematica; or, a Collection of Treatises on the Mathematics and Subjects connected with them. Edited by J. O. Halliwell.	The Editor.
Mémoire sur differens Procédés d'Intégration. Par J. Plana, à Turin.	The Author.
Abhandlungen der Königlichen Akademie der Wissenschaften zu Berlin. 1839.	
Bericht über die zur Bekanntmachung geeigneten Verhandlungen der Konigl. Preuss. Akademie der Wissenschaften zu Berlin, Juli 1840 bis Juni 1841.	The Academy.
The American Almanac and Repository of Useful Knowledge for 1841.	The Phil. Society of America.
Proceedings of the Zoological Society. Oct 13. 1840 to July 27. 1841.	The Society.
Letter-Press to the First Part of the Natural History and illustrations of the Scottish Salmonidæ. By Sir William Jardine, Bart.	The Author.
Ordnance Survey of Ireland. County Galway.	His Excellency the Lord Lieutenant.

December 20.

Comptes Rendus Hebdomadaires des Séances de l'Académie des Sciences. Tome xiii. Nos. 19, 20, 21.	The Academy.
Verhandelingen over de Natuurlijke Geschiedenis der Nederlandsche Overzeesche Bezittingen, door de Leden der Natuurkundige Commissie in Oost Indie en Andere Schrijvers. Afleverings 1, 2, 3.	
Nouveaux Mémoires de l'Académie Royale des Sciences et Belles Lettres de Bruxelles. Tome xiv.	Ditto.
Mémoires Couronnés par l'Académie Royale des Sciences et Belles Lettres de Bruxelles. Tome xv. P$^{\text{tie.}}$ 1.	Ditto.
Astronomische Beobachtungen auf der Königlichen Universitäts Sternwarte in Königsberg. Von F. W. Bessel. 20$^{\text{te}}$ Abtheilung.	The Author.
Proceedings of the Royal Society. No. 49.	
Catalogue of Miscellaneous Literature in the Library of the Royal Society. 8vo.	The Society.
An Account of the Vegetation of the Outer Hebrides. By J. H. Balfour, M.D., F.R.S.E.	The Author.
Descriptive and Illustrated Catalogue of the Physiological Series of Comparative Anatomy contained in the Museum of the Royal College of Surgeons in London. Vols. iv, v. 4to.	The Royal College of Surgeons.

January 3. 1842.

Mémoires de l'Académie Imperiale des Sciences de Saint Petersbourg. (Sciences Mathematiques et Physiques.) Tome ii. Liv$^{\text{ns}}$ 5, 6.	
Ditto. ditto. (Sciences Naturelles.) Tome iii. Liv$^{\text{ns}}$ 5, 6, et Tome iv. Liv$^{\text{ns}}$ 1, 5.	
Ditto. ditto. (Sciences Politiques, Histoire, Philologie.) Tome v. Liv$^{\text{ns}}$ 1, 4.	
Ditto. ditto. (Par divers Savans, et lus dans ses Assemblées.) Tome iv. Liv$^{\text{ns}}$ 3, 4.	
Recueil des Actes da la Séance Publique de l'Académie Impériale des Sciences de Saint Petersbourg, tenue le 29 Dec. 1840.	The Imperial Academy.
Bullétin de la Société Impériale des Naturalistes de Moscow, 1840. Nos. 1, 4, et 1841. No. 1.	The Society.
Ueber den Galvanismus als chemisches Heilmittel gegen örtliche Krankheiten, von Dr Gustav Crusell.	The Author

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January 17.	
Ordnance Survey of the County of Wexford in Ireland. 56 sheets.	The Lord Lieutenant.
Proceedings of the Linnean Society of London. Nos. 10, 11, and 12.	
Transactions of the Linnean Society of London. Vol. xviii. part 4.	The Society.
February 7.	
Proceedings of the American Philosophical Society. Vol. ii. No. 19.	Ditto.
Laws, Regulations, and Annual Report of the Leeds Philosophical Society for 1840–41.	Ditto.
Journal of the Asiatic Society of Bengal, 1841. Nos. 112, 113, and 114.	Ditto.
Ordnance Survey Maps of England and Wales. Nos. 77, 78, and 87.	Master-General of the Ordnance.
Karten der Isothermen-Curven auf der Nordl. Hemisphære. Von Wilh. Mahlmann.	The Author.
Comptes Rendus Hebdomadaires des Séances de l'Académie des Sciences. Tome xiii. Nos. 22, 26. Tome xiv. Nos. 1, 2, 3.	The Academy.
February 22.	
Twelfth Report of the Scarborough Philosophical Society. 1841.	The Society.
Comptes Rendus Hebdomadaires des Séances de l'Académie des Sciences. Tome xiv. Nos. 4, 5, 6, 7.	The Academy.
Report of the Commissioners appointed to consider the steps to be taken for Restoration of the Standards of Weight and Measure.	The Commissioners.
Letter to the Right Honourable George Earl of Aberdeen, on the State of the Schools of Chemistry in the United Kingdom. By W. Gregory, M.D., F.R.S.E., &c.	The Author.
A faithful Record of the Miraculous Case of Mary Jobson. By W. Reid Clanny, M.D., F.R.S.E.	Ditto.
Bien-Etre et Concorde des Classes du Peuple Français. Par Le Baron Charles Dupin.	Ditto.
Vie d'un Bienfaiteur du Peuplo, A. P. de la Rochefoucauld. Par Le Baron Charles Dupin.	Ditto.
March 7.	
Lexicon Syriacum Chrestomathiæ Kirschianæ denuo editæ accommodatum a Georgio Henrico Bernstein. Part 2.	Ditto.
Madras Journal of Literature and Science. Oct. and Dec. 1840.	The Madras Lit. Society.
Letter to the Right Honourable the Chancellor of the Exchequer from J. E. D. Bethune, Esq. on Weights and Measures.	The Author.
Address delivered at the Anniversary Meeting of the Geological Society of London, on the 19th of February 1841; and the announcement of the award of the Wollaston Medal and Donation Fund for the same year. By the Rev. Professor Buckland, D.D. &c.	Ditto.
The American Journal of Science and Arts, conducted by Professor Silliman, for January 1842.	The Editor.
Mémoires de la Société Géologique de France. Tome 4, ptie. 2.	The Society.
A Lecture on the Employment of the Microscope in Medical Studies. By John Hughes Bennett, M.D.	The Author.
Notes sur le Développement de Nerfs Particuliers à la Surface du Cervelet. Par le Docteur Bennett.	Ditto.

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April 4.	
Annuaire de l'Observatoire Royal de Bruxelles. Par A. Quetelet, Directeur de cet Etablissement. 1842.	The Author.
Nouveaux Catalogue des Principales Apparitions d'Etoiles Filantes. Par A. Quetelet.	Ditto.
Annuaire de l'Académie Royale des Sciences et Belles Lettres de Bruxelles. Pour l'Ann. 1842.	Ditto.
Bulletin de l'Académie Royale des Sciences et Belles Lettres de Bruxelles. 1841, Nos. 7-12; et 1842, Nos. 1, 2.	The Academy.
Magnetische und Meteorologische Beobachtungen zu Prag. Herausgegeben von Karl Kriel. Erster Jahrgang.	The Author.
Philosophical Transactions of the Royal Society of London, for the year 1841. Part 2.	The Society.
Examination Papers of the University of London for 1841.	The University.
Plausible Reasons and Positive Proofs, shewing that no portion of the Devonian System can be of the age of the Old Redstone. By the Rev. D. Williams, A.M., F.G.S.	The Author.
The Reminiscences of an Old Traveller throughout different parts of Europe. By Thomas Brown, Esq.	Ditto.
April 18.	
Proceedings of the Royal Society. No. 52.	The Society.
Theoretical Investigations on the Motions of Glaciers. By W. Hopkins, F.R.S.	The Author.
Comptes Rendus Hebdomadaires des Séances de l'Académie des Sciences. Tome xiv. Nos. 8-12.	The Academy.
Journal of the Asiatic Society of Bengal. 1841. No. 116.	The Society.
Elements of Agricultural Chemistry and Geology. By James F. W. Johnston, F.R.S.	The Author.
December 5.	
Report of the Eleventh Meeting of the British Association for the Advancement of Science, held at Plymouth in July 1841.	The Association.
Nieuwe Verhandelingen van het Bataafsch Genootschap der proefondervindelijke Wijsbegeerte te Rotterdam. Vol. viii. St. 2.	The Society.
Archives du Museum d'Histoire Naturelle Publiées par les Professeurs-Administrateurs de cet Etablissement. Tom. i. Livr^{ns} 2, 3, 4, et Tome ii. Livr^{ns} 1, 2.	The Editors.
A new Analogy for determining the Distances of the Planets from the Sun, and of the Satellites from their Primaries.	The Author.
The American Journal of Science and Arts, conducted by Professor Silliman, for April July, and October.	The Editor.
Scheikundige Onderzoekingen, gedaan in het Laboratorium der Utretchtsche Hoogeschool. Stuks 1, 2, 3.	The Editors.
Proceedings of the London Electrical Society. Session 1841-42. Parts 3, 4, 5.	The Society.
Voyage dans la Russie Méridionale et la Crimée. Planches, Livr^{n} viii. Par M. Anatole Demidoff.	The Author.
Astronomische Nachrichten. Nos. 433-446.	Prof. Schumacher.
Observations Météorologiques faites a Nijne-Taguilsk (Monts Oural), Governement de Perm. 1841.	The Author.
Novorum Actorum Academiæ Caesareæ Leopoldino-Carolinæ Naturæ Curiosorum Voluminis Undevicesimi Supplementum Alterum.	The Academy.

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The Quarterly Journal of Agriculture, and the Prize Essays and Transactions of the Highland and Agricultural Society of Scotland. June, September, and December.	The Society.
Journal of the Statistical Society of London. Vol. v., Parts 1, 2, 3.	Ditto.
An Exposition of the Nature, Force, Action, and other Properties of Gravitation on the Planets.	The Author.
Tijdschrift voor Natuurlijke Geschiedenis en Physiologie. Uitgegeven door J. Vander Hoeven, M.D., en W. H. Vriese, M.D. Deel viii. St. 4; and Deel ix. St. 1.	The Editors.
The Transactions of the Linnean Society of London. Vol. xix., Part. 1.	
The Proceedings of the Linnean Society of London. Nos. 13, 14.	The Society.
Ordnance Survey of the County of Kilkenny in Ireland. 49 sheets.	Lord-Lieutenant of Ireland.
Bulletin de la Société Impériale des Naturalistes de Moscow. 1842. Nos. 1, 2.	The Society.
Memoirs of the Literary and Philosophical Society of Manchester. (Second Series), Vol. vi.	Ditto.
Journal of the Asiatic Society of Bengal. Nos. 115, 118, 119, 120, and 121.	Ditto.
Six Lectures on Arithmetic, containing a Familiar Explanation of the Principles and Rationale of the General Rules of Arithmetic.	The Author.
Transactions of the American Philosophical Society held at Philadelphia, for promoting useful knowledge. Vol. viii., Part 1.	
Proceedings of the American Philosophical Society. Nos. 20, 21, and 22.	The Society.
The Ninth Annual Report of the Royal Cornwall Polytechnic Society. 1841.	Ditto.
Ueber das Magnetische Observatorium der Königlich-Sternwarte bei Munchen, von Dr J. Lamont.	The Author.
Die Galvanographie, eine methode, gemalte Tuschbilder durch galvanische Kupferplatten im Drucke zu Vervielfältigen, von Franz von Kobell.	Ditto.
Proceedings of the Royal Society. No. 53.	
Philosophical Transactions of the Royal Society of London for the year 1842. Part 1.	Royal Society.
Astronomical, Magnetical, and Meteorological Observations made at the Royal Observatory, Greenwich, in the year 1840, under the direction of George Biddell Airy, Esq., Astronomer-Royal.	Ditto.
Proceedings of the Geological Society of London. Nos. 77 to 87.	The Society.
Teoremi sulle Sezioni Coniche dimonstrati da Nicola Trudi. Nos. 5, 6, 7.	Ditto.
Seconde Mémoire sur les Kaolins ou Argiles a Porcelaine. Par MM. Alexandre Brongniart et Malaguti.	The Authors.
Forty-ninth Report of the Literary and Philosophical Society of Newcastle-upon-Tyne.	The Society.
Flora Batava. Nos. 123 and 124.	King of Holland.
Notice respecting the Fossils of the Mountain Limestone of Ireland, as compared with those of Great Britain, and also with the Devonian System. By Richard Griffith, F.R.S.E., &c. &c.	The Author.
Mémoires de l'Académie des Sciences de l'Institut de France. Tome xviii.	
Mémoires Présentés par divers Savants à l'Académie Royale des Sciences de l'Institut de France. Tome vii.	Royal Academy.
Ueber die Abhaengigkeit der Physischen Populationskraefte von den einfachsten Grundstoffen der Natur, mit specieller Anwendung auf die Bevolkerungs-Statistik von Belgien, von Dr Ferdinand Gobbi.	The Author.

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Mémoires de la Société de Physique et d'Histoire Naturelle de Génève. Tome ix. p^{tie} 2.	The Society.
Journal of the Royal Asiatic Society of Great Britain and Ireland. No. 13.	Ditto.
Magnetische und Meteorologische Beobachtungen zu Prag, von Karl Kriel. (Zwieter Jahrgang.)	The Author.
1st and 2d Bulletins of the Proceedings of the National Institution for the promotion of Science at Washington.	The Institution.
Researches in Physical Geology. By W. Hopkins, Esq. Parts 1, 2.	The Author.
On the Errors of Chronometers, and Explanation of a New Construction of the Compensation Balance. By E. J. Dent, Esq.	Ditto.
Twenty-second Report of the Council of the Leeds Philosophical and Literary Society, 1841-42.	The Society.
Transactions of the Royal Cornwall Polytechnic Society, 1842. No. 1.	Ditto
System der Krystalle, ein Versuch von M. L. Frankenheim, Professor an der Universitat von Breslau.	The Author.
Abhandlungen der Königlichen Akademie der Wissenschaften zu Berlin 1840.	
Bericht über die zur Bekanntmachung geeigneten Verhandlungen der Kongl. Preuss. Akademie der Wissenschaften zu Berlin. Juli 1841 bis Juni 1842.	The Academy. The Academy.
Liber Cartarum Prioratus Sancti Andree in Scotia e Registro ipso in Archivis Baronum de Panmure hodie asservato. (Printed for the Bannatyne Club.)	O. T. Bruce, Esq.
The Grasses of Scotland. By Richard Parnell, M.D., F.R.S.E.	The Author.
The Second Supplement, completing the Seventh Edition of Dr Turner's Chemistry. By Justus Liebig, M.D., and William Gregory, M.D.	The Editors.
Transactions of the Society instituted at London for the Encouragement of Arts, Manufactures, and Commerce. Vol. lii., Part 2.	The Society.
Journal of the Royal Geographical Society of London. Vol. ix., Part 2.	Ditto.
Abhandlungen der Mathematisch-Physikalischen Classe der Koeniglich Bayerischen Akademie der Wissenschaften. Band iii., Abth. ii.	The Academy.
Prodromus zu einer neuen verbesserten Darstellungsweise der Hohern Analytischen Dynamik, vom Grafen Georg von Buquoy. 1 Lieferung.	The Author. Ditto.
Proceedings of the Zoological Society of London. Nos. 93 to 108.	The Society.
Transactions of the Royal Institute of British Architects. Vol. i., Pt. 2.	The Institute.
Historical Transactions of the Royal Society of Copenhagen. 6 Vols.	The Society.
Pilote Français comprenant les Côtes Septentrionales de France depuis Barfleur jusqu'a Dunkerque. Publié par ordre du Roi. Partie 5^{me}.	Par le Department de la Marine.
Ueber das farbige Licht der Doppelsterne und eineger anderer Gestirne des Himmels, von Christian Doppler.	The Author.
Comptes Rendus Hebdomadaires des Séances de l'Académie des Sciences. Tome xiv., Nos. 13-26, et Tome xv., Nos. 1-8.	The Academy.
A number of Mineral and Fossil Organic Specimens, from various localities.	Lord Greenock.
Specimens of Land and Fresh-water Shells, chiefly from the neighbourhood of Edinburgh.	
Specimens of Marine Shells, chiefly from the Firth of Forth.	
Specimens of Zoophytes, chiefly from the Firth of Forth.	John Stark, Esq.

December 19.

Astronomical Observations made with Ramsden's Zenith Sector, together with a Catalogue of the Stars which have been observed, and the amplitudes of the Celestial Arcs, deduced from the observations at the different stations. Published by order of the Board of Ordnance.	The Master-General, &c.

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Proceedings of the London Electrical Society. Part 6. Session 1842-43.	The Society.
Bulletin de l'Académie Royale des Sciences et Belles Lettres de Bruxelles. 1842. Nos. 3-9.	
Nouveaux Mémoires de l'Académie Royale des Sciences et Belles Lettres de Bruxelles. Tome xv.	
Annales de l'Observatoire Royal de Bruxelles. Publiées par le Directeur, A. Quetelet. Tome ii.	Royal Academy.
Annuaire Magnétique et Météorologique du Corps des Ingénieurs des Mines de Russie, ou Recueil d'Observations Magnétiques et Météorologiques faites dans l'étendue de l'Empire de Russie. Par A. T. Kupffer. Année 1840.	Par l'Administration Imperiale des Mines.
Astronomische Nachrichten. Nos. 447-461.	Prof. Schumacher.
Transactions of the Philosophical Society of Cambridge. Vol. vii., Part 3.	The Society.
Scheikundige Onderzoekingen, gedaan in het Laboratorium der Utretchtsche Hoogeschool. Stuk 4.	The Editors.
Specimens illustrative of Mr Stark's paper on the Food of the Herring and Salmon.	John Stark, Esq.
Specimens of the Woods of Ceylon (fifty different kinds.)	J. Anstruther, Esq.
Specimens of Minerals and Fossil Organic Remains from Malta, the Ionian Islands, and Ceylon.	Dr John Davy.
Geological Specimens from the Velay and Vivarais (Haute Loire and Ardêche.)	Professor Forbes.

January 9. 1843.

Maps of the Ordnance Survey of England and Wales. Nos. 80, 81, and 90.	Master-General of the Ordnance.
Maps of the Irish Ordnance Survey, containing the County of Clare. 77 sheets.	Lord-Lieutenant of Ireland.
Report made at the Annual Visitation of the Armagh Observatory. By the Rev. T. R. Robinson, D.D.	Governors of the Observatory.
Proceedings of the American Philosophical Society. Vol. ii., No. 23.	The Society.
Journal of the Asiatic Society of Bengal. Nos. 122 and 123.	Ditto.
Specimens of Fossil Organic Remains from East Kilbride and neighbourhood. Lanarkshire. Collected by the late Rev. David Ure, A.M.; and a number of them figured in his "History of Rutherglen and East Kilbride."	John Stark, Esq.
Tail of a Wild Elephant from Ceylon.	Rob. Bryson, Esq.
Specimens of Fossil Fishes from Syria.	Dr John Davy.

January 23.

Proceedings of the London Electrical Society. Part 7.	The Society.
De Fide Uranometriæ Bayeri Dissertatio Academica. Scripsit D. F. G. A. Argelander.	The Author.
Memoirs of the Royal Astronomical Society. Vol. xii.	The Society.
Philosophical Transactions of the Royal Society of London. 1842. Part 2.	Ditto.
Arsberattelser om nyare Zoologiska Arbeten och Upptackter. Afgifne för Aren 1837-40. Af C. J. Sundewall.	Royal Swedish Academy.
Kongl. Vetenskaps-Academiens Handlingar, för Aren 1839-40.	Ditto.
Arsberättelse om framstegen i Fysik och Kemi Afgiven den 31 Mars 1839 and 31 Mars 1840. Af Jac. Berzelius.	Ditto.
Ärsberättelse om Technologiens Framsteg, Afgiven den 31 Mars 1839 and 31 Mars 1840. Af G. E. Pasch.	Ditto.
Three Specimens of Salmon, showing the rapid growth (on descending to the sea) of the Smolt to the state of Grilse, and of the latter to the adult condition.	Mr Andw. Young, Sutherlandshire.

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February 6.	
Elements of Chemistry, including the Applications of the Science to the Arts. By Professor Graham, University College, London. Part 6.	The Author.
Maps of the Irish Ordnance Survey, containing the County of Waterford, in 42 sheets.	His Excellency the Lord Lieutenant.
Specimens of Volcanic Rocks from Vesuvius, and Minerals from Derbyshire.	Sir T. M. Brisbane, Bart
Specimens of Fossil Shells from Grignon, collected by Dr Stark in Oct. 1833.	John Stark, Esq.
February 20.	
Voyage dans la Russie Méridionale et la Crimée, par M. Anatole de Demidoff. Tome iv. avec un Atlas des Planches.	The Author.
Bulletin de la Société Géologique de France, from 15th March to 9th September 1841.	The Society.
Proceedings of the Glasgow Philosophical Society, 1841-42.	The Society.
March 6.	
Monthly Notices of the Astronomical Society of London, containing Abstracts of Papers and Reports of the Proceedings of the Society. Vols. i. ii. iii. iv. and Vol. v. Nos. 1–27.	
Memoirs of the Royal Astronomical Society. Vol. xiv.	The Society.
Examination Papers of the several Faculties in the University of London, for 1842.	Council of the University.
Mémoire sur la Chaleur des Gas Permanens, par Jean Plana, Astronome Royal, et Directeur de l'Observatoire de Turin.	The Author.
Journal of the Asiatic Society of Bengal. Nos. 124 and 125, for 1842.	The Society.
The Quarterly Journal of Agriculture, and the Prize Essays and Transactions of the Highland and Agricultural Society of Scotland. No. 60, for March 1843.	
A Specimen of a Vegetable Impression from Burdie House.	D. Balfour, Esq. younger of Trenaby.
March 20.	
Archives du Museum d'Histoire Naturelle, publiées par les Professeurs-Administrateurs de cet Etablissement. Tom ii. Liv. 3.	The Editors.
Comptes Rendus Hebdomadaires des Séances de l'Académie des Sciences. Tome xv. Nos 9–26, et Tome xvi. Nos. 1–7.	The Academy.
Monthly Notices of the Astronomical Society of London. Vol. v. No. 28.	The Society.
Specimen de l'Imprimerie de Bachelier, Rue de Jardinet.	M. Bachelier.
March 27.	
Bulletin des Séances de la Société Vaudoise des Sciences Naturelles. Nos. 1 and 4.	The Society.
Essai Historique sur les Phénomènes et les Doctrines de l'Electro-Chimie. Par Elie Francois Wartmann.	The Author.
Comptes Rendus Hebdomadaires des Séances de l'Académie des Sciences. Tome xvi. Nos. 8 and 9.	The Society.
On the Transparency of the Atmosphere and the Law of Extinction of the Solar Rays in Passing through it. By James D. Forbes, F.R.S., &c.	The Author.

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April 3.	
Scheikundige Onderzoekingen, gedaan in het Laboratorium, der Utrechtsche Hoogeschool. St. 5.	The Author.
Natuurkundige Verhandelingen van de Hollandsche Maatschappij der Wettenchappen te Haarlem. Deel 2.	The Society.
Series of Specimens of the different Rock Formations. (150 specimens.)	Lord Greenock.
Specimens connected with Mr Shaw's paper on the Development and Growth of the Sea-Trout of the Solway.	Mr John Shaw.
April 17.	
The Quarterly Journal of Meteorology and Physical Science. Edited by J. W. G. Gutch, M.R.C.S. No. 6. for April 1843.	The Editor.
Proceedings of the London Electrical Society. Part 8.	The Society.
Annual Report of the Council of the Yorkshire Philosophical Society, for 1842.	The Society.
Elements of Agricultural Chemistry. By Sir Humphrey Davy, Bart. (Sixth Edition.)	Dr John Davy.
Proceedings of the Royal Astronomical Society. Vol. v. No. 29.	The Society.
Proceedings of the Royal Society. Nos. 55 and 56.	
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An Introductory Lecture on Botany, considered as a Science, and as a Branch of Medical Education. By Edward Forbes, Professor of Botany in King's College, London.	The Author.
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Maps of the Irish Ordnance Survey, containing the county of Tipperary, in 93 sheets.	Lord Lieutenant of Ireland.
Transactions of the Royal Irish Academy. Vol. xix., Part 2.	The Academy.

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Elements of Agricultural Chemistry and Geology. By James F. W. Johnston, M.A., F.R.SS.L. & E., &c.	The Author.
Transactions of the Institution of Civil Engineers. Vol. iii., Parts 2, 3, 4, and 5.	
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Transactions of the Society instituted at London for the encouragement of Arts, Manufactures, and Commerce. Vol. liv.	The Society.
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Twenty-third Report of the Council of the Leeds Philosophical and Literary Society. 1842-43.	The Society.
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Astronomical Observations made at the Radcliffe Observatory, Oxford, in 1840 and 1841. 2 vols. By Manuel J. Johnson.	The Radcliffe Trs.
Maps of the Irish Ordnance Survey, containing the county of Dublin, in 30 sheets.	The Lord Lieutenant.
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Literarische Sympathien oder industrielle Buchmacherei: Ein Beitrag zur Geschichte der neueren Englischen Lexicographie, von Dr J. G. Flugel.	The Author.
Fifty-Fifth Annual Report of the Regents of the University of the State of New York.	Dr Christison.
Journal of the Asiatic Society of Bengal. Nos. 136, 137, 138, and 139.	The Society.
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